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EDITOR'S NOTE

In 1925, when we founded The Library of Medical History (see Book Number of Medical Life, August 1925), the first manuscript that was sent to press represented John C. Hemmeter's lifework in medical historiography—*Master Minds in Medicine.* We have since explained (see Second Book Number of Medical Life, May 1926), that because of the magnitude of the work, it was slower in reaching book-form than several of our Mss. which went to press subsequently. Accordingly, while *Master Minds in Medicine* was in the making, there appeared, bearing the imprint of Medical Life Press, Charles Beneulyn Johnson's *Sixty Years in Medical Harness;* Karl Sudhoff's first book in English, *Essays in the History of Medicine,* edited by Fielding H. Garrison; *Reminiscences,* by George Henry Fox; and the short *History of Cardiology,* by Louis Faugères Bishop and John Neilson, Jr.

Prior to publication, over 200 advance orders were received for the Hemmeter book, and it should be pointed out that aside from a single prospectus, and announcements in Medical Life and in The Scientific Monthly, no advertising methods were employed. The list of advance subscribers is a remarkable one, since it includes several of the foremost names in contemporary medicine, and likewise the names of internes and even undergraduates. With the exception of orders from booksellers, the names of all advance subscribers will be found in the pages that follow, and each will receive an autographed copy.

In commemoration of the completion of *Master Minds in Medicine,* it has been decided to devote Medical Life for April—the author's birth-month—to a discussion of John Conrad Hemmeter as physician, philosopher, professor, scientist, musician, and historiographer. The scholarly names on the Contributing and Advisory Committee are sufficient assurance that the Hemmeter Festschrift Number of Medical Life will be both interesting and informative. It will serve, in a manner, as a biographic supplement to *Master Minds in Medicine.*

VICTOR ROBINSON.

January 1927.

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MASTER MINDS IN MEDICINE

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* MEDICAL LIFE: monthly journal of medical history. Edited by Victor Robinson.

* Indicates the volumes already published; the remainder are in press or in preparation.

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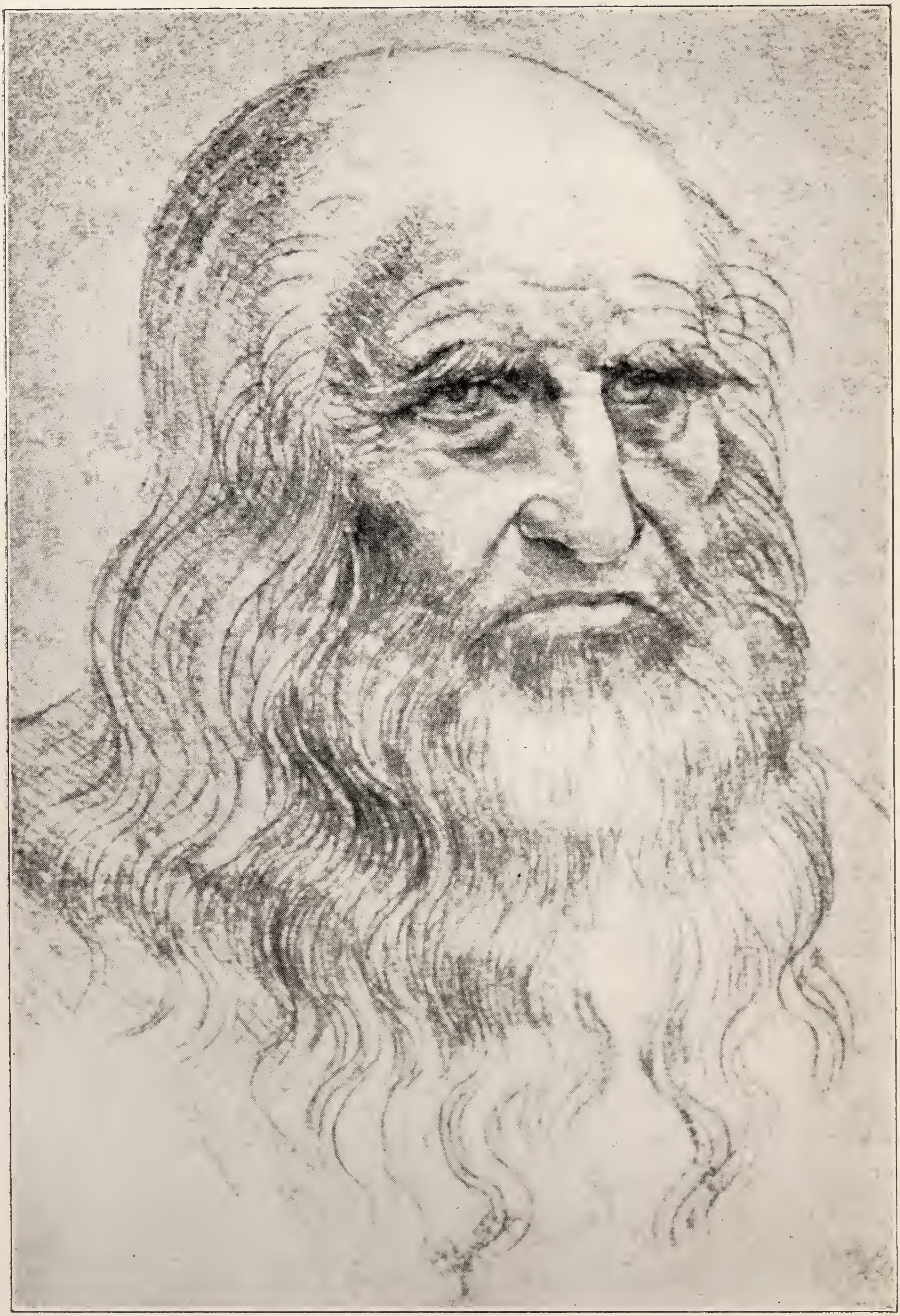

Leonardo da Vinci

A Crayon Portrait by Himself (Royal Library of Turin). Another Self-Portrait of Leonardo Appears on Page 500.

MASTER MINDS IN MEDICINE

An Analysis of Human Genius as the Instrument in the
Evolution of Great Constructive Ideas in the
History of Medicine

Together with
A System of Historic Methodology

BY

JOHN C. HEMMETER, M.D., PH.D., SC.D., LL.D.

WITH AN INTRODUCTION BY

KARL SUDHOFF, M.D.

Professor of the History of Medicine, University of Leipzig, 1895-1924

NEW YORK
MEDICAL LIFE PRESS
1927

BA /HCM

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inspiring teachers, and above all, great lovers of Mankind,

CHARLES HORACE MAYO

AND

WILLIAM JAMES MAYO

of

Rochester, Minnesota

this volume is dedicated to you by the Author
as a contemporaneous token of a loyalty that
History will sustain and future generations will approve.

PREFACE

The reciprocal relation between the great master minds which the human race has produced and Society at large, has not received the concentrated study which this great problem deserves. The following chapters give evidence of the study of one or another of these two elements, either the great individual or the mass of humanity, exclusively. But the interaction, the interpenetration, and the reciprocal influences which these two factors exert upon each other has not been treated in any special work that I know of.

At the present time we are not yet able to exactly define these relations because they have not yet been studied sufficiently. The first thing incumbent upon the Sociologist is to establish the fact in all its bearings that there is such a relation between Society and the "great man." The next step is to ascertain the geographic, economic, political, climatologic, psychologic, etc., prevailing conditions under which, according to history, Society produced the most influential great individuals. Then we must learn what forces have been at work which impels Society to arrest and even destroy the very highest fruits of its existence. The great mind is an effect of Society; to what an extent the reverse is true, history has as yet only incompletely revealed.

It is true that Society itself is an effect, but the great personality can neither produce nor prevent this effect. It can only modify it and give direction to it after it has been brought into existence. It is perfectly hopeless to contrast Society and the great individual as antagonistic agents. In fact this way of looking upon their mutual relations has already produced a great deal of harm. They mutually interact with and interpenetrate each other and each is incomplete without the other. The following pages demonstrate only too clearly that great thinkers are not omnipotent. They record even the martyrdom, self-sacrifice of these master minds and their violent death at the hands of the masses.

The great man is well aware that, though intellectually he exceeds the common measure, the effects of his influence never reach very far as long as the great man himself lives. In fact the tests of the great constructive ideas of the eminent man is the duration of life of this idea and its expansion after the death of the great mind that has produced and disseminated it. All so-called great men to whose life work no origination of great constructive ideas can be attached and to whom no single constructive thought can be assigned are really not great men. They may be big men, very deserving men, but they are not great.

A great man can not arrest the progress of events by his interference or only to a very slight degree. He can only give direction to it and modify it. Personally I gain the impression, in spite of what may be said in the following chapters, particularly chapter VI, that the best that can be conceived concerning master minds is not their work in inventing and discovering new things, but their deep and broad understanding insight and comprehension of the drift of human affairs in any particular domain. In this light the explanation of the might of effective minds working under the most favorable conditions, is that master minds are neither a cause nor an effect, nor a goal. They are a transitory means by which Society unconsciously seeks to orientate itself during the historic process. They will always be credited with the extraordinary merit that they direct general sympathy toward common human interests and because their labors bear the unmistakable impress of impersonal, hard and earnest exertion.

Up to the last century the great man was the only incentive toward the human proclivity to collect the relics, remnants and remembrances of the past. The effectiveness of the work of the great minds is usually arrested by the resistance of mass groups who are either not yet fully prepared to understand the new course which the great man wishes to steer or they instinctively feel that their own personal interests may be endangered by any innovation. To most of the mass a new thing is a dangerous thing. The mass then forms so called "Political Centers" which have their own passions, and their own spirit. A political center means a unification of the totality of expression of the human will as it permeates large groups.

It can be seen from the newest work on the subject of history and sociology by Harry Elmer Barnes, entitled *The New History and the Social Studies,* that the mass, or the large social group, possesses an individuality also. It is a heterogeneous individuality in its contrast to the homogeneous individuality of the single great thinkers. Dr. Barnes does not express that in the identical words that I do, but one can gather as much from his chapters III and V, and also from chapter IX, which deals with a genetic view of the theory of conduct. The heterogeneous mass individuality is the result of combining all conceptions, and all directions which are furnished by the mass. The people taken as a totality, in its manifold functions, is an actual being just as much so as if it could be condensed to a single body. It sometimes reacts to its own inner impulses in a feeble, and at other times in an excessive manner. At times even the reaction bears the impress of the pathological and results occur that are expected by no one, and which no one will confess to have originated. The reactions of the mass are stronger than those of the individual and are backed up by a greater psychic momentum, but it is of lesser enduring value than the effects of the individual, because the effect and the reaction of the mass depend upon impulse more than upon reason, more upon instinct than upon a clarified will.

I think it can be demonstrated that the majority of historic reactions of the masses are negative rather than positive—that is, they seek to frustrate and prevent rather than construct and edify. This very historic feature should indicate clearly enough that the masses are in reality not capable of originating great constructive ideas, and this is also one of the reasons why a people, in fact all peoples, are incessantly accusing those that govern them for all that they have to suffer, and this in my opinion is due to the mass consciousness which realizes that as a multiple, complex organism it suffers from an inner organic weakness and that it is in some way conscious of the defects and incompleteness of the mass-personality. For this very reason again the mass is continually producing leaders almost as a tree produces blossoms, fruits and seeds; but a leader can only be the man who penetrates into the manifold currents of the will of the mass, understands them, clarifies and unifies them, brings them together and gives them a new direction into what he recognizes is the better destiny.

It is evident from the introduction to this book by Professor Karl Sudhoff that the mass as such with its complex and unwieldy body, and its incapacity for realizing and clarifying its own under-currents of impulses, passions, wishes and feelings, is incapable of crystallizing out great constructive ideas. On the other hand we can see from Chapter IV, as well as chapter VI, that the great leaders and master minds almost uniformly disclaim in their amazing spirit of altruism and humility that they themselves have originated the great constructive ideas. Note what Mozart describes as the origin of his great musical compositions. Observe what Helmholtz and Ostwald and Goethe declare to be the source and origin of their great discoveries and works. Read what Oliver Wendell Holmes says on this subject. Here we stand before a critical insight and a most amazing riddle. The mass can not originate a great constructive idea and the great man denies he can originate a great constructive idea. Where, then does the great constructive idea and its resultant work come from? The solution of this question, if it ever will be possible, leads the historic investigator far beyond any possibilities of historic research. We are transplanted in our minds far beyond the horizon which any historian has ever permitted us to gaze upon. The reciprocal relations and interactions of great individuals and the great social mass open up the view into a domain of thought, for which at the present time we have no adequate thought instrumentarium. We, therefore, can not as yet investigate it, but beyond the limits of present human thought there is a region of abysmal profundity which we can only have faith in by the occasional wave of influence that reaches our thought world. "Such knowledge is too wonderful for me; it is high, I can not attain to it." (Psalm 139:6.)

Stimulated by the teaching of Carl Lamprecht in his work *Was ist Geschichte* (chapter I), and by James Harvey Robinson, Harry E. Barnes in his *New History and the Social Studies* discloses two very old infirmities of history writing which are still manifest at the present day. He says the lure of the episodical, the dramatic, the biographical and the political history, produced by the inertia in the heritage from older historical interests and concepts and the development of nationalism in the nineteenth century, has proved too strong for the majority of historians, and has seduced them into a grotesque concern with the irrelevant and into a sad ignoring of the vital elements in historical development. It has not been sufficiently emphasized that there are two chief types of superficiality and inaccuracy in history, the first being that which leads to statements made without a sufficiently thorough and dispassionate study of the sources of information; and the second and equally serious variety being that which leads to a failure to concentrate historical investigation upon those topics which seem likely to reveal most clearly the nature and causes of human progress. It is a strange but interesting fact that those who have been most cautious in avoiding the first type of superficiality have been most frequently and fatally guilty of the latter. It would seem that the chief cause of this has been the unfortunate tendency to make states, politiical institutions and political figures the chief center of orientation and point of departure for historical study and exposition.

In the special History of Medicine these two types of deficiency are very evident and it is no disregard for the advice of very close and esteemed friends, all lovers of history, if I have labored to free myself of these superficialities. It was not easy to resist the temptation to insert episodes and anecdotes about master minds. The rules laid down in chapters I and II apply to these features as well as to all others belonging to the great life. And when they are investigated in this methodical manner, the great majority of episodes, anecdotes, etc., when traced to their sources, are ill-founded and lose their significance as historic factors.

History, historic research or historic science, and historiography are three entirely different things. Not one of these words or designations says the same thing as any one of the others and it would in my conviction lead to confusion if any of these words were misconstrued so as to cover the meaning of any of the others. In a written system that endeavors to reach precision and clearness (as far as they are humanly attainable) concerning a process that pervades all nature and which may be classed as a transcendental process and concerning the methods and the technique which man has devised and invented, to at least partly understand, investigate and record those small constituent elements of this historic process that are accessible to his intellect, these terms command separate and distinct consideration, no matter what common or popular usage would prefer.

It has always been the tendency of common usage to contract, abbreviate, and abstract words and meaning, and the printers, typists, and popular orators have connived at this pernicious practice under the delusion that it saves time and work. As a matter of fact it loses time and augments work; for whenever two different concepts are "boiled down" and forced into a single word, the further penetration into the exact thought content that may be in the mind of the one who uses such a word, compels another expression, generally by circumlocution, before the listener or reader understands precisely what the speaker or writer had in his mind.

Scientific thinkers can not be too punctilious in the exact use of words, for words are the gradual resultant of an evolution, a slow adaptation of "sounds and signs to signify processes and things." And as such, words have a history which proves that all modern languages owe half of the words in them not to origination by the people who speak and write those languages but to the introduction of the languages of people that either conquered them or by virtue of a superior literature became their intellectual masters.

English is a Teutonic language, come down to us from the Anglo-Saxons, but approximately one-half of its vocabulary is borrowed from the Latin and French. But when one reflects on how much of the French is borrowed from the Latin and how much of the Latin from Greek it must be evident that the scientific thinker can not avoid going to the Greek and Latin when the exact meaning of words comes into question. Furthermore it must become evident that there is no language used by any contemporaneous people that could be designated as autochthonal, i. e., native to the soil, indigenous. This fact brings the further insight that the habitual users of any modern language have no exclusive monopoly of its correct employment. Languages are so interpenetrating that the best usage is founded on the broadest knowledge of language in general. This is exactly what common or popular usage is destroying and what scientific thinkers must strenuously uphold.

As a result of many years of historic study, especially if an author has personally lived through certain phases of development of science or has actually participated in and contributed to these developments or if the student of history has himself lived through definite phases of development of the subject which he is studying and recording, it is inevitable that individual perspectives will form in his mind. Personal experience brings personal viewpoints, and these unconsciously lead to changes in the manner in which an author conceives the meaning of a word. I mean by this to suggest that a strictly individual new interpretation of a word by an author should not be met with immediate destructive criticism provided the work of the same author gives indication of being the result of consistent and honest labor and investigation of the sources.

An exact definition of the word "history" is difficult because we are here dealing with a process which in its essence is beyond human comprehension. It is transcendental in the sense of Kant. In the first six chapters of this work and in the final chapter the meanings of this word given to it by numerous great scholars of history are stated. I would prefer, if the words "historic process" had been employed everywhere in place of the simple word "history" for I have been astonished to find how frequently even the lovers of historic studies do not realize that history is a process. If we meditate still further that this process is only to a limited degree comprehensible by the instrument of human reason, we will become convinced that it is an error to say that "men make history." I have committed this error repeatedly myself, because I was speaking in the language of great historic research workers. But I was aware that even they did not exactly mean what these words indicate. By saying that "men make history" is not meant that they are the actual creators of history. We mean that as far as human discernment is possible men are the instruments through which the historic process works and expresses itself in human affairs.

This process itself, its causality, is up to the present time transcendental but that part of it which concerns the activities of human beings as social entities is the proper domain for study and scientific research by scholars in this specialty. Historic science then is the science which deals with the activities of human beings as social entities and seeks to interpret them with regard to their inner connection and with regard to cause and effect as far as possible. Historiography is the art of recording the results of these studies. It seeks to lay down a set of rules, an educational discipline for the orderly recording of the facts and truths concerning the manner in which the historic process has manifested itself among human beings. To say that both this process and the "technique" of writing and recording its effects could be expressed by one and the same term would be synonymous with stating that a process could be identified with its effects. As a parallel example I might remark that Astronomy, the science that deals with all of the bodies that fill space, in short, with the universe, could be covered by the concept contained in the word Astro-physics. What is going on in the universe is also a process that has attracted the minds of the greatest thinkers of our race ever since men could think. Astrophysics is a method for studying the universe. In an analogous manner history is a process and Historiography is a discipline or method for describing and studying it.

In the history of medicine everything can be designated as historical and can be treated historically which has become fixed in the memory of human beings as a fact or collection of facts of the past, and the principal criterion which looms up to the student in this endeavor is that of change. When anything gives evidence of undergoing a change we can discern in it a manifestation of the historic process. A further criterion or characteristic which is noticeable after prolonged study is that these changes never repeat themselves, that is they never recur in the identical order of events or the identical form in which they occurred before. It is therefore an error to speak of the repetition of history.

All historic presentations, even the most comprehensive, are never complete. They are nothing more than intentional sections from an unlimited domain in which the historic process occurs. Therefore what appears at times as an exhaustive and most comprehensive historical research is in reality only a sector of this process which memory has been able to retain and recall.

It has been observed that the various systems of the history of medicine do not mutually supplement each other as in other sciences but that in reality they are nothing but monographs which at best can present only the author's individual capacity for tracing the thread of a historic process in medicine through the labyrinthine maze of concomitant political, religious, climatological, geographic, and other events, and influences. All this makes it more important that the writing of medical history should be based upon some fundamental canon or rule.

Returning once more to the term "Historiography" as a training for the collection, recording, and elucidation of historical remembrances, after all rules should be agreed upon, there would still remain a "something" that is needed for the art of history writing, which is undefinable.

Mommsen has designated it as the divinatory or divining certainty of judgment which characterizes the eminent historiographer and which, he explains, is in nine cases out of ten nothing but the application of a subconscious doctrine to complicated problems. When I say that this divining discernment is an important factor in history writing, I do not intend by any means to minimize the utility of the theoretic principles of the historic methods as a means of inducing those who desire, to write medical history in a manner that will vouchsafe an effective contribution. The thoughtful student will agree that no art can be satisfactorily executed without this divining discernment. As Eucken has expressed it, "the method which would produce results without the use of intellect has still to be discovered." It may, I hope, not be considered pedantic to sum up in a preface what in reality should have been stated in the chapters on the methods of historic technique. A preface is really written after a book is completed and therefore affords a final opportunity to present in a new perspective the salient lessons of the work.

That which makes clearness concerning the methods of Historiography so very important are the misunderstandings in much of medical history writing which are caused by the authors confounding the material of their own consciousness with the objective material of proof. To show how even very eminent masters of history writing may drift into a most casual use of method, I will presently quote from the work of Langlois and Seignobos, *Introduction aux-études historiques,* Paris, 1898. It is true of medical historiography as well as of general historiography that many writers on the historic method, occupy the untenable standpoint that scientific history writing is founded on nothing else than authentic documents and sources that are scattered throughout the archives and libraries of many countries. To this they sometimes add a complete testing of sources that have already received the attention of some competent investigator. In other words the authors I have mentioned attempt to establish an axiom "the writing of history is constructed from documents" and historic critique serves to vindicate the certitude of the isolated facts that have been harvested out of these documents. In the history of medicine we would thus arrive at a collection of conceptions of events, transactions, and physical objects that concern either individuals, communities, things, processes, etc. This mental aggregate of motives and conceptions the historian must transform into properly classified and divided groups, according to the analogy stipulated by his own store of observation and knowledge. Seignobos and Langlois are masters of historic technique and yet it seems to me that their sole reliance upon technique alone and not even mentioning the gift of divining, which is referred to in the preceding quotation from Eucken, has given their doctrine a weakness which calls for correction. Seignobos introduces a series of historic operations which begin with the discovery of a document and conclude with an end-formula. In the *"Operations synthetiques necessaires pour elever l'edifice"* Seignobos insists on four categories:

1. On reading the document, let the historian form his own conception.
2. One must construe an imaginary or theoretic model according to which the totality of what one has read or learned may be grouped as if it were transferred into the present.
3. Any remaining gaps or vacant periods must be supplemented by logical reasoning.
4. The entire mass of the facts thus accumulated must be condensed to general formulæ and relations.

If these rules are to serve as a heuristic discipline for teaching correct history writing, it must be immediately apparent that they are difficult to follow out. A beginner can not write history according to this outline of Langlois and Seignobos. To use such a system effectually the aspirant to historic research would already have to be a competent historian to start with. As a matter of fact Seignobos adds a sentence which disqualifies his own system as an effective way of teaching history writing. And singularly enough this eminent and brilliant French historic scholar in this very disqualification of his own manner of writing history expresses such high praise of three great German historians that I can not omit quoting his remark in this connection. He says, "Such learned historic specialists as Curtius, Mommsen, and Lamprecht in reality work in a different manner. As soon as they write such comprehensive presentations, they voluntarily give themselves over to their fantasy." This feature of Historiography the author has already discussed under its proper heading in Chapters I and II.

But in justice to Seignobos I should add that he is not satisfied with method alone or fantasy alone for he is no admirer of such careful historic constructionists as Augustine Thierry, Carlyle, and Michelet because they do not sufficiently attend to the critique of the documents and building stones of history which has to precede all synthetic work. It is true Seignobos emphasizes the following: "A historiographer must never work from second hand but one is compelled by necessity to do so occasionally because the documentary materials on certain subjects are too abundant to control and to read. But one says nothing about it for fear of causing a scandal." As a final good advice to escape the excessive labor of reading and studying too great an accumulation of documentary material on the one hand and of indulging too freely in fantasy and intuition on the other he recommends a procedure for saving time and labor which I could not personally advise, even in medical history writing. It is the following: instead of reading all the documents that really should serve as a foundation, the research worker is to read only the newest work of other historians on the same subject that he is investigating. Herein lies the danger that we may be driven by a natural instinct to accept first of all the conclusions of these historians as established truths. This however should be avoided by continued analysis of the facts, evidences, and fragments of documents which these authors present. It might be objected that thereby one has to do the work of these authors over again, which is true. But one does this work in much less time since the collecting and sifting of materials causes so large an expenditure of time and energy.

There is something about the recommendations of Seignobos and Langlois that smacks of ultrapractical expediency and is not indicative of solid work. I know I am expressing a very severe criticism which I am reluctant to do because I greatly admire these gifted authors. I feel, however that it is unavoidable in view of the fact that the technique proposed would strike at the very root of a method suitable for history writing, since usable material for the writing of history are remembrances, recollections, and images resulting from contemplation. Any justifiable observation that can be made on the existing stock of historic things on earth may serve as evidence to decide debatable points or to fill out vacancies. All other sciences may utilize the same proof-material in common with historic sciences because all sciences are justified to subject such material to methodical elaboration for their own special needs. The important point never to be forgotten is that true history writing has *its starting point not from documents but from an intellectual process, namely, the intense interest in the changes observable in conditions, things, conduct of men, events,* etc., and the desire to explain these changes in a manner free of contradiction, and to correlate the facts and truths, eventually reached in this manner in our consciousness, with reality. Eventually there comes a time in this work when the historic research seems to be at an end but its true goal is not reached until it has been proven that the solution or conclusion which has been arrived at is more justified and better founded than any other. It accomplishes this in two ways, first, by demonstrating that the conclusions reached are more in agreement than all of our other experiences, and secondly, because the material for proof can be better inserted and fitted into the whole genetic relation of the subject that is investigated.

Events are changes. All observed changes are perceived as such because they do not occur regularly. If some occurrences in our environment in the world of our experience transpired regularly, that is to say if they recurred according to some plan, they could no longer be interpreted as changes. All thinking persons possess a stock of assimilated experiences. The effort to harmonize the changes in environment which I have designated as the historic process with this stock of experiences is the main incentive to the writing of history. It may be possible in the future for the forces that are effective in the historic process to become to a limited degree intelligible to us depending on the status of our intellectual endowment. At present historic research carried on according to the most approved methods is the only means by which we may hope to eventually lift the veil that conceals the forces active in the historic process. The solution of historic problems by these methods could be postulated as a specifically human characteristic. As Goethe says, "History just as nature and as everything that is profound, be it past, present, or future, exhibits the following feature, namely, the further one penetrates into them the more difficult are the problems that appear. Those persons that are not afraid of them but boldly continue to grapple with them as they progress further and further, will feel themselves more edified, more highly educated, contented, and comfortable."

When I think of the many friends in this country and Europe who aided me with their advice and experience in reading the various chapters after they were typewritten, I am conscious, not only of a deep gratitude but of a very real humility in view of the fact that I owe so much of what is good in this work to others. I do not only refer to those great minds that the text treats of but what I owe to my contemporaneous friends who are lovers of history. First among these are my friends: Professor H. Norman Gardiner of Northampton, Mass., who read the chapters on *Ideas as Factors in Medical History,* the chapters on Leonardo da Vinci, and of whose broad experience and profound knowledge in philosophy as well as in psychology I could always avail myself and depend upon; and Professor Walter T. Marvin of Rutgers College, Brunswick, N. J. Next I have to express a debt of gratitude to Professor Harry Elmer Barnes, also of Smith College, Northhampton, Mass., but in this case the direction given to my thought was received mostly from his published works, especially his book on *The New History and Social Studies.* Professor Frederick H. Wilkens of the Department of Philology of New York University, a companion of my boyhood days and an eminent scholar, translated the introduction by Sudhoff, gave the entire literature for the article on *Goethe as a Naturalist,* and was generally helpful throughout the work. John Martin Vincent, Professor of History at the Johns Hopkins University revised Articles I and II on the Methods in Medical History Writing. Katharine Jean Gallagher, Professor of History, Goucher College, Baltimore, Md., revised Chapters IV and V; her special influence is noticeable in these chapters. Herman Ebeling, Professor of Greek, Goucher College, went through the first five chapters to assure that the Greek quotations were in the best possible form. Professor Arthur Oehm, like Dr. Frederick H. Wilkens, a friend since my boyhood days has given the eminent service of correcting the entire printed proofs. Professor Oehm's exceptional versatility and his taste for form, style, and diction, and especially his rare conscientiousness were of such aid as can not be overestimated. Monsignor Leo. P. Manzetti, Professor at St. Mary's Seminary, Baltimore, Md., and Director of Music there, was a constant aid in translating the difficult Italian language of the period of Leonardo da Vinci. Besides these I should merely mention here Karl Sudhoff, Professor of Medical History at the University of Leipzig; Max Neuburger, Professor of Medical History at University of Vienna; Charles Richet, Professor of Physiology at the University of Paris; Professor H. J. Hamburger, the physiologist at the University of Groningen, Holland; Herbert S. Jennings, Professor of Biology at the Johns Hopkins University who revised Chapters XXX and XXXII. All of these friends and particularly Professor Charles Singer of Oxford and London, benefited me by an exchange of correspondence whenever I required advice on a particular subject. But my friends who are neighbors at Woods Hole, Mass., which is a great center of Biologic Science, were a constant source of instruction and information whenever I needed it. They did not think at the time that they were coöperating in an historic work and therefore I shall not mention their names specifically, because this would make it appear that they took a more important part in the composition of this work than their own modesty would permit them to concede.*

It is always a great help for an author to have at his right hand secretaries and stenographers, who not only are ladies of skill and experience, but who gradually acquire a knowledge of the author's special phraseology and method of dictation, and who can promptly find a book or manuscript in his extensive library, or read the exact definition of a doubtful word in any modern language. Such clerical aid is the *sine qua non* in the first typing of an author's work and it is a very pleasant duty to thank Miss Anita Faatz of Goucher College, Miss M. Ellen Moore and Miss Effie Abbott in this connection. The name of Miss Gertrude Walton, of Goucher College, Baltimore, should be added for the final proofreading.

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* An index adds greatly to the attractiveness and usefulness of any book. This feature of the present work was executed by Dr. A. K. Bond, of Baltimore, whose lively interest and exacting care are herewith gratefully acknowledged.

But there are other things in addition to all of this technical work and they are the never ceasing watchfulness and punctiliousness about the library and study of an author, the familiarity with the location of sources, and the orderly arrangement of proofs and manuscripts. For these things and a sustained care of his own psychic energy, the author can not express sufficient appreciation and thanks to Mrs. Helene E. Hemmeter, the good wife and companion.

Lovers of medical history finding a System of Historic Methods preceding a series of historic biographic studies may receive the impression that the medical psychographies are intended as illustrations of the preceding Methodology. This is not true of all of them because most of them were written long before the Methodology was conceived which is here presented because as yet no system of historic method as applied to medicine has been published in English.

Nor would it be true to the intentions of the author to look upon the lives and work of eminent medical thinkers as biographies simply. It must be evident that these presentations go beyond such a purpose.

They are meant to present great constructive ideas as active living forces and to demonstrate that these forces operate through master minds which are the instruments of those forces.

The separate chapters can be made to serve as reference works for the subjects, men and periods with which they deal, but their real purpose can only be appreciated when taken together, as a synoptic whole.

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INTRODUCTION

My friend, Hemmeter, requests me to contribute a few remarks as an introduction to his *Master Minds in Medicine.* I am delighted to accede to his request. It appears that I am not expected to define my position as to details of the book, which I know only from its Table of Contents and reprints. These, however, raise great expectations.

I am acquainted, to be sure, with a number of biographic historic studies from the pen of Hemmeter which are republished in the book. I know that they deserve a high degree of praise and that each has its special historic merit, and are referred to and quoted in my *Kurzes Handbuch der Geschichte der Medizin,* on pages 7, 235, 306, 307 and 386. The whole collection will be eloquent testimony to the wide range of Hemmeter's intellectual interests and his unusual psychic receptivity. For he has, like so many other eminent physicians, stood for a long time in the closest of relations with the most spiritual of all arts, as an artist and composer. Euterpe herself, the Muse of Instrumentation, has impressed upon his brow her kiss, in recognition of his creative musicianship.

But Hemmeter has stood in close spiritual communion with Klio, the Muse of History, as well. Far be it from me, because I happen to be her officially approved servant in the domain of Medicine, to look askance at his friendship with my austere mistress. I do not wish to be considered an ossified and pedantic specialist. For did I not myself, for decades, serve this Muse in voluntary but ardent devotion until I was chosen to become her slave by official appointment? And even now, although I have grown gray-haired in her service, I do not possess the conviction that my work has become essentially better or more valuable simply because I came to be the officially approved representative of the History of Medicine.

The highest contribution leading toward success in this sphere of effort is the inborn gift residing in the individuality of the historical research scholar which must be developed and brought to light. More than one road leads to the Temple of Klio; there is more than one way to serve her. The first part of Hemmeter's book shows how scrupulously careful he is in the choice of his historic equipment and in the general preparation for his task as a historian by employing the most exact critical methods.

The medical man and the natural scientist of today, more particularly the biologist in every process of thinking, in all research, is under the influence of the evolutionary idea—that is History. And if we make clear to ourselves that biologic processes can only be grasped adequately in their evolution, a slight deviation from the narrow path of the specialist's method of investigation will lead us to recognize that science itself, is a living entity of an intellectual nature and has its life in evolution. Thus it comes about that the most approved natural scientist of today brings with him a considerable mental equipment for historic contemplation and perception of his subject, his special discipline; in truth this has arisen out of his work of investigation itself.

In order to be able to enter intelligently into the inherent life of science, one must be in constant contact with the present status of one's speciality, whatever it may be, down to its latest developments. The scientist of today possesses qualifications for the understanding of the historic past of his specialty and in consequence, of all earlier developments of the same; this the scientist or physician who is trained only historically has to obtain by earnest application and laborious study while the contemporaneous scientist almost inductively, with unerring precision, grasps the truth. It happens that the historian who is not likewise a scientific specialist at times misses the mark.

Compare for instance Hirsch's *History of Ophthalmology* (1877)—and Hirsch was undoubtedly one of the foremost medical historians of Germany in the 19th century—with the earliest volumes of Julius Hirschberg's standard *Geschichte der Augenheilkunde seit 1899* (*History of Ophthalmology since 1899*). Julius Hirschberg was likewise an historian to the manner born, which statement brings us back to our individual postulate above. But the command Hirschberg had of all phases of his specialty places upon his work the stamp of supreme excellence; he happened to be not only an historian but an ophthalmologist of the highest rank.

If I am to select another work that gives evidence of a specialist being a master historian of his subject, I might mention the splendid *Lectures on the History of Physiology* (1901) by Sir Michael Foster, professor of physiology at Cambridge University. To be sure, mere special knowledge is not sufficient; there must be a special historical grasp in order that the results may be perfectly satisfactory; a certain modicum of training for such work will prove a welcome help. Not every specialist, even though he feels his interest incline towards the history of his specialty, is necessarily qualified as an historian. Different degrees of qualification are possible as becomes apparent on examining Volumes II and III of Puschmann's great *Hand Book of the History of Medicine* (1903, 1905) in which specialists have presented the history of their specialties.

But such historical treatment of various methods, by those, who are outsiders, from the point of view of the historian, is not the only way in which a medical man who is not exclusively an historian of medicine may advance the history of his science and particularly develop the historical sense among the medical profession. For this also we can find striking examples, how eminent men of medicine, of the highest intellectual qualifications and comprehensive general education, endowed with the gift of plastic presentation and possessing the historical sense were unable to produce commanding and impressive works in the domain of medical history. It will be sufficient to recall the two great clinicians of England and America, Sir Clifford Allbutt, and Sir William Osler, whose principal historical works excel in plastic vigor and depth the work of most professional historians and who in their influence on the medical world, can scarcely be rivaled by the efforts of a professional historian.

Dr. John C. Hemmeter likewise possesses the gift of grasping the historical problems of medicine, of comprehending and developing them; of doing justice to the historical representations of science and medicine by a sympathetic understanding; of penetrating their characteristics psychologically and of transmitting to the outside world his understanding of them; of bringing them home to the attentive reader by an agreeable presentation; of revealing as it were their greatness as they become alive again before our very eyes, that is, in a manner that has been designated as ergographic. I am relieved in this connection of the obligation of stressing the necessity of correct methods of historical research. This question has been so thoroughly investigated and so thoroughly presented by the author of *Master Minds in Medicine* that all further remarks would be superfluous.

But another question, a reply to which might at first sight suggest a justification for this book, forces itself on our attention. In any case it seems to deserve a discussion by the medical historian in introductory remarks like these, and he is perfectly justified in stating his position with reference to it. Let us consider what significance leading personalities have had for the development of medical science, what their significance was in the past. Such inquiry is of great value for the present, and will prove valuable in the future.

Certain views have found wide acceptance to the effect that the collective effort of the masses has been of decisive influence in science as well as to progress in general. The collective energy is regarded as the decisive impulse in the development of medicine. Let us attack with sober historical method such doctrinaire constructive philosophizing which befuddles "modern" thinking. It will then become apparent that the assumption of a progress distilled from the instinct of the masses is a mere fiction. Real medical scientific progress cannot be produced by "communis opinio" or mass suggestions. Such progress demands intense effort of specially trained individuals, who obtain their guiding ideas from the intuitive genius of creative leaders.

If we call upon the past to respond to our question, the reply will be unequivocal; there is no foundation for the collectivistic theory in reference to medicine and science.

We might perhaps speak during the Babylonian-Egyptian period of a "communal knowledge" produced by the common labor of the members of a caste that guarded such knowledge as their secret. But this impersonal effort of many individuals of the priestly caste along the shores of the Euphrates was blighted by early paralysis and gradual ossification. Not a single one of the medical personalities that we know from both civilizations contributed in any way creatively to the traditional knowledge nor can any one be considered the discoverer of any important fact or of any valuable curative measure. The individual is a cipher, only the guild the caste, the corporation is of importance. These are the guardians of the traditional lore, which is codified, becomes obsolete, decays after early stagnation, the principal cause being the lack of leading individuals.

There is a marked change in the second period, in a modest degree in India, but more significantly among the Greeks, a people so highly endowed by nature. Thinkers and scientists of dominating influence emerge who bring about great and lasting progress in the whole range of the physical and biological medical sciences, a progress that makes itself felt for centuries to come. Each work is backed by its author. These leading intellects for the first time produce a real science, they elaborate and guide it as living individuals. The final conveying of this knowledge to the work of Islam is effected by the intermediation of individuals, through Greeks, Syrians, Jews, Persians. In the Orient this science, emanating from individuals is never interrupted until the heirs of Dschingis-Khan hurl their fire brands into the palaces of Bagdad. When in the Occident, Greek Science perished upon the destruction of the Roman Empire, the continuity of tradition was interrupted for lack of competent individuals. If it had been possible to have progress coming from the masses how it would have appeared among these peoples, full of youthful life, eager to learn, highly gifted! Medicine like all other sciences fell back into the lower impersonal stage. After the lapse of centuries the labor of the medical guild of Salerno presents itself as the supreme effort of this period of stagnation. A modest guild knowledge there brings about a continuation of ancient medical practice and science on a small scale. Only when the medical individualities of increasing independence of thought and action arise and write their "works" through the intellectual infection from the Orient, through Constantine the African, the instrument of Providence, is the stagnant mass aroused to life. This scientific life spreads in the young universities of Italy, France, and England; everywhere we find names that make a strong appeal; with the Renaissance the full independence of research is inspired with a new life.

The time of great scientific personalities, as in the epochs of ancient Greece, has returned; the leaders of progress of most individual type are at work. The long line of "master minds" runs without interruption from the thirteenth to the twentieth century, many of whom Hemmeter has treated in his book in such attractive and masterful fashion.

True the "great man" is the offspring of his people, which has transferred to him the function and responsibility for progress. The people are the maternal womb from which he sprang; he is their creation, for their own higher purposes and for the world at large as well. He is a child of his times. But in spite of all the influence of environment, the great thoughts are his, his very own, the product of that unique individuality that places him among the elect. The people create or produce him by a process not yet understood, for their own purposes and for humanity. Let there be no confusion of ideas about this; let there be clear thinking. It is not the masses who overcome the errors, who tear down the crumbling outworn structure, and putting something else in its place, thereby erect the new pantheon of science. To mention only one example of progress, it is not the masses, but a Semmelweis, a Pasteur, a Lister, a Koch, who have bestowed on humanity the blessings of antisepsis and asepsis. They had the great intuitions, they created the methods and the masses generally accepted them. To great individuals is given the great vision. They wield the bow and what is obsolete falls; according to their own plans they build the temple of the new science. Howsoever many enthusiastic followers may coöperate with them, the work is theirs. That is the lesson which the History of Medicine conveys to the open-minded scientist, from the beginning to the present day.

KARL SUDHOFF.

Leipzig
October, 1925

MASTER MINDS IN MEDICINE

MASTER MINDS IN MEDICINE

I

METHODOLOGY IN MEDICAL HISTORIOGRAPHY

MEDICAL historiography can be founded upon a basic stock of facts and truths, in which the manifold and different sources are clearly concordant, and from which any possibility of fraud or counterfeit can be excluded and, therefore, the synthetic deductions be accepted as indubitable. Goethe said about this subject that the history of a science is the science itself; which aphorism could fairly include the meaning that eminence in medical historiography for example is conditional upon two capacities: first, competence in the methodology of scientific historic research; and secondly, masterful control of one or more of the manifold sub-domains of knowledge and art that constitute Medicine.

Medical history cannot be written by the possessor of either of these two capacities alone. It has not been my experience that even brilliant teachers, although eminent in one or two specialties, gave evidence, as a rule, of training in the methods of medical history in those endeavors that extended into historiography. Much of the medical history of to-day evidences lack of training in the methodology, a most essential and peculiar preparation requiring much time and patient application.

In his *Skizzen,*[1] Karl Sudhoff remarks, "The longer I occupy myself with the history of medicine, the more it becomes clear to me that up to the present day it is an accidental (casual) science, without a well planned elaboration or method."

Historic events are to be regarded as a series of inwardly connected developments, and the ability to regard them as such is not inherited by man, but can only come as a product of high intellectual culture. Whenever a unified presentation of succinct chronologic events results from a synthetic elaboration of data, etc., which indicate a penetrating research, there is a preponderance of evidence that the presentation is a genuine and ingenuous piece of work.

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[1] Richtungen und Strebungen in der medizinischen Historik, 1921.

The most experienced and conservative historiographers are in agreement that in almost all subjects that are worthy of historic investigation, the general facts and data, the records, documents, monuments, relics, remnants of various kinds, etc., are so mutually entwined and supplementary that it would be impossible for the most skilled and systematic falsification to fabricate a uniform historic structure of these instruments of record. Even the attempts at such fictitious work have soon been detected by connoisseurs of historiography.[2]

Without intending to be guided by a religious tendency, it becomes necessary to cite as an example of historiography that has been subject to the most scrutinizing and critical investigation for 1900 years, the historicity of the genuine epistles of St. Paul, and the historicity of the Acts of the Apostles.

Would anyone capable of conservative and critical judgment believe that the thousands of documents, records, and collateral records, inscriptions, monuments, relics, historic accounts in many ancient languages, geographic studies, etc., scattered all over the ancient and modern world, could have been invented, and so fabricated into a synthetic mosaic, from which falsification the early history of Christianity could be constructed? There are technically trained historians who are so imbued with the "unreliability" of the history of religion that they avoid using religious sources wherever possible. Such hypersensitiveness to these sources would deprive the medical historian of indispensable information, for not only is Medicine intimately entwined with the history of Christianity, but much earlier and more so with religious rites and functions of ancient Mesopotamia (Babylon and Nineveh), and of the Egyptians. In fact, everywhere, early healing art appears as a religious function. The archives of history of religion are inestimable sources of medical information, but critical judgment and broad experience are necessary in making use of such sources. Extensive historic and other literary products of antiquity cannot be invented so as to fit accurately into other remnants of evidences preserved for us; I mean, of course, to fit homogeneously into form and general content.

Especially convincing are the occurrences during past historic researches, when remnants of documents, monuments, and other sources turned up, confirming an old conclusion, which sources and evidences were not known at the time this conclusion was reached. Who could, for instance, doubt the approximate year of accession of a Mesopotamian monarch or his period of reign, when both an ancient record of a religious character, and a clay tablet record found during the last century agree?[3]

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[2] See for example "Die Apostellehre und die Jüdischen Beiden Wege," Adolf Harnack, Leipzig, 1886.

The methodology of *Medical* history must follow and observe the principles and rules of the methodology of *General* history if it is to be elevated to the dignity of an academic science in the sense of Sudhoff. All the methods and principles for guidance in historic research are laid down by numerous English, German, and French historians, and in the last twenty-five years American historiographers have come forward with systems equally inspiring of confidence.

A thorough formal education in methodology is the unconditional premise to all successful medical historic investigation, but it is rare that the medical specialist is capable of acquiring a formal historic training in methodology without special effort. This is true of the physcians of the United States, as well as of Europe.

The trained historian, however, is much more frequently found, who effectively works himself into the mental control of a specialty in medicine and some of our best medical history has been presented by men who had no medical degree nor clinical experience.

This same question arises with regard to art, military tactics, religion, and engineering. It is a fatal error of all specialists in any science or art to assume that in treating historic questions in their special art or science, they need no methodic historic training.

Shall medical history then be written by a specialist in one or the other medical branches, who has acquired historic training, or by a professional, technically experienced historian, who has acquired the special knowledge of that part of medicine that is to be illumined?

1. *The medical specialist requires technical historiographic training in methodology of history.*

2. *The technical historian requires training in the understanding of theoretical and practical medicine, which is easier to acquire, because he takes up only very restricted and limited domains at a time; the knowledge of medicine that he needs is easier for him to acquire than the technical methodology is acquired by the clinician.*

The reason for this difference in facility in acquiring each other's knowledge-spheres does not lie in the nature of these two subjects, but in the nature of the two different types of minds.

A doctor of medicine is as a rule not a linguist, nor a patient bibliophile, nor habituated to precise, accurate weighing of evidence, because his practice demands of him to effect results promptly. But a historiographer must be a philologist, his patience in record searching must be unlimited.

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[3] See J. Taylor: "History of Transmission of Ancient Books to Modern Times," together with the progress of historical proof, 1875.

He is by nature a person who weighs the "pros" and "cons" deliberately; thus it has come that better medical history has, in my opinion, been written by trained historiographers that were not clinicians, than by the professional clinician himself.[4]

QUALITIES AND FITNESS OF A HISTORIAN

To judge of the qualities and fitness of a historian, we must consider his personal station, his education, intellectuality, personal relations to the subject under consideration, elimination of patriotism and religion as tendencies influencing historic judgment; whether he avoids rhetoric and whether he offers good reports concerning his opponents and criticism concerning his friends.

One of the first tests of historiography is the discernment between original knowledge and personal acquaintance with the sources and documents, and simple narration of an author, according to what he has heard or learnt from others. The evidence of personal acquaintance and research is an ear-mark of the historiographer. The copied narration is an ear-mark of the writer who is presenting only entertaining causeries. As a guide in making this discernment we must look for comparisons between the most approved authors and endeavors to bring out repeated accordance of reports and sources. Such evidences are the surest indications for the actuality of the events described.

One must not be too rigidly severe in censuring erroneous statements and inaccurate opinions. Mental indolence is a condition that can befall the best of historians at one time or another, and then the repetition of such errors may creep in without being tested. It is therefore not wise to condemn an author as unreliable because of single incorrect or fanciful statements, but at the same time such statements are not to be accepted without distinguishing them from well-grounded other statements of such an author, that is, provided he can be judged in a general way to be a conscientious historiographer.

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[4] As examples of effective *medical history and medical scientific contributions written by laymen,* I may cite: TOLLIN—A Lutheran pastor, author of many excellent investigations on the history of the discovery of circulation. All of his contributions are quoted in my work on the history of the discovery of the circulation (*John's Hopkins Hospital Bulletin,* 1905), see Chap. XI. GOMPERTZ—In his "Griechische Denker" admirable chapter on Physicians of Ancient Greece. MOMMSEN—*Medical Organization of the Roman Armies.* DA VINCI—Eminent Classical Anatomist. GOETHE—DISCOVERER of the intermaxillary bone, vertebral theory of cranial bones, color vision, morphology. EBSTEIN (1) *Medizin des Alten Testaments;* (2) *Medizin des Neuen Testaments.* HAUPT—Many contributions of great merit on Babylonian, Assyrian, Egyptian, and Judaic Medicine. Brilliant interpreter of the medicine in Cuneiform texts and Leviticus, etc. EBERS—Deciphering of the famous Papyrus, author of many medical historic notes in his extensive writings.

In the third volume of his "History of European Thought in the Nineteenth Century," John Theodore Merz presents a fascinating chapter on the growth and diffusion of the critical spirit, in which he delineates the characteristics of the methods of the great German historian Niebuhr and his comprehensive picture of the old world which he formed for himself out of the connection of historical events and political forces. He speaks also of the methods of Leopold von Ranke, of Theodor Mommsen, of Ernst Curtius. It is instructive to read the analyses of the methods of these historiographers which Merz gives. Whilst he assigns to each a certain individuality of method, he also points out that the fundamental quality of all of them was a poetical and artistic comprehension of the *totality* of the subject they treated. There is a danger in medical historiography as well as in general historiography that the artistic presentation may suffer from two factors:

1. The tendency to penetrate too deeply or too far into a problem, of dissecting and analyzing it so far that the reader is incapable of perceiving the event or thing described as a whole. In fact there are two ways of going into the history of a medical event; one may be called the *analytic* method, and the other the *synoptic* method. The latter cannot be properly executed without a previous following out of the analytic method. The two methods are reciprocally and mutually inter-dependent, and excess of penetration by either method alone can only lead to a diffuse, unclarified presentation of thought.

2. The second distracting factor is excessive mass and weight of material introduced into the presentation. It must never be forgotten that one of the objects of historiography is pedagogic; it is to serve as didactic aid to discipline.

Right here we must be mindful of the heuristic principle which aims to effect understanding in the student, not so much by wealth of fact, as by leading the student to discover the truth for himself. It is by the heuristic method that experienced educators can be recognized.

This same principle is applicable to medical historiography. The endeavor should be not to force too much detail into the historic picture, the end object of which is to succeed in a comprehensive and readily understandable presentation for the clinician of average understanding and culture. (Heraclitus Aphorism XV, XVI: "Too much knowledge does not lead to understanding.") If we cannot recognize any definite method of historiography in such masters as Ranke, Mommsen, Niebuhr, Curtius, nor in Wilhelm and Alexander von Humboldt, nor in our American historiographers, Prescott, Bancroft, Jameson, Rhodes, Hart and others, nor in the English historians: Edward Gibbon, Macaulay, Lord Acton, etc., nor in the French historians: Xénopol, Seignobos, Langlois [5] how could we expect to find it among strictly medical historiographers? There are great thinkers on these subjects who, like Mommsen, did not postulate any special method for historiography. All that was necessary according to this eminent writer, was a knowledge of language and of laws of the times of the epoch to be studied, and Seignobos, the great French historiographer, states that every theory of historiography must either be transcendental or trivial. ("La Méthode historique," etc., Paris, 1907.)

PEDANTRY AND EXUBERANCE OF INTELLECTUALISM

Whenever the endeavor is made to be too distinct and intense; industry is applied to explain self-evident things or to refute what never has been supposed, then pedantry begins, which should not be condemned too severely, however, for I can look upon it in no other way than excessive virtue and as an outgrowth of industrious reasonableness.

Ludwig Riess in his *Historik* goes too far, however, in his defense of pedantry as a factor in historiography, for after all the correct meaning of pedantry is vain ostentation of learning. The colossal multiplication of magazines and journals in the United States, and the increase in the number of the daily papers, bring with them such a fierce competition for the popular taste that it has become a downright trick to formulate pedantic short articles and so-called catchy headlines. We have grown entirely too gentle and tolerant toward literary productions of to-day which in every sentence show the frantic effort for popular appeal, articles which give evidence of lack of system and order, absence of the succinctness of argument, and the thread of thought continuity. They ensnare the reader with dynamic expressions, paradoxical half-truths, with a literary pose in which the pseudo-genius is only too evident. Even the average medical reader is too easily duped by the fulminating rhetoric of authors who are in reality imparting no knowledge whatever. This applies to Nietzsche, to Houston Stewart Chamberlain and Gobineau. Even R. W. Emerson, who is supposed to have been influenced by Schelling, does not always give evidence of closed convincing argumentation or logical comprehensiveness. The order of argument and harmony when they do occur in Emerson, are hardly continued long enough to make the experienced thinker feel that he is led by the hand of a master. It has been said that (to express in short what I have stated of Emerson) "his pearls are not held together with a string."

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[5] Introduction aux études Historiques, 1898, and also Manuel de Bibliographique Historiques, 1904.

A great deal of medical history that has been printed appears to me to be nebulous, reaching further than can be substantiated by the facts. One often finds in the journals devoted to medical history, articles giving no evidence whatever of original thought. They constitute what might be termed *medical feuilleton* which may entertain us by the apparent certainty of hitting the aim, but are devoid of an indispensable historiographic style, needful definition, and transparency of thought. It is much to be regretted that even in medical history the taste of the times, if I interpret it correctly, expresses itself in an admiration of scintillating aphorisms. There is too much ready acceptance of lax diffuseness and pseudo-talent. Some European writers have given this exhibit of glittering *bons mots* the designation *Aphorismus.*

METHODIC PRAGMATISM IN MEDICAL HISTORY

Medicine, whilst it is a part of biology, has become an independent cultural entity because it is an indispensable necessity to human existence. It is a healing art as well as a science. In short, medicine owes its growth to its pragmatic results. In writing a history of such an art and science, therefore, we cannot avoid picking up lights for guidance by the test of pragmatism. To make the forces of nature subservient to man is the end object of all science, but to make natural forces subservient to the comfort and preservation of health of man is a specific domain of medical art and science. Therefore we are justified in seeking in methodic pragmatism a guide for the orderly arrangement of medical history. Great has been the change that came over historiography in the course of the nineteenth century. Sir John Herschel, in an address on the influence of science on the well-being and progress of society, stated it in the following terms: "history not as formerly, the mere record of tyrannies and slaughters, which, by immortalizing the execrable actions of one age, perpetuates the ambition of committing them in every succeeding one, but as the archive of experiments, successful and unsuccessful, gradually accumulating towards the solution of the grand problem—how the advantages of government are to be secured with the least possible inconvenience to the governed."

An account of the manners and customs of different peoples and their struggles from the darkness and bondage of pre-historic times to light and freedom, is not relegated to isolated chapters or to the meager appendix of so-called political history. The historians whom I have mentioned in the preceding have already expressed the idea long ago that the progress of culture and civilization, that the laws, arts, sciences and industries and the common life of a people, form by far the most important side of history. We are having now the beginnings of a history of the popular masses, their occupations, habits, interests, impulses, general and special psychology. Historians nowadays deal more with the continuous, not so much with the discontinuous forces of historical life; with the properties of masses, not so much with the characters of individuals. Now as the masses of human beings are a part of nature, their changes are very gradual and imperceptible, rarely sudden or catastrophic. This explains why the historians of contemporaneous periods deal mostly with those phenomena which are analogous to the slow moving processes of nature and the term *evolution* has come in appropriately to define the quality of the events, transactions and changes which historians are attempting to describe and explain. *Evolution is history,* but historic evolution should deal not so much with persons and special events as with the imperceptible and gradual development of great organizations of human society, that is, with the life and interests of the masses. Here we have reached a critical conclusion, for if contemporaneous history is a description of the collective phenomena of human society independent of the individual, then medical history is entirely separate and distinct from general history, for the evolution of medicine as far as the history of that subject reveals it, is not attributable to any efforts of the masses, but rather to the work of individuals. The masses are concerned in those special domains of medical science which we designate as social, industrial medicine, in the hygiene and sanitation of large groups of human beings, especially in epidemiology, medical statistics and biometry. I am not aware, however, that the masses have been instrumental in bringing about any advance in medical science by their own collective effort. On the contrary, there appears to be a pernicious tendency of the masses to drift into the vices of lack of hygiene and disregard for the most primitive rules of health, not to speak of the fanatic sacrifice of health and the actual invitation of suffering recorded in the history of certain religious cults, the Flagellantes for instance.

The progress of medicine is the history of the work of individuals and that is the principal reason why I have devoted this work to a description of a few selected master minds in medicine, in order to make it clearer to us by which mental operations and at what cost and effort our present knowledge of medicine has been attained.

There have been historians who were so firmly convinced of and impelled by the collective conception of social-psychic forces that they represented them as existing in a sense outside of and independent of the individual, like a certain mystical something that directs the individual. We find such expression among the Germans and French particularly, as *Esprit des temps,* or *Volks-Geist, Zeit-Geist.* Gustav Freytag says that human beings of different centuries live together contemporaneously. A historian who deals with so-called psychic mass-dispositions and wishes to operate with such a conception to formulate an outline of history, has to bear in mind that within one and the same people great groups of beings of the same civilization have shown no evidence of such a mass-disposition, so even in general history, the feelings and impulses of the masses, are only obscurely understood. What are we to understand on reading the aphorisms in L. Gumblowitz ("Grundrisz der Sociologie," 1885, S. 167), "That which thinks in man is not he himself but the social community that is in him"? In his "Sociologie und Politik" (1892, S. 54), he states "Sociology offers man, the individual, upon the altar of knowledge. In sociology the individual sinks down to zero. Even the mightiest statesman is only a blind tool in the invisible almighty hand of his social group, which in turn follows the irresistible social natural law." On the other hand, K. Lamprecht distinguishes between influences of society on the individuals from influences of the individuals on society. This thought I have expressed in my article on Leonardo da Vinci as a Scientist, when I stated that history makes men, but also men make history.

Opposing the view of Freytag and Gumblowitz (and this is important for the history of medicine), we have the words of Hegel (Hegel's Werke, Bd. IX, S. 44): "Every human being is a child of its own times." If we draw the consequences out of this aphorism, we will recognize the impossibility of the statement of Freytag. Our naïve human subjectivism inclines us to premise our own conceptions and feelings as existing in the human beings that lived before us and also in those that will live after us. Wundt has characterized this error as "vulgär Psychologie." The historiographer must make every effort to emanicipate his readers from this involuntary habit of applying contemporaneous personal thoughts and motives to ancestors and posterity alike. He must show how the conceptions and feelings of man in the period that he is investigating have changed from other preceding periods, and if possible trace again the further changes into succeeding periods. In other words, human viewpoints, perspectives and outlooks on the vital questions of different periods must be studied in order to apply them in accordance with that period which is to be investigated. These viewpoints are influenced by governments, by contemporaneous forms of religion, by climatic and environmental changes, etc., etc.

Long before Freytag and Gumblowitz, Dante and Aristotle (De Monarchia) expressed the same ideas. Both first of all asserted that the supreme power of the human mind, that quality of man which makes him a man, is the power of understanding things. It is thought. But Dante goes further than Aristotle concerning the sociologic source of thought. He asserts that the endowment of understanding things is not an individual power. No one can thus think by himself. It belongs to man as a species, and only by the multitude of other men can any individual man be capable of increasing his faculties. Dante, however, wrote during a time of wars and civil unrest. The epoch of the rebirth of science, or Renaissance, had not yet begun, and the multitudes of men from whom, according to his idea, the individual derived his reason, were in his time (1265–1321) not coöperating agents engaged in a great process of building up truth, but they were mere passive participants in a movement not initiated by them, a movement that was an outgrowth of the universalism of Christianity. It was not until the rebirth of science in the 16th and 17th centuries that historiography records the social nature of the progressiveness of thought. The thoughts of progress and humanity which are basic elements in sociology, are not noticeable until the beginning of the 17th century. The demonstrator of the circulation of the blood, William Harvey, is credited with a pregnant phrase, "Mind begets mind," and this is indicative of a so-called collective mind that is supposed to be evident in the development of scientific truth. It is not only evident in the scientific literature of that age, but the historian must learn to appreciate that it becomes more and more evident in the revelations of archeology, for it is archeology which has established the conviction of a converging tendency in human thought of all ages towards common conclusions about ourselves and the world in which we live, and it is dispelling the barriers which nationality and religion of the medieval people had set up to divide mankind. It reveals the entire human race as struggling together from a common starting point to a common goal.

The discrepancies between the statements of Hegel, that the human being is a child of his own times and of Gumblowitz and Freytag and Dante, that that which thinks in man is not himself but the social community which is in him, are more apparent than real, for even at the present day the idea of the inheritance of acquired characteristics is still a subject of scientific discussion. But if acquired characters are inherited, then individuals of any time or period must of necessity have some of the thoughts of periods antedating their own. If they did not have such thoughts by inheritance, they would gain them from the records of history; so there is some basis for the assertions that in one and the same period of time there are individuals of many past epochs. But again the assertion of Hegel that every human being is a child of its own time, is a sociologic axiom because, it is only through the environment of his own time that he can become that which he is, the two basic factors of human evolution being inheritance and environment.

ROMANTICISTS AND CLASSICISTS. CONSERVATIVES AND PROGRESSIVES

There are in reality men who bear the impress of the past historic experience of the race in them at the present time and there will be such men at all times who in their manner of thinking, in their conduct, in their deliberate way of action, always manifestly based upon a previous judicial weighing of the "pros and cons," show that they are not guided by the motives and influences of the present day. Such men have been called the conservatives.

But there are men who in thought and action are exclusively the children of their own time. They are quick to take the initiative on any question of action, they are the men of affairs who precipitate political movements, regardless of what the traditions and historic customs of their people have been. In all human history so far as it can be discerned as recorded or written history, the conduct of human affairs has been the resultant of an adjustment or compromise between these two apparently opposing forces. What Ostwald in his "Grosse Männer" terms the classicists and the romanticists are in reality nothing but these two types of men personified by their different pulse of mentality in the realm of scientific investigation. A great many different terms have been given by students of human psychology to these two different types but it has not been sufficiently emphasized that the coming into existence of two types of mind of this character is so far beyond human control that we may almost look upon this phenomenon as an *automatism of history.* In the development of Medicine, these two types of mind are as noticeable as Ostwald makes them out to be in general science. In the conservative or as he terms him, the classicist, we have pronounced reverence for authority and this may reach an excessive degree so as to actually prevent the classicist from making a great discovery. An evidence of this is found in the anatomic and physiologic studies of Leonardo da Vinci. It is my firm conviction that he would have grasped the entire truth of the physiology of the circulation of the blood before Harvey, if he had not been over influenced by the views of Galen. (Refer to the article on the *Physiology of Leonardo da Vinci* Chapter XXVI.) It can also be seen in the various chemists and physiologists who gradually and laboriously built out the structure of the physiology of respiration. Here the inhibitive force was a marked respect of the *phlogiston* hypothesis of Stahl. At one time even such a clear thinker as Lavoisier placed this *phlogiston* in the table of his chemical elements (see article on Lavoisier and the Physiology of Respiration and Metabolism Chapter XXI).

As these historic examples could be multiplied and extended into most fields of medicine, it is important for us to realize the reciprocal interaction of these two types of mind as a principle of human activity. History as Bernheim defines it, is the Science which presents the facts of the development of mankind in its activities as social beings and investigates them in connection with psycho-physical causality.[1] As medicine is one of the oldest activities of human beings and one of the activities that penetrates deeply into their social relations, it is an axiom that the history of medicine has a justification like that of the history of any other Science. It rests on its own foundation. Medical Historiography is developing its own methods of research and whilst Sudhoff asserts in his "Skizzen," (page 17), that we are not far enough advanced in any domain of Medical History to erect an enduring structure, nevertheless a very substantial foundation of the methods of Medical Historiography has been laid, a truth which must become evident by a study of Sudhoff's own publications, for a greater master of the historic art in our science has so far not been produced among medical men.

WHAT IS MEDICAL HISTORY?

This is the title of the third article in Sudhoff's "Skizzen" in the perusal of which the sincerely interested physician and surgeon will discover that he has entered a domain of science in which he may have thought he was hitherto quite well versed, to suddenly discover that he is passing through new territory with every step and has really not fully grasped what is meant under the term—*medical history*. It is not simply a record of medical events nor of the actions of eminent Asclepiads nor of the discoveries of research workers. It is not a history of the revelation of other sciences bearing upon medicine. It is not a chronologic arrangement of years or dates in which important medical ideas originated or discoveries were made, it is not a record of various doctrines that were

[1] Bernheim, "Lehrbuch der Historischen Methode," 1908, page 9.

taught or of the different conceptions of anatomic, pathologic, physiologic, clinical, therapeutic and surgical endeavors that were conceived and executed. All these things, important though they are, constitute nothing but the material of Medical Historiography. As the surgeon would term it, it is the instrumentarium. What then is Medical History? It is the evolution and elucidation of the historic cultural and scientific developments, medical ideas, events, doctrines, etc., with regard to their inner connections. Medical history aims at a real understanding of the periods, their physicians and their manner of healing. We can look upon medical history in two perspectives:

First: the *pragmatic medical history* of which I have already spoken and which I shall define once more here. In pragmatic medical history the historic material is of importance only in so far as it can be applied to definite utilitarian purposes. The pragmatist wishes to learn something for practical application out of the happenings and occurrences in history. This form of history writing has been recognized as afflicted with tendencies, for the danger is near to deduce motives and purposes from the historiographer's own subjective viewpoints whereas they did not exist in reality. Then there is also the tendency to explain present events out of past events and again, the tendency to overlook motives and historic facts because they are not in accordance with his own personal viewpoints.

There is no exact English word for the German *tendenziös* which means really having a pronounced tendency or purpose; to term it "being biased" is perhaps a little too strong because some of the earlier historians were *tendenziös* and not exactly biased.

Secondly: *The genetic, evolutional or developmental history.* This type of investigation aims to establish how any given historic event or development became what it is. It seeks an inner connection of the events, it is not satisfied with the mere collection and ordering of material nor to deduce a practical or instructive phase. It aims to understand the specific and peculiar character of the historic material and in fulfilling this comprehensive problem it does justice to the lesser problems of pragmatic historiography at the same time. It is genetic historiography which has recognized that historic events constitute a series of inwardly connected evolutions and the ability to recognize this is the resultant of a very advanced intellectual development.

Bernheim has a third type of history which he calls the narrating or reporting history. As this is, however, only a method of the oldest forms of historic interest and does not imply historic research, it occurs in medical history only for the purpose of preventing from falling into oblivion or of entertainment or gratifying esthetic interests. It is an improvement on that type of medical history writing which I have designated as medical *feuilleton.*

The *narrating* or *reporting method of historiography* is the oldest and most primitive form of giving an account of medical activities. In general history it is most clearly represented by Herodotus, the first scientific representative of narrating history, and to the present day medical histories of a certain type have never aimed to get beyond this method of simple narration, which contains no evidence of the higher problems and aims which modern medical historiography has set for itself.

In spite of the higher aims of genetic historiography there is a justification for the narrating form. There will be a permanent need for tabulation of the historic material as such—to facilitate the work of historiographers by a prospectus of medical data, documents, events, doctrines, relics, discoveries, etc., etc. J. Pagel has given us the special historic medical literature in his "Einführung in die Geschichte der Medizin" (1898, S. 345–960) and Sudhoff presents a "Tabelle der wichtigesten Fortschritte, Erfindungen und Entdeckungen seit dem Untergang des Römischen Weltreichs" etc., etc. (506–512) of his *"Kurzes Handbuch der Geschichte der Medizin"* which gives evidence of many omissions and gaps: for example there is no mention of William Beaumont nor of the Americans concerned in the discovery of ether anesthesia, nor of Major Walter Reed, James Carroll, Jesse Lazear and their brilliant discoveries in the pathogenesis of Yellow Fever; Henry R. Carter is not mentioned. But Sudhoff's "Tabelle" is only a beginning and too short to embrace all of even the "Wichtigsten" advances and discoveries.

Ludwig Darmstaedter's "Handbuch der Geschichte der Naturwissenschaften" is a work of 1262 pages beginning with 3500 B. C. and extending to 1908 A. D. It is time that American medical historiographers went to work on systematic compilations of this character, for it is a great aid to have medical events, etc., arranged chronologically in epitome or compendium form by competent authorities. Narrating or recording medical historiography may also create a wider interest in medicine among the laity by gratifying naïve interest and curiosity, even if it is argued that chronology is not history.

Our talented American medical historiographer, Fielding H. Garrison, gives to his epoch making work the title "An Introduction into the Hisory of Medicine with Medical Chronology, Suggestions for Study and Bibliographic Data." In this work American medical history has found itself and in it we find, what may be considered a medical chronology beginning with the year 7000 B. C. and going to 1920 A. D. and in the appendices he presents in a highly instructive way, the leaders of medical historiography and biography in this country and Europe, mentioning particularly Osler, Welsh, Billings, Dock, Camac, and Streeter.

Prof. Julius Pagel of Berlin, whose lectures on medical history I attended in 1906, made at that time no distinction between pragmatic and genetic historiography. This is evident from his statements (on p. 17) of his *"Einführung in die Geschichte der Medizin"*—where he includes in pragmatic History of Medicine:

1. Progress of development of Medicine in the broadest sense.
2. The entire content of Medical Doctrines, Training, Opinions and Viewpoints about the most divergent domains of the healing art of all times and all peoples.
3. Emphasis on those facts to which progress proper is known to be bound.
4. Consideration of those men who as representative physicians manifested the greatest interest in contemporary productions of medical science and their evaluation for clinical application, that is the historiography of the profession, as such, in its entirety.
5. The training and education of physicians—didactic discipline—clinical teaching.
6. History of Epidemiology.

All of this is of course a subject of pragmatic historiography and more, but when this splendid medical historian on the same page (17 l. c.) continues to state that in the various medical historic periods he intends to point out the *continuity of medical thought* and the inner *connection of medical ideas,* which are evidenced in the new results of investigation that modify pathologic and therapeutic fundamental perceptions, he is already exceeding the limitation of pragmatic and entering upon genetic historiography.

Pragmatic historiography is an improvement upon simple narrating historiography because it is not satisfied to arrange and record what has happened, but it desires to ascertain how and from which causes and purposes everything happened. It thus seeks motives for historic transactions.

The first one to follow this method was Thukydides: in Book 1, Chapter 2 of his "History" he definitely announces this program, namely, to trace out the *Why, How and Wherefore of Events.* It was really Polybius who first used the term (*Historia lib.* III cap. 31—also II–56–I–1) *Pragmatic History.* But he understood under this term only a history of affairs of State.

I have already called attention to the infirmities of the pragmatic method. It unconsciously traces all events to psychic causes, neglecting all physical and cultural elements. There seems to be also the logical defect that pragmatic historians incline to the tendency of determining the objective possibility of future occurrences on the basis of inadequate generalization of past causes and insufficient analysis of individual occurrences of the past. In writing special biographies of great medical men this tendency is quite discernible to the critical eye; it appears in some of the best medical biographies that we have. Personal impelling forces are very likely to be overestimated in those types of biographies.

GENETIC OR EVOLUTIONAL HISTORIOGRAPHY

This form of history writing aims to ascertain how each individual event, fact, etc., evolved in connection with other historic occurrences. It is the correlation and coördination of historic occurrences that is aimed at. In this endeavor historiography has developed into a science, because it is here that it aims at pure knowledge of the great material of history.

All of the great historiographers that I have mentioned in the preceding, have confessed themselves as adherents of genetic or evolutional history. Three intellectual requisites are necessary to fully appreciate the significance of this evolutional historiography.

1. The conception that all historic occurrences and events constitute a series of inwardly connected developments. Human occurrences, then, have an inner relationship and continuity and this leads to the view of the unity of the human race. Such an idea could not be conceived in antiquity because the necessary accumulation of historic material was wanting. Where little was known or recorded of history this unity of the race could not be thought of. This idea also lays the foundation to the conception of a diffuse, general community of interests of human beings, of the solidarity of all humanity. It was Christianity that taught this idea. H. G. Wells in his "Outline of History" attributes the sense of solidarity of mankind to the political and social unheavals of the last ten years. One might as well say that the last ten years spread a notion of the dissimilarity and disintegration of the human race; but that Christianity first effectively taught and aimed at the solidarity of the human race is acknowledged by all of the great historians that I have mentioned.

2. The conception that the different activities of human beings stand in reciprocal connection with each other and are correlated among themselves and with the physical environment. In other words, human activities are under the influence of climate, tribe, geographical location

and the type and character of the nation, race or people among whom they transpire, but they are also dependent upon the social relations, the arts, sciences, religions, and forms of constitution. All of these factors have a marked influence upon the fortunes of a people and reversely the events occurring among the people in turn affect these factors.

3. The conception that a continuous and progressive change has been and is now going on in all human affairs. This view like the first and second view was impossible in Antiquity as well as in the Middle Ages because the perspective of unity of the race from which the uninterrupted connection of events could have been perceived, was missing. During the Middle Ages the idea of human unity began to arise but it was so remote, so intangible, that it did not occur to any historiographer to bring it into application as a principle of historic evolution. It is from the standpoint of the totality of perception of historic events, that historic science can successfully be developed as a coherent discipline.

TABULATION OF SUBJECTS FOR MEDICAL HISTORIOGRAPHY

I. Medical Things—Books, Documents, Letters, Instruments, Monuments, Apparatus, relics pertaining to history of medicine. Example: When and how and by whom was the opthalmoscope or laryngscope, or miscroscope invented.

II. Medical Persons—Biographies, Great Medical thinkers, Heredity and Genetics of Master Minds of Medicine.

III. The Systematization—Coördination, origin and growth of medical knowledge. Medical epistemology. The sciences contributing to the structure of Medicine, including History of Anatomy, Biology, Physiology, Pathology, Bacteriology, Pharmacology, Medical Botany, Chemistry, Psychology, Medical Geography, Archeology, Philology, Geology, Paleontology, and Climatology, Paleopathology, Biometry and Statistics, Psychiatry.

IV. The Art of, or Applied Medicine—Historiography of Conceptions of Nature of Disease. The study of disease by observation and experiment. History of Diagnosis, of Treatment, Therapeutics, Hygiene, Prophylaxis, Dietetics, Applied Medicine, Surgery, Obstetrics, Gynecology and Medical Specialties, Architecture and Construction of Hospitals, Military Medicine, Sanitation.

V. Relation of the State and Governments to Medicine—Medical legislature. Epidemics and Epidemiology. Position of the Clinician before the law. History of Medical Sociology. Medical Statistics. University Administration and Medical History. Historiography of Medical Relations of large Industries, Corporations, Public Schools, etc.

VI. PHILOSOPHY OF MEDICAL HISTORY—Introducing professional rules, regulations, and customs. Ethology. Historic development of Medical Syndicalism and Communism. Medicine among Aboriginals. Prehistoric Medicine. Medical History of the Ancients. Assyrian, Babylonian, Chaldean, Egyptian, Roman, Greek, Sudaean, Persian, Arabic, Chinese, etc. Medical sculpture, Paintings, tradition, anecdotes, proverbs, Periodization of medical events. Chronologic epochs.

CONCLUSIONS, "A POSTERIORI" OR HISTORIC INFERENCES FROM THE KNOWN TO THE UNKNOWN

One of the most impressive methods of attaining certitude about events long past is the logical elaboration and demonstration of the connection or intertwining of historic facts, data and events among themselves so that they are shown to be an unbroken series of developments. The historiographer is incapable to transpose himself to times long bygone in which those developments transpired, but by intensive and concentrated investigation and reflection, he can realize their actuality by their consequences and results. He can experience immediately and personally live through again, in the present time, what those bygone actualities and happenings were, from their effects and consequences which are before him at the time when he is investigating them. The historiographer can retrace and argue backwards from present consequences of happenings and acts that took place long ago, what the necessary presuppositions and implications of those results he sees before him in the present, must have been in the ages when they actually occurred. This retrograde method of deduction and argument cannot only be executed from our present standpoint to a remote period but from any intervening point in the chain of historic evidences. In short, it can be demonstrated that culture transformation and scientific evolutions that occurred a thousand or more years ago are intimately bound up through their consequences with later events in such a way, that the earlier always influence the later, down to the state of affairs in the midst of which we are living to-day. This method of retrograde (regressive) historic investigation ought to be designated as the method of Thukydides because he was the first to use it. For those, who, like Klebs, argue that it is futile to occupy oneself with prehistoric or aboriginal history (it is true that Klebs means only medical history), it will be worth while to read the declarations of Thukydides concerning that which was, even at his time, stated (460 B. C.) to have occurred in "the dim distant past."

As this historiographer was not only the first to use exact methods of research in the records of general history but was actually the *first medical historiographer* and as this side of Thukydides is not brought out in any contemporaneous historical work with exceptions of perhaps the great three volume "Handbuch" of Neuburger and Pagel I believe I am rendering a service to modern medical historiography by presenting this brief biography of Thukydides.

His father's name was Olorus, his mother's Hegesipyle. He possessed gold mines in Thrace opposite the island of Thasos, and was in consequence one of the richest and most influential men in Thrace. In 431 B. C. the Peloponnesian War, which forms the subject of his work, broke out. In 430 the plague broke out in Athens. Thukydides contracted it, but recovered, and in 424 B. C. commanded a squadron of seven ships at Thasos. The Spartan general Brasidas besieged Amphipolis, and Eucles, who commanded that important post, sent to Thukydides for aid. Thukydides made all haste in preparing to answer the summons, and seems to have used his private means to forward his equipment, but Brasidas, apprehensive of the approaching relief, offered favorable terms to Eucles, which were accepted, and Thukydides only arrived the day after the surrender. Yet he was in time to save Eion from falling into the hands of the enemies. Whether this transaction led to the banishment of Thukydides, or whether he exiled himself to escape death as its probable consequence, is not known; but from this time he became an exile, and the duration of his exile according to his own account, was 20 years. Before the conclusion of this term the war terminated (404 B. C.) and all political exiles were permitted to return. Thukydides returned to Athens in the following year; and met a violent death a year or two later, but at just what time, and whether in Thrace or Athens, is not known. During his exile he remained close to the theater of the war, of which he was a diligent observer, and as he could not remain on Athenian territory he may have passed this period of his life, or the greater part of it, within the domains of the Spartan alliance. It is also probable that he visited Sicily and southern Italy. His history consists of eight books, the last of which differs from the others in containing none of the political speeches which form so striking a feature of the rest, and is also generally supposed to be inferior to them in style. Hence it has been thought by various critics to be the work of a different author, of Xenophon, of Theopompus, or of a daughter of Thukydides; but it is more probable that it is the author's own without his final revision. The history is incomplete, the 8th book stopping abruptly in the middle of the 21st year of the war. As a historian Thukydides is in the front rank. He was painstaking and indefatigable in collecting and sifting facts, brief and terse in narrating them. His style is full of dignity and replete with condensed meaning. It is, however, sometimes harsh and obscure from overcondensation. He is unsurpassed *in the power of analyzing character* and *action, of tracing events to their causes,* of appreciating the motives of individual agents and of combining in their just relations all the threads of the tangled web of history, and his work has been styled the "Statesman's Handbook." It is to the superiority and impartiality of his judgment that he owed the power of producing a work which should be, as he himself said, a possession for posterity. (Encyc. Americana, and Gomperz' "Griechische Denker, Geschichte der Antiken Philosophie.")

Thucydides is one of the first historians to give a description of an epidemic, which considering that he was not a physician is a meritorious account.

Victor Fossel in "Geschichte der Epidemischen Krankheiten," 2nd volume of Neuburger and Pagel's classic "Handbuch" (p. 736) instructs us concerning the symptomatology and the epidemiology of the Athenian plague. Hére I simply wish to emphasize that Thukydides, the first one of the great medical historiographers, evidences such excellence of historic technique that I cannot avoid pointing out two methods that are original with him. First, the *a posteriori* method spoken of in the preceding. The second is the *evaluation and application of the present conditions of contemporaneous people at a low stage of development* for the purpose of throwing light on the antecedent and primitive stages of culture of nations that have advanced far in civilization. This is a means of investigation which is used to-day in the history of ethics, religion and law because there are, even at the present day in the central part of Brazil, people who live under conditions of the old stone age, and in New Guinea there are human beings who live in habitations built on piles.

From this retrospective argumentation Thukydides concludes that his own countrymen acquired the consciousness of national solidarity at a period not far removed from his own and that in an earlier stage of ethnology they were not very different from the Non-Greeks or Barbarians; that brigandage and piracy were the chief sources of acquisition and that the unsafety of communication and transportation and poverty in human beings and estates, held down his race and prevented their rise in culture.

METHODOLOGY OF HISTORIOGRAPHY

Almost all ancient nations record periods of such rapid changes in the forms of government and in the underlying basic principles of government that men of stability of character found it very difficult to preserve and maintain any fixed political or religious convictions.

Such convictions are, however, indispensable to the progress of man kind; for example, the conviction of monotheism among the Jews and of the solidarity of the human race among the early Christians, or of the equal rights to "life, liberty and the pursuit of happiness" among Americans, etc., etc. Such moral convictions form the incentive to the activities and the conduct of these races. They are so self-evident at the present day that they appear almost as social or political axioms. And there are many more of such convictions, some of which are not so readily defined, but yet great wars have been waged and the dominion over great countries and millions of human beings gained and lost, or regained and re-lost for the maintenance or the destruction of such convictions. It has even occurred that skillful, though unscrupulous, leaders have contrived to make use of such deeply rooted convictions in order to accomplish for a time at least the very opposite effect of that which the conviction or principle proclaimed, or really warranted. (πολεμος πατηρ παντων—Heraclitus.)

At such times it was not wise, at least not expedient, to express any firm convictions that were contrary to critical contemporaneous movements in political or religious life. The great men that disdained to bow to such expediency became the martyrs of their convictions. The historic path of all nations is written with blood of martyrs, for the bed rock of principles upon which humanity is founded must not be permitted to undergo destruction. There will always be men who will prefer loss of life to loss of moral principle. Must the prophet then always become a martyr? Not always, for there is a way of upholding and preserving the dangerous truth without involving so great a peril to the prophet or the historian, and this method we owe to the inventive power and genius of Thukydides.

It is the method of employing the orations and announcements of other persons, other characters, that are sometimes real, at other times the product of the historiographer's invention, in order to bring into the white heat of discussion, a neglected or forgotten fundamental truth, which though at variance with the temporary prevailing ideas, constituted a principle of such enduring guiding power that its loss would have proved a catastrophe in the long run.

In this temporary sacrifice of morality to expediency, the elemental truth is eventually saved, which if announced directly from the mouth or pen of the historiographer would not fail to arouse the resentment of leaders of opposing opinion.

By this heuristic type of historic artifice the masses are unconsciously led to truth, by indirect artistic persuasion and at the same time often imagining that they found their way to light by virtue of their own ingenuity.

What has been termed mass psychology has been contrasted by some historiographers with the psychology of the individual as a causality in the evolution of history. Much has been written about this phenomenon of human events by Seignobos, Lacombe, Lamprecht, Bernheim, etc., and the subject is of far reaching importance because in it are represented some of the mightiest factors of history.

The activities of human communities are distinguishable as of three kinds:

1. The individual or singular activities.
2. The collective.
3. The typical.

These three types of human activities are not separable into three distinct categories by hard and fast lines.

1. The *individual* or singular type of activity or occupation is the one in which the historic effect proceeds and emanates from the efforts or influence of one person.

2. The *collective* activities are the effect of the total social processes of larger communities—the so-called mass activities. The doing of human beings becomes an action when it is directed toward a definite purpose with the full consciousness of the personal will. Things done with no concentration of purpose are transient and as a rule play no rôle in history and things done without the consciousness of personal will make no impression on the formative course of events. In the collective activities, the great mass acts as a unit, comparable to a totality individual.

3. The *typical* activities—concern the cultural achievements of a class of human beings at a given chronologic period. In the typical activities, the identical achievements of many are embraced, yet in such a manner that the coöperation of each individual can be taken as the average example.

According to the nature of historical material and its contents, it must be evident that its scientific treatment permits of no systematic classification. And the conception of motives and activities as 1) individual, 2) collective and 3) typical cannot be established according to any law that has so far been studied by historians. I have not become convinced that historic events can be forced into a mathematic expression or can even be demonstrated to be sequences of or subject to any form of law.

Psycho-physical causality has to a limited extent been found expressible in laws or rather rules, and as historic knowledge depends upon psychologic rules to a certain extent, these rules form the basic structure of the understanding of men among themselves and therefore of their activities. To a large extent human emotion, sensation, will, and conception in their fundamental, elementary form are identical. They constitute the processes of consciousness which reveal themselves in history as the most constant motives for human action.*

To a limited degree then the historiographer may assume a constant uniformity and peculiarity of human feeling, willing and thinking, and the conception of these characteristics as incentives to activities and motives; furthermore it might be thought that historic phenomena could be deduced as effects of certain causal psychologic laws. Let me compare this idea with that of the deduction of physical effects from physical laws. In line with the above, historic phenomena would thus be conceived to be explainable on a basis of natural laws and as these concern themselves with human actions, history would thus be conceived as a biologic science. It contains peculiar and specific conditions however in addition to the truths and phenomena of pure biology. For history deals with the activities of human beings as *social* creatures, whose characteristic is to occupy themselves, by establishing their own aims and purposes, with ever heightening and expanding consciousness (E. Bernheim). In this peculiar feature, history differs from and goes beyond biology.

The great historians of the English speaking races, as well as of the German, French and Italian, agree, however, that causative psychologic laws have only a very restricted application to historic phenomena. In the following biographies of eminent medical investigators, it will be evident that the biography of a single man cannot be circumscribed by psychologic or biologic laws. Much less so the history of great communities or nations. Nevertheless an attempt of this kind will be made in the chapter on the psychography of the investigator. Chapt. XXXI.

A. D. Xenopol, "Les principes fondamentaux de l'histoire," and also K. Breysig in "Der Stufenbau und die Gesetze der Weltgeschichte" (in *Archiv für Kulturgeschichte,* 1906, Bd. 4, S. 92 f.), have endeavored to show that a few of the more stable general forms and processes of historic occurrences, the broad motives and forms of activities of human beings which happen in connection with the common human requirements and certain tribal properties of individuals as well as of groups, could be deduced from psychologic causality, but the true content of the various stages of culture cannot be determined by such generalities, for in spite of all the similarity and homogeneity of the effective physical and psychologic laws, human activities are not determined by the direct impressions brought to bear upon human beings, but by the specificity and peculiarities of their reflex psychic responses to these impressions.

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* G. Simmel, Die Probleme d. Geschichtsphilos. 3 Aufl. 1907, p. 14.

Ch. Seignobos. l. c. p. 120: "Si le faits rapportés dans le documents n'avaient pas été analogues á ceux que nous observons, nous n'y pourrions rien comprendre.

Let us assume a community of individuals with an equal disposition and equal reactive power brought under the identical influences, even then the consequent reactions cannot be predicted in a lawful manner so that, for example, the historiographer could say that if the following causes a, b, and c, continue to act upon the individuals D, E, and F, we shall have the result X, Y, and Z. The reason for this difficulty in calculating from a predetermined cause the expected effect, consists in the different psychic reaction of the human individuals, D, E, and F, and also in the well known observation of contrast effect. When the same causes repeatedly act on the same human being, it has been observed that a new and entirely different reaction ensues, one that is at variance with the effects observed when it is applied only once. But it cannot be ascertained when the contrast effect will occur nor what it will be, because it depends upon the degree of psychic irritability of the individuals and, therefore, escapes determination in a mass. This is true of all so-called social-psychologic laws. The *fundamental fact of human existence is the qualitative difference of the individuals.* This brings it about that changes of cause do not always result in corresponding changes of effect. The same cause can produce different effects and at times it is not possible to ascertain the cause until one or another effect has actually taken place and then the cause can be disclosed only by *regressive* investigation, and for the same reason, *similar* effects can be produced by *different* causes. One cannot say that similar phenomena were produced by similar causes, but in each case, mass effects must be solved regressively for each specific case. This means that the consequences of a historic mass effect, even if disclosed and revealed, are attributable only to the one case or incident that has been investigated. By no means can such a disclosing and revealing of cause and effect be made or construed into a general law.

The possible relation of the activities of masses of human beings to the fundamental theory of probability and the function of the methods of statistics to history will be discussed in Chapter VII.

We have progressed far enough to understand why the activities of masses under given conditions as regards time, place, and character of the human beings cannot be predetermined. Now as influential leaders of men must avoid producing an effect which is contrary to the one they desire, it is above all things necessary to bring the masses to the consciousness of the right path, action, or duty, in a manner which is at least partially in accordance with their own convictions, and which is also not calculated to demonstrate that the leader is hostile to the masses. In this finely balanced judgment of the temperament and psychology of the masses Thucydides gave evidence of the highest art in historiography.

Sir William Ramsay in his book "Paul the Traveler" (page 17), states it as a fault of Thucydides that he made characters speak declarations which they in reality never uttered. In this criticism Ramsay does not give evidence of a thorough understanding of Thucydides, for the very process he has criticized constitutes a feature of the art of this early historiographer, who, as we have seen, was also a medical historiographer.

Especially noteworthy is the historic method of Thucydides where he declines to admit the stories of deities and miracles as being effective in influencing the course of history. Not for one moment does he credit the possibility of breaking through the course of natural events by the fictitious power of oracles and prophets. To give an example of this clarified judgment in writing history, one which is on the border line between his general and specifically medical history, and one which characterizes his contempt for superstition, I will refer to his account of the outbreak of the plague during the Peloponnesian War. There was an old edict of the Delphian Oracle which stated the following: "Once will come the Doric War and with it the plague." Now the Greek word for plague is *loimos,* but the word for famine is *limos.* Commenting on the similarity of these two terms, Thucydides continues, "at the time when the epidemic broke out, the meaning was construed to be *loimos,* or plague, for the people adapted their remembrance to what they actually experienced, and if so it is my opinion that if a Doric War should ever again break out and there should be a famine in consequence, they will then construe the Delphian Edict to mean *limos* or famine." There are other similar edicts of the Pythian God which he rejected with stinging sarcasm. There was, for instance, a warning of the oracle that the Pelagic Field to the northwest of the Acropolis in Athens should not be settled, but after the flat land was greatly devastated by the Spartans, the inhabitants outside of the city fled in masses to Athens and this forbidden field was occupied, in spite of the prohibition of the Pythian God. From the overcrowding great misfortune came to Athens, which was attributed to the violation of the Pythian warning. Here Thucydides declares, "It seems to me that the oracle edict was fulfilled in the reverse manner from that in which it was originally intended or understood. It was not the prohibition and the settlement of that field which caused the misfortune of the state, but the war produced the unavoidable necessity of that settlement. Now the Oracle at the time that it made that pronouncement saw in advance, although it did not speak of a war, that such a settlement would never be made except in a period of great distress."

Thucydides also asserts that it is very pernicious to acquiesce in superstition which induces the masses to turn to prophecies and oracle edicts and the like, during situations where salvation might yet be possible through human power, whereas these prophecies actually lead to destruction by encouraging deceptive hopes. May these examples suffice as indicative of the rare and unprecedented exactness of the first medical historian.

If we come now to his masterful description of the plague, which at all times has created the admiration of specialists, I will have to refer to the investigations of historiographers and epidemiologists who have made the *plague of Thucydides* the object of special investigation. Among the newer authors, I refer to the brilliant American work of Victor C. Vaughan, "Epidemiology," Vol. II, p. 771; also to Wilhelm Ebstein, "Die Medizin im Alten Testament," pp. 94–101, and to the same author's "Ueber die Pest des Thucydides." The criticism has been made from the description of Thucydides, that one could not definitely ascertain that it was the bubonic plague as we understand it to-day. Professor Vaughan thinks it might have been either typhus or the pneumonic form of the plague. In the absence of any bacterial knowledge of the nature of the infection, a better description than that of Thucydides could hardly be given. I need only to remind the reader of the doubt that arose in the medical mind during the destructive epidemic of influenza which caused hundreds of thousands of deaths all over the world during the war of 1914 to 1918, to illustrate, that even in our times the most accurate nosology does not necessarily imply a knowledge of pathogenesis and etiology.

ON THE NECESSITY OF METHOD IN THE WRITING OF MEDICAL HISTORY

One occasionally reads in historical journals that the so-called method or technique of historiography is a self-evident thing, that it requires no preliminary knowledge and that therefore anyone could write history who has familiarized himself with the sources and facts and that no systematic preliminary training is necessary. This includes the idea that anyone who chooses could write history in an autodidactic manner. We have referred to a sufficiently large number of the difficulties involved in correct history writing to offer a reasonable refutation of that assumption. Even at the present day the methodology of medical historiography is not sufficiently developed to reproduce effective pictures of what happened in the medical world of the past. It takes a trained historic consciousness to weigh all the historic evidences, facts, documents, and tradition in the balance, to know how to interpret and how to combine them, and to become disciplined in the recognition of historic factors.

Some of the objectors to methodologies have argued that each individual historic case had a specific form, and if this was worked out from the sources and other historic factors, it was suitable only to that single case and not to others, and therefore the investigating historian was compelled in each case to work out the special, corresponding method of procedure. I have already referred to this in the paragraphs on individual, collective, and typical activities of human beings. While this objection is partially true, it does not prove that methodology is an unnecessary thing. The question which this objection raises has been compared by Ernst Bernheim ("Lehrbuch der Historischen Methode," p. 181), to the fancied need of a special method for each pathologic condition in clinical medicine. Each disease has an individual clinical picture and has to be worked out through its own signs and symptoms, notwithstanding that a systematic elaboration of the methods of diagnosis, treatment, etc., has become more and more necessary. There can be no doubt that no science has suffered more from dilettantism than historiography (George Weitz in Sybel's *Historische Zeitschrift,* Bd. I, p. 20). In his methods of historical study (p. 92), E. A. Freeman condemns the so-called historic dilettantism as productive of no good results, and G. C. Lewis, "A Treatise on the methods of observation and reasoning in politics" (1852, Vol. 1, p. 5), says: "The mere knowledge of a scientific method of reasoning will not lead a person to right conclusions more than the mere possession of a chest of tools will make him a carpenter" . . ."but with equal natural ability, equal study, and equal experience, the man who is provided with a good method will outstrip him who employs a defective or feeble method or who trusts to mere common sense."

One of the most helpful treatises on this subject is the work by Professor John Martin Vincent, entitled "Historical Research" (1911).

THE DIFFERENCES BETWEEN THE MATERIAL OF THE NATURAL SCIENCES AND THAT OF THE HISTORICAL SCIENCES

The material of historic science is so popular and so accessible that almost every daily paper contains what is designated an historical article.

The majority of these articles are written in what I have described as the narrative method and few of them rise above the caliber of *feuilleton.* But this popularity is also due to a mistaken impression, inasmuch as it confuses the interest concerning the historic material with the interest in the scientific presentation of that material. The material of historiography is not immediately capable of perception and cognition as is that of the natural sciences. According to the best definition of the subject matter of history, that of Bernheim, *Historic science is the science of the activities of human beings as social entities, presented in connection with psycho-physical causality.* It presents the human activities through the facts of their development in time and space. Ludwig Riesz ("Historik," p. 60), characterizes this definition as too narrow and argues that the conception of development which Bernheim declares is a specifically historic conception, belongs to natural science. This criticism is more apparent than real, for a large part of history is of necessity natural science because it deals with the activities of the uppermost living thing on earth; therefore Bernheim's definition is a good and comprehensive one. But we cannot immediately perceive the events and activities of human beings, nor subject them to the experimental methods as we can with the material of natural sciences. Under rare and favorable conditions it has been possible for the historiographer to personally and immediately perceive the events which he described, but this is the exception and not the rule.

The three main objects of historiography are

1. The collection and classification of the historic material and factors.

2. The determination and confirmation of the actuality of the events and occurrences. This means the verification and certainty that they really happened, which is accomplished by the method of historic criticism, the word coming from the word κρινειν meaning discernment and decision, not fault-finding.

3. The perception of the inner connections of these factors. This leads to the conception and interpretation of the historic material, as that of a continuous process wherever possible.

Although they demand separate systematic study, the knowledge of the connection and the establishing of the actuality, condition each other mutually and interact continually. Say, for example that the object of a historic investigation is to establish whether Vasari is a dependable historian of the period of Leonardo da Vinci. (See article on Leonardo da Vinci, by the author. Chap. XXIV.) The manner of procedure will be the following:—

EXAMPLE OF HISTORIC METHOD IN DETAIL

All writers that are contemporaneous with Vasari will have to be searched for any judgments or expressions of opinion concerning him, especially those contemporaneous authors which Vasari himself cites. Then we would have to select a few single statements of Vasari and test them concerning their correctness, credibility, and sources, continually bearing in mind the state of the literature of his period. Having done this with regard to a few of his statements, we eventually form a judgment of the credibility of every particular of his biographies. Most of the errors in the conception and the criticism and lack of clearness in the deductions are caused by the fact that historical writers operate without a conscious control of the factors which I have just explained. They assume them to be correct. They tacitly accept statements without having tested them.

Historic investigation operates with psychologic principles which are only partly gained through INDUCTION proper. A larger part of such principles of experience are gained by ANALOGY. Now the difference in historiography between induction and analogy as means to obtain certitude is this, induction is reasoning from particulars to generals, but analogy refers to a parallelism and similitude in things which are in some respects different. If human thinking, emotion, and will, which condition all historic conceptions were identical at all historic periods, we could reach conclusions easier, but these psychologic principles differ at different historic periods and they are generally only relative and very rarely absolute. Sometimes in spite of the finest argumentation and critical research into the sources concerning a man, say for instance Michael Servetus, the discoverer of the pulmonary circulation of the blood, we find that our manner of conclusions is occasionally different from those which we would like to employ in the biologic sciences. In fact, in writing medical historiography we are compelled to use both the methods of general historiography as well as those of natural science. As far as Michael Servetus was a physiologist, we can use the methods of natural science. But wherever he displays himself as a speculative theologist and philosopher, we have to use the methods of general history.

Elsewhere I have stated the contrast between these two methods of historic research, namely, the methodology of general historiography and the methodology of the natural sciences. After all, the facts of medical history contain more of the properties of natural sciences because they are capable of verification to a large extent by the methods of biology.

PERIODICITY, DIVISION OF MEDICAL HISTORY INTO TIME PERIODS, AND CHRONOLOGY

The arrangement of human and of medical events into various time periods has occupied the thoughts of great historiographers of general history for a long time. We find evidences of it among the Greek historians. It is stated by C. Wachsmuth ("Einleitung in das Studium der Alten Geschichte," p. 93), that Diodorus had a chronologic arrangement which however was only an external juxtaposition of events without any effort to explain their inner connection. It was impossible to divide up the entire substance of historic material prior to the spread of Christianity, because before that period, the essential conditions for chronologic arrangement of human activities were wanting, namely, the indispensable conception of humanity and its fate as a solidarity, as a connected unit. Accordingly the first chronologic arrangement of history was of a religious order. In fact the time periods of Augustine ("De Civitate" Dei, XXII, 30), divides up the periods into seven: first from Adam to Noah; second, Noah to Abraham; third, Abraham to David; fourth, David to the Babylonian exile; fifth, Babylonian exile to the birth of Christ; sixth, age of Christ until the end of the world; seventh, the super-mundane age, the eternal sabbath of heaven. That there were important historians who actually carried out this time division is indicated by the work of Isidor of Sevilla.

That other systems of historic chronologic arrangement came about later is seen in the historians since the Middle Ages, for all through the Middle Ages the theologic periodization of history prevailed. Fielding H. Garrison in his "History of Medicine" gives us a periodization of medical events which from the scientific and pragmatic standpoint is highly to be recommended:

I. Egyptian Medicine
II. Sumerian and Oriental Medicine
III. Greek Medicine
  1. Before Hippocrates
  2. The Classic Period (460–136 B. C.)
  3. The Græco-Roman Period (156 B. C.–576 A. D.)
IV. The Byzantine Period (476–732 A. D.)
V. The Mohammedan and Jewish Periods (732–1096 A. D.)
VI. The Medieval Period (1096–1438)
VII. The Period of the Renaissance, the Revival of Learning, and the Reformation (1453–1600)

VIII. The Seventeenth Century: The Age of Individual Scientific Endeavor
IX. The Eighteenth Century: The Age of Theories and Systems
X. The Nineteenth Century: The Beginnings of Organized Advancement of Science
XI. The Twentieth Century: The Beginnings of Organized Preventive Medicine

DISCERNMENT BETWEEN PROBABILITY AND CERTAINTY OF MEDICAL HISTORICAL EVENTS

In some of the branches of medical history, exact mathematical methods can be applied for the verification of data, so for example in biometry for purely anatomical or clinical purposes; also the application of higher mathematics in the newer general physiology, such as have been applied by the late Jacques Loeb, Osterhout, and their pupils, and probably in all of the problems that Raymond Pearl brings out in his "Medical Biometry and Statistics." I shall endeavor to give the conception of the general historian concerning the value of statistics in historiography in Chapt. VII. In the special history of medicine, the statistical methods are gaining more and more importance, not only in the two subjects I have just mentioned, but also in epidemiology, sanitation, medical architecture, and the application of the principles of hygiene. A good practical system for the use of the physician to familiarize himself with the methods of such mathematics as are of special importance in the subjects mentioned, is the work by Leonor Michaelis, "Einführung in die Mathematik für Biologen und Chemiker."

The statistical method was also used, together with biometry, by Sir Francis Galton, Professor of Eugenics, in University College, London and by Carl Pearson. The method of statistics and theories, however, has only been stated precisely in very recent times. In fact, Carl Pearson might be called the creator of biometry, and Yule, in his well known introduction to the theory of statistics, was the first to define its methods in 1922. In this country, these two methods are most prominently brought forward in the works of Raymond Pearl, Herbert S. Jennings, William Travis Howard, Jacques Loeb, and C. S. Peirce, "A Theory of Probable Inference," Johns Hopkins University Studies in Logic, 1883.

The very fact that biometry and the statistical methods are of most recent development is evidence sufficient that they are not suitable for application to the periods of history of medicine when such methods did not exist and were not even thought of. If we therefore wish to decide between the probability and certainty of historic events that happened during the lifetime of any of the great thinkers referred to in this volume, none of the special theorems in probability, nor of the "Wahrscheinlichkeits und Fehlerrechnung" of Leonor Michaelis (p. 260), would help us very much, because these methods all require certain definite data and figures as starting points, and where these are not furnished by history, we cannot use the statistical method, and in the absence of such exact methods, we can only use the methodology of historic induction, of inference, analogy, and parallelism (see previous and succeeding *Methodology*). Important factors to be held clearly in view are: first, the personal qualities of the authors, their mental equipment, freedom from bias and tendencies; second, the distinction between actual events and their modifications by the viewpoint of the reporter or witness. It has been repeatedly proven that a given event may undergo manifold changes by the narration and re-narration of witnesses; third, the notoriety or popularity of the events as established either by personal witnesses, relics, monuments, documents, and regressive conclusions from present relations and effects; fourth, the clarification of contradictory data and the elucidation of different possibilities therefrom, during which certitude must be clearly separated from probability, and in this, it will be found that there are various degrees of both certitude and probability; fifth, the evaluation of the motives of historic events; sixth, where there are many witnesses, we must ascertain which one had an actual original knowledge of the occurrence and events; seventh, ascertaining whether the author we finally favor most is immune to tendencies conscious or subconscious, due to religion, patriotism, attachment to institutions, and great human organizations of widespread influence, such as political parties and sects; eighth conscientious regard for the work of preceding thinkers in the same field of endeavor. The more sincerely a historian strives to place his seeming adversary in a favorable position, the more dependable are his own deductions. It is not wise to speak in a too severely critical manner of those works with which we are led to differ as a result of our own study. Frequently the conclusions of the severest critics in medical history have themselves proved to be erroneous. Utterly to be condemned is the use of sarcasm and irony.

The eventual aim and purpose of all of these procedures is impartial conception, objective presentation, graphic description, and a mental visualization and realization of the full truth of the past brought to the present time as far as that is ascertainable by these methods.

The four great subdivisions of medical historic methodology are:

1. Investigation of sources.
2. Historic criticism.

3. Synthetic reconstruction of the evidences gathered by 1 and 2 (Synthèse historique of Seignobos.')

4. Presentation of the facts in language, capable of ready understanding.

The term of Seignobos, *Operations Synthètiques,* which he uses in his "Introduction aux Études Historiques," is the equivalent of the German word *Auffassung.* It is a combination of the conceptions with the subsumption of all the details evaluated by the above methods.

II

METHODOLOGY IN MEDICAL HISTORIOGRAPHY

SOURCES OF MEDICAL HISTORY. SUBDIVISIONS OF METHODOLOGY AND HISTORIC CRITIQUE

SOURCES are records of human activities and thoughts in medical pursuits which reveal the knowledge and demonstrate historic facts in an original manner, responding to the intellectual state of a given period. French historic authors speak of sources as *documents* and they distinguish *documents matériels* and *documents écrits*. German historiographers use the word *Quellen* which is their word for the term "source." The investigation of the sources is only preliminary work. It is only the means to an end. The investigator must be careful never to allow his thirst for knowledge and his curiosity to lead him to consider source investigation as the main end of historiography. I have elsewhere defined the term "heuristics" as suggestive of a pedagogic method, a kind of investigation which leads the student to discover truths for himself. In this sense investigation of sources is a type of heuristics.

What we desire to understand through the sources is the activity of human beings as physicians and surgeons and as medical investigators. It is possible to understand more recent activities and events by having personally and individually lived through a period of medical development, and having come in personal contact with the men that were the principal workers in special medical or biological research. If you will pardon me for speaking of myself, by way of example, I can state that, I may be able to describe the modern physiology of digestion without reference to sources that go back further than 1890, because I have been personally engaged in research based on this subject and because I have had as my own teachers six of the leaders of medical thought and medical research in this special line.[1] I could also present a history of the transmission of the infection of yellow fever by the bite of an infected stegomyeia fasciata, because I have known personally Surgeon-General George M. Sternberg, whose personal instruction in bacteriology it was my good fortune to enjoy, Major Walter Reed, Dr. Jesse Lazear, and Major James Carroll of the United States Army Yellow Fever Commission; and Henry R. Carter, the discoverer of the period of extrinsic incubation of the yellow fever parasite was a personal friend. Major James Carroll was for a time a student under my instruction, so the author becomes a source when he himself has either an immediate and direct experience with the historic subject to be investigated, or an intimate personal acquaintance with the workers who accomplished special great pieces of research.[2] This is applicable, however, as a rule, only to contemporaneous historic events, and of these only a very small part is directly perceptible to contemporaneous observers. Even here a large part of the investigations must be based on the reports of others.

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[1] A large publishing house has requested a History of the Physiology of Digestion from my pen.

Immediate perception and personally living through a series of events is an important source, but on account of the specific character of each individual observer, and the facility with which psychologic elements affect personal interpretation, and also because immediately after observation every such a record passes into phases of memory,—for all of these reasons and contrary to the usual belief personal and immediate observation of contemporaneous events is not considered the highest and best source of history.

Sources will vary according to the special object of a medical investigation, and the aspect of the inquiry. This latter denotes the mental attitude of the investigator concerning medical history. The Germans call this the *Fragestellung,* for the point of inquiry determines the extent, the direction and the entire nature of the historic investigation. It cannot be emphasized too strongly that in all historic investigation the point of inquiry is the most important thing to establish before one begins to write medical history. At the very beginning of a historic research, let us understand *"What is it that I want to know?"* and *"Is there any prospect to accomplish something new or to make the subject clearer by my investigation?"* After a precise understanding concerning these questions has been reached, the investigator will be largely safeguarded against a confused and oblique conception of his work and theme. This preciseness will prevent a diffuse and aimless seeking as well as a too concentrated and limited perspective. Even during the progress of an investigation, it will prove to be a saving habit to come back to the precise point of inquiry and put it before our minds every now and then.

There are some subjects in medical historiography which through the fascinating grip they have upon the contemporaneous interest compel the attention of prospective writers, and which produce a psychic state that is quite noticeable in the literature of any medical period. It is a kind of literary halo of an evanescent type which accompanies the appearance of any striking piece of work or discovery in medicine, for example, the discovery of the x-rays by Roentgen, the identification of the bacillus of tuberculosis by Koch, the work on the digestive glands by Pawlow, the investigations of Pasteur concerning fermentation, anthrax, and hydrophobia, and just at the present time, the isolation of the principle which controls carbohydrate metabolism from the islets of Langerhans of the pancreas. Under the stimulus of these and other epock-making discoveries, an encouraging progress has been made in medical knowledge and the control of disease. Yet, at the same time, at each one of these periods it has become the fashion to write on the contemporaneous subjects without a precise point of inquiry, the result being that much of the accompanying new literature has brought nothing new, but is only indicative of the mental compulsion under which some authors believe what Juvenal states as: "Tenet insanabile multos scribendi cacoethes." It could almost be set down as a general rule that the least historic investigation is done in those domains in which it is most necessary that historic research should be carried on. The cause of this is very plain. There are no already existing publications to stimulate mental interest, and to urge inquiring minds on to an investigation into these unexplored regions.

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[2] The work of the U. S. A. Yellow Fever Commission I have repeatedly discussed and studied with William H. Welch, Sternberg, and Carroll. See Chapter XV and XVI.

BIBLIOGRAPHY AND COLLECTION OF SOURCES OF MEDICAL HISTORY

Fielding H. Garrison's "History of Medicine" gives the best collection of sources that are available in American libraries; other collections of sources in the English language are found in the "Studies in the History and Method of Science" by Charles Singer, Oxford, England. A very valuable collection of sources is that by Julius Pagel, "Geschichte der Medicin," which contains a Historisch-Medicinische Bibliographie für die Jahre 1875–1896. Karl Sudhoff rewrote this work in 1922, calling it "Kurzes Handbuch der Geschichte der Medizin," which also contains a wealth of sources and bibliography. But throughout Sudhoff's *Archives* and the many volumes of his "Studien" as well as his *Mitteilungen,* an abundant source-bibliography is available on almost every conceivable subject in the history of medicine.

The volumes of *Isis,* edited by George Sarton, are to be recommended highly.

The *Annals of Medical History* published under the editorship of Dr. Francis R. Packard of Philadelphia, has grown into a most important collection of sources and historic studies in medicine. Packard's separate historic researches, not published in the quarterly, are to be recommended. *The Bulletin of the Society of Medical History of Chicago,* edited by Dr. Morris Fishbein, contains much valuable historiographic research by American authors. In addition to these, the American medical profession enjoys a monthly journal of historiography entitled *Medical Life,* edited by Dr. Victor Robinson, of New York, and containing historiographic research, not only by American, but also by eminent foreign contributors.

It is my duty also to recommend the study of Ludwig Choulant's "History and Bibliography of Anatomic Illustration," translated and edited by Mortimer Frank (University of Chicago Press); also "Historical Research, an Outline of Theory and Practice," by J. Martin Vincent. Lovers of more ancient methodologies will be gratified by Choulant's "Anleitung zu dem Studium der Medizin," and also by "Methodus Studii Medici" of Boerhaave, the latter being edited by Haller.

PALAEOGRAPHY

Before printing was invented by Johannes Gutenberg (1436–50), all preservation and transmission of records by writing either on parchment, wood, soft clay, wax, or cutting into stone. And historic research into medicine prior to the fifteenth century, to be original, required a knowledge of palaeography and understanding of the forms of writing. The English historiographers are referred to articles on this subject in John M. Vincent's "Historic Research" (p. 46), and in Bernheim's work (l. c., p. 289).

DIPLOMATICS

In general history this is the knowledge of legal papers written in regular form and testifying to transactions executed according to law. They are sometimes of importance in medical historiography to ascertain when, for example, in the case of Leonardo da Vinci, who was an illegal child, he was received as a legally adopted child in the family of his father; furthermore, the date of Leonardo da Vinci's registry in the Compagnia dei Pittori Firenze. That these dates are important in the life of this great genius, is evidenced from the articles on that subject contained in this volume. Chapter XXIV.

We could define diplomatics as the doctrine of the genuineness of historic documents. In studying the history of medicine of the Middle Ages, for instance, the "Medieval Libraries of Medicine, 500 A. D. to 1500 A. D.," by Edward Clark Streeter, a knowledge of diplomatics is extremely important.

GENEALOGY OF GREAT MEDICAL THINKERS

As a means of tracing the descent and family origin of medical and scientific thinkers, genealogy is indispensable to historic research. It is true, history reveals many imperfections in the genealogies of great men. Not only the family themselves, and their historic predecessors, have been unable to resist the temptation of bridging over gaps in the genealogy, but they have yielded to the tendency of tracing the more prominent members of the family to some distinguished historic person. John M. Vincent (l. c., p. 37) is of the opinion that the adoption of a family tree is no longer a serious matter to the historian in America from the documentary point of view. Except as a social phenomenon of more or less amusing interest, the subject does not afford much material. With medical men, however, this is different, for here it is a duty of science to trace, if possible, the origin and descent of human genius. I am not sure that such a problem of human genetics is at all capable of solution, but a beginning has to be made, such as, for instance, the publication on "Hereditary Genius" by Sir Francis Galton (1869); "The History of the Sciences and of Scientists since Two Hundred Years, with Studies on Inheritance and Selection in Human Beings," by Alphonse de Candolle (This work is originally in French, and has been translated into German by Wilhelm Ostwald); "Makers of Man," by Charles J. Whitby, "Grosse Männer," by Wilhelm Ostwald (1910); "Control of Heredity," by Casper L. Redfield.

O. Lorenz ("Die Geschichtswissenschaft in Hauptrichtungen und Aufgaben") has endeavored to develop genealogy into a truly genetic discipline, and in Leipzig there exists an association for the preservation of personal and family history which even edits a publication on the subject.

MEDICAL GEOGRAPHY

Geography enters as an element in medical historiography as soon as considerations of a climatological nature are brought into the subject. It is also concerned in the question of the quantitative and qualitative production of food of various nations, especially of the quality and quantity of the drinking water. Ostwald, de Candolle, and Galton have considered the localization and geographic distribution of human genius and arrived at instructive conclusions. In anthropology, the subject of historic geography looms up very large, as also in the subject of medical botany. An important three-volume treatise on medical geography was published by Leonhard Ludwig Finke in 1792–95. On page 717 of his "History of Medicine," Fielding H. Garrison gives nine references of authors who have written on medical geography. It is clear to foresee that for epidemiology and medical statistics a new medical geography will gradually be developed, which may inform us concerning the geographical localities from which epidemics spring, and their favorite routes of extension. Such geographic knowledge will be essential to a more successful sanitation of these original sources of epidemic diseases, and it is much to be desired that a specialist in medical geography should arise to work hand in hand with the pathologists, epidemiologists, statisticians, so that the various governments may be impressed with the necessity of international sanitation, but as a prelude to all this, the history of medical geography must first be investigated.

In a lecture on "Epidemiology of Intestinal and Respiratory Infections: an Experimental Study" at the school of Hygiene and Public Health of the Johns Hopkins University, Dec. 8, 1924, by Dr. Simon Flexner, Director of the Rockefeller Institute for Medical Research, much emphasis was laid upon the changes which various microörganisms undergo, whereby they can either become infectious from a previous non-infectious state, or become non-infectious from a previous virulent stage. Simon Flexner designated this as a change from the saprophytic state to the parasitic state and vice versa. He called attention to the fact that the so-called cholera of India was not an epidemic disease until the year 1817. From then until 1875 this virulent epidemic disease ran through four phases which August Hirsch, in his "Handbuch der Historisch-Geographischen Pathologie," Vol. I, p. 278, calls the four cholera pandemics. The second edition of Hirsch's Handbook of Historic Geography was published in 1881, and the English translation by Charles Creighton was made from this second edition, but it was not until 1883 that Robert Koch demonstrated that the cause of Asiatic cholera, first called the *vibrio choleræ Asiatica,* was a bacillus, which on account of its striking curvature received the additional designation, *comma bacillus,* so that in reality a new history of every epidemic disease has to be written from the standpoint of the discovery of its exact cause, but the doctrine of the specificity of pathogenic microörganisms, which is an original idea of Robert Koch, requires historic treatment also in the light of the variability in the pathogenicity of these organisms. (See F. Neufeld, *Deutsch Med. Wochschr,* Jan. 4, 1924, Ueber Veränderlichkeit d. Krankheitserreger.)

An admirable beginning in this direction has been made by a work entitled "The Principles of Human Geography" by Ellsworth Huntington (1924). Among other historically important factors it shows a remarkable relation between the production of the world's most important products and the climatic and racial characteristics of various countries. It also discusses the relation of geographic surroundings to health.

SPHRAGISTICS

This word designates the knowledge that has been acquired concerning the seals which were put upon important documents to protect the contents, or to give them the sanction of a person eminent in government or ecclesiastic power. Sometimes the seal of such documents has been lost entirely, but if marks still show on the parchment where it was affixed, and if mention of its having been attached is made in the document itself, we may assume that at one time an official seal was attached to the document in question. It occurred that kings were not able to write, and therefore the seal increased in esteem, so that even if the signature of the authority was present, the emblem of the seal gained prestige to such an extent that in the period of the Carolingian kings and emperors, it was the seal rather than the signature that decided for the validity of the act. The emblem which is known as the seal was used by the Egyptians and the Babylonians and archeology has delivered up for study not only the seals themselves, but the instruments with which they were made.

In medical historiography this seal knowledge or sphragistics sometimes becomes a necessary feature of a piece of research when the medical historian desires to consult the old libraries in the various capitols of Europe. In our own country the medical diplomas of the early medical schools and even membership certificates of medical societies bore seals which are more a matter of curiosity than of historic interest.

NUMISMATICS—KNOWLEDGE OF COINS AND METAL FORMS OF MONEY

The Code Hammurabi contains statements concerning the fees to be charged by physicians for operations, especially the operation for cataract. The term is *sekel,* and throughout his "Natural History" Pliny speaks of the prices charged by physicians in talents and sesterces. For instance he remarks (Histor. Natur XXIX, 4. 5.), that King Ptolemy paid Erasistratos 100 talents for curing his father, King Antiochus. This Erasistratos was the son-in-law of Aristotle. He also remarked that the body physicians of the first emperors received 250,000 sesterces annually as their salary. One of these physicians, Quintus Stertineius, expresses himself as having made a sacrifice to accept 500,000 sesterces when he could demonstrate that his city practice brought him a revenue of 600,000 sesterces. (500,000 sesterces are calculated to be equal to $25,000.) It is therefore of historic interest to know the value of contemporaneous money in studying medical history.

HISTORIC CRITIQUE—OUTER AND INNER CRITICISM

Historic criticism is the determination of the actuality of the sources and data. It is therefore not that which we ordinarily understand in the English language by the term "criticism." The term comes from the Greek word κριvειν which means to discern. In the use of the term, historiographers have, by common consent, continued to use the word criticism in this sense. There are two forms of criticism, one the outer, and the other the inner criticism. The confirmation of the actuality is the main object of criticism. In this connection I must again emphasize the distinction between facts and testimonies. The word fact comes from the Latin *factum* and means a thing done, an action, a reality, and the word testimony from *testimonium* which means evidence or proof. But in the strict language of the historiographer, the relation between facts and testimonies is the following: All accumulated and collected sources, reports, data, etc., after having been thoroughly tested by those facts which are known to be certain, are designated as testimonies.

The discernment or judgment whether certain data are at all admissible, as testimonies, constitutes the lower or external criticism, and furthermore, the determination of how the evidences and testimonials are related to the facts, constitutes the inner or higher criticism. These two endeavors form the most important procedures in testing the validity of the collected sources. The outer and the inner criticisms have to establish the principles, the rules, and the artifices by which the historiographer is enabled to solve the problems involved. Historic knowledge, certitude, proof of the validity, are based upon principles of psychologic experience, and the principal mental operations in establishing this certitude, are the comparison and combination of the problems to which we are applying the criticism, with certain data of whose actuality and certitude we have already become convinced. In practice, criticism must always have regard for the general connection and correlation of all the facts, and for this reason criticism cannot be separated from interpretation. As a matter of fact, all methods of historiography are here considered separately simply for heuristic or didactic purposes. In practice they are all intimately connected. It is these individual methods that constitute historic technique, the mental equipment of the research worker, and methodic criticism has made historiography a science, for it is only by these methods that truth can be distinguished from error. There are also varying degrees of trustworthiness of statements which have to be decided in this manner. We could classify three degrees of historic data:

(1) Those that are absolutely trustworthy and certain.
(2) Those which are probable, or possible, and
(3) Those which are worthless.

To reach these various degrees we could follow the questions of Bernheim: First, Is the source that which it pretends to be? Second, Is the source really that which it was credited to be hitherto? Negation of No. 1 signifies counterfeit, fraud or forging; negation of No. 2 signifies error.

FALSIFICATION OF MEDICAL DOCUMENTS

A source may not be historic material at all, or at best, if it is historic material, it is not such in the manner it pretends to be. We are dealing here with either complete or partial falsification, i. e., the source is not genuine but forged.

Investigation of genuineness is of far greater importance in historic research than in scientific research, for in other sciences the motives for falsification are not so numerous as in historiography. In other sciences the predominant, probably the only motive for falsification, is covetousness in one form or another, but in historiography the entire gamut of human character weaknesses may come into play. Especially should I mention pseudo-patriotic and pseudo-religious impulses, ambition, the interest of political party, the vanity and disingenuousness of savants who seek distinction by phantom discoveries, the impulse of egotism seeking to support personal hypotheses and opinions by false proofs and sources that are only apparent, not real.

The lower human passions have been recognized as motives such as revenge, wickedness, jealousy, resentment. What Goethe lauds as the "Lust zum Fabulieren" and which he assigns as an inheritance from his mother, is frequently a human tendency that is unnecessarily fostered by the narrating of fairy tales to children, but in persons of certain temperament, it degenerates into an ineradicable habit and taints all of their statements, even to a mature age. In writers with this tendency this quality can be easily recognized by their phantastic accounts, their love of the bizarre, their mystic inclination, and oftentimes by their ill-placed facetiousness. Traits of this kind should disqualify efforts at historiography.

It has been assumed that counterfeits and falsifications played no rôle in medical historiography on the supposition that medical research works with material that has already been tested, examined, criticized by philologists, archaeologists, theologists, geographers, and general historians.

It must be evident on second thought, however, that all such experts, investigating a problem from their special and personal viewpoint, may end their research at a point where it appears concluded to them and yet it may not be concluded for the medical historiographer, for they may have committed errors themselves, or authorities and experts of fairly equal renown may contradict each other on essential fundamentals, or an authority of acknowledged scientific ability and even international fame may omit to read original sources, become satisfied with transmitted or quoted information, to lighten his work, to save time, or he may be under the ban of an inescapable patriotism at a historic time when his country can no longer endure being deprived of the honor of a particular medical discovery, because his country has been deprived of almost everything else that makes life worth living.

In studying medical documents of the Middle Ages, one may occasionally have to decide whether the manuscript is genuine or not. Here we first apply the rule of palaeography and those of diplomatics. The handwriting of almost every period in Asiatic and European history has been so thoroughly studied that it is now possible to identify the period in which a certain manuscript was written. The forms of letters used at various times and in various countries are found described in the works on palaegraphy and diplomatics. Even the materials employed in the manuscript, the ink, the substance of which the document or the relic is made, the seal, the container in which the document was preserved, and all other external facts have been so exhaustively studied and classified, that the expert can determine with considerable assurance, the source of a written document. Fortunately in the medical manuscripts, forgeries and falsifications have been rare. The difficulty of discovery increases in proportion to the proximity of the forgery to the time and place of the alleged origin of the suspicious document, for the counterfeiter is then able to enter more fully into the spirit of the period. The invention of printing did not destroy the possibility of fraud in manuscripts and documents, for there are many documents of former times which remained permanently in the written state (see Bernheim l. c. p. 331–391.) This great teacher of the historic method also has a number of pages devoted to hypercriticism by which perfectly genuine sources have been pronounced as false by inadequate critical methods. If medical documents come into question in historiography, we will have to place before ourselves four questions:

1. The time or period—when did this source arise?
2. The place and locality—where did this source originate?
3. The author?
4. Is this source an independent original source, or a derived source?

According to Sudhoff there is no fundamental difference between the methods used in the history of the natural sciences and those employed for medical historiography. This refers to methods only. The distinctions between the two sciences I have emphasized previously as well as the analogy. Both have to follow the strict method of proof of facts and data with all possible means that are obtainable and in medical historiography the observations have to be arranged in logical order as conscientiously as in all other biological investigations.

Sudhoff emphasizes that the testing of the historic material requires a control of all auxiliary sciences which have been mentioned in the preceding; in particular palaeography is indispensable when the chirography of languages must be read in older works and letters, especially up to the time in which it became customary to give ones work to press and to read the proofs personally. For letters and documents, palaeography is indispensable even after the invention of the printing press, especially for the 16th and 17th Centuries and also for the original manuscripts of works on certain medical subjects, if such can be obtained.

Sudhoff's views expressed to me personally in many letters concerning Diplomatics and Sphragistics are those which I have previously expressed.

For the determination of medical texts that were written in times before the invention of printing and even extending into the periods of the early printed editions, the entire philologic method must be controlled for every serious study of the sources. Even when old works are required to be reprinted this method is necessary. The central part of the total history of natural science, especially in its relation to medical science is the investigation of the numerous doctrines in their development and connection with related disciplines; that means, the entire science of medicine in association with natural science and contemporaneous philosophy. Of equal importance is the psychologic investigation of the personality of the investigators. The psychographic (not biographic) study of great Masters in Medicine extends far into the realms of psychology—as can be ascertained in Chapt. XXXI.

To the lover of medical biography it becomes evident that a faithful and scientific presentation of the lives and works of the Master Minds in Medicine necessitates a cultural comprehension of the entire historic period in which these great personalities lived. These men can only be understood in the perspective of the social, economic, religious, and caste life of the times in which they lived. In short, the entire conditions of life during the epoch in which a great man lived, must be lived over again psychically and intellectually by the sympathetic historiographer. The belletristic literature and the art of his time constitute a further realm of conditions that must not be overlooked if he wishes to understand these masters thoroughly.

The investigation of the life of the past from every feasible aspect transposes us into the situation in which we become at home in any definite historic period, to such a degree, that we are actually enabled to live over again the events of those periods and to understand what transpired in the minds of those pathfinders and professional brethren of note who lived in days long gone by. After all, it is more important to develop a sincerely conformative and ingenuous understanding of a period than to gain a judgment or evaluation of the thinking and the work of that period. For the sympathetic understanding comes from an altruistic love and is a surer incentive to good historiographic work than the severe reasoning executed without regard to historic understanding. The historiophilist is more certain to find the correct way than the historiosophist.

SYNTHETIC RECONSTRUCTION, INTERPRETATION AND CONCEPTION

After the historic research has yielded all the deductions that are possible, these results require conformation into a living presentation. The facts in themselves are simply the individual stones of a mosaic; in order to set these stones into a picture capable of easy perceptibility certain new principles of historiography demand consideration. These are the artistic exposition and composition which Seignobos has designated as *synthèse historique*. At this point the master of English has his say. That a man must have had a good training in English composition is self-evident; without such a preliminary experience he cannot succeed in bringing the results of a very laborious historic research into comprehensive form. One of the best ways to acquire linguistic form in historiography is to study the writings of scholars in history with which the English language is already blest. The style and methods of exposition of Macaulay, Carlyle and Gibbon cannot fail to leave an impress upon the sympathetic reader.

The historic conciseness, succinctness and clearness of such masters has a contagious quality in it which would not leave any sincere student of history untouched. Among our medical historiographers in America the linguistic style of Welch, Packard, Garrison, Streeter, and James J. Walsh are much to be recommended for this purpose; and in England, the "Studies in the History of Method of Science," edited by Charles Singer.

PRESENTATION AND EXPOSITION

After the sources have all been collected and their validity proven, their actually confirmed, and after the results of this investigation have been sifted by historic criticism, and thirdly, after the ideas and convictions, gained from the tested sources and selected facts, have been reconstructed into conceptions, then comes the final endeavor of the historiographer to present his convictions in such words and sentences that the reader or listener can readily enter into the meaning of his ideas. Some of the great historiographers that I have mentioned in the preceding part of this article, were possessed of such rare gifts of exposition, that the force of their historiographic logic was irresistibly compelling.

The inexperienced writer who enters into historiography without the preliminary steps just briefly outlined sometimes imagines that when the stage of presentation and exposition has been reached, all difficulties have been overcome, that further errors are now impossible, and all that remains to be done is to sum up the conclusion in simple language. But the notebooks and unpublished letters of great German and English historiographers show that essays which have come down to us from them in such linguistic perfection that not an iota can be added or subtracted were preceded by one or more sketches in which the thought mass was evidenced in the minds of these great historians as a complex interpenetration of ideas, so much so, that with some of the historians two or three preliminary sketches were necessary before the final form of composition was decided upon by the writer. This is a similar mental dystochia as has been observed in the productions of great musical composers. The cause of this mental embarrassment lies in many differing operations of the mind. In the first place, individual historiographers vary with regard to the vividness of fantasy. Others have difficulty with memory. I have personally studied two scholarly friends, who observed the same event and who were endeavoring to be equally faithful, to present what they had seen and heard, and yet when their communications were presented in the final conception, it was found that the inner psychic reproduction of what they had experienced had undergone transformations in each individual, and in each one the transformation was different. It is a psychologic fact that we are incapable of embodying all factors and moments of an event or process into a distinct conception of our mind.

There is generally a selection of the integral time and place factors of an event. This selection is at times conscious, at others unconscious. In most individuals there is also a concentration of the historic facts of an event in the mind, and if a historic process is very rich in single constituent factors, then the possible selection of factors must naturally vary within wide limits. Thus great differences may develop between the observation and perception of an event and the final conception and presentation of the same event.

Historic sources are roughly divided into (1) remnants, and (2) tradition. Remnants are everything that is immediately transmitted concerning events. Thus remnants refers to those things which are still left over and objectively present of a past event. But everything that has passed through human conception or interpretation we define as tradition. Now all presentation and exposition which rests upon tradition has passed through the process which we have above referred to as concentration of conception, and therefore at every stage of handing down by tradition, a certain modification is effected, consciously or unconsciously. This process unavoidably produces some clouding of the deduction which however can very frequently be recognized by the character of the presentation. The process of mental concentration in treating historic deductions is by no means a fault. On the contrary, when this process is consciously conducted with clearness and fidelity, it may yield a very high degree of convincing presentation.

The accuracy and fidelity of personal memory is an influential factor in the artistic reproduction of the results of source analysis and historic criticism, but very few historians can rely exclusively upon their memory, most of the more famous historic writers having been observed to be constant note-takers, and making use of every artifice to facilitate memory.

REPRODUCTION OF HISTORIC CONCEPTS IN TERMS OF LANGUAGE

ANALYTIC AND SYNOPTIC METHOD

The fruits of our labors in applying the rules of historiography for the elucidation of any event or development in medical history, will in the majority of cases have revealed themselves as a set of isolated and disconnected facts which when viewed, each one singly, may create a lively interest because of the joy resulting from research and in having found things which we did not know of when we started the investigation, but the isolated facts per se do not constitute the complex events or the discovery or the doctrine which we started to study. In order to understand this thing in its totality, we are compelled to join these clearly established facts again and endeavor to comprehend the thing we originally started from, by reëstablishing its complete historic entity from the individual analysis which our study has yielded in a now clarified form. Thus we have what could be termed the analytic tendency in medical historiography and the synoptic tendency. The analytic tendency is made necessary because it is impossible for the human mind to fully understand complex events in their totality as they first present themselves. Too many writers of history take the validity and correctness of complex events for granted, only to find out later that a great deal that was accepted in good faith, later revealed itself to be untrustworthy, or at least requiring further proof. The view that takes in the totality of things or the synoptic view is called by the French, *vue d'ensemble,* by the Germans, *die Gesammt-Anschauung,* and this synoptic view of medical history is complementary to and as a rule follows the combined methods of analysis and synthesis which were described in the preceding chapters of this article.

The student of general history as well as of medical history, when hearing or reading of synthetic and analytic methods, at once conceives them not only to be two different processes of reasoning, but that they are actually diagonally opposed to each other. Such an idea of these two methodical processes in the study of history would be misleading for I have endeavored to use them in the sense that Plato uses these terms. To him they do not really mean two separate methods, but related aspects of one and the same method. It seems to me they are never really separate methods in scientific investigation. Let us take for example chemical analysis and synthesis. The technical chemist who is engaged in either one of these processes rarely is occupied with the one to the exclusion of the other, but always with the expectancy of testing the results of one method by the results of the other. In other words, the two methods are complementary to each other, though they are capable of differing emphasis, and in medical historiography, especially when we are dealing with the history of chemistry, these two methods while borne in mind simultaneously, are differently emphasized in different minds. Among historiographers, also, we find minds that are more bent on discovering identities, while others are disposed to singling out differences. This distinction between analytic and synoptic views of history has to be borne in mind by prospective historiographers. It would be well to ask ourselves which of these two different, yet identical, methods we desire to emphasize. Perhaps we could discover a natural trend in ourselves and in following this trend we would probably reach the best results because we are doing that which suits our natural mental disposition.

To discover one's natural trend of expression in effecting the synoptic view of historical events, characters or documents, we are following the path of least resistance. We do the work with least consumption of mental energy. We find great pleasure and love in doing the work because of the consciousness that it is done with ease and facility. Therefore the endeavor to find one's natural method of expression of the facts, truths and deductions of historic investigation is one of the great aids of writing history. As Cicero says, "Id maxime quemque decet, quod est quiusque suum maxime."

"That best becomes every man, which is more particularly his own (at which he is *best*)."

It is a well known fact of psychology that the expression in words of our conceptions does not always exactly correspond to these conceptions. Now history, and especially medical history, is a connected series of conceptions, and all of our conceptions and all of our knowledge is made up of ideas. The psychologists speak of such ideas as presentations. Locke, Berkley, Hume, and Kant had a great deal to say concerning the nature of ideas, but the fact that there is a difference between the idea and its best principal expression in words is not so frequently dwelt upon, and because there is a difference between human aspect of things, conceptions, and ideas, of those that describe and report history, and the very expression of such conceptions and ideas, therefore a separate element of historiography became necessary, and that was *the interpretation of the tested sources after they have been subjected to the critical method.* The modifications of the expression in words by various authors appears to necessitate such an interpretation. It may happen in the course of historic research that the worker becomes aware of an evident incongruity, even inconsistency, between the historic event as he himself has traced it, let us say, from old sources, and the presentation and description of it by a recent historiographer. Such incongruities are, as a rule, more or less involuntary, but under certain conditions they reveal themselves also as voluntary, and then they lead to considerable clouding of the event with which they deal. In expressing the results of our historic research in words, we must endeavor to use the simplest expressions that are possible. In order to relieve a long series of purely methodical principles and doctrines, an author may be pardoned for indulging once in a long while in the expression of a familiar quotation, or taking the privilege of an anology in the form of an anecdote. But in the long run, the maxim of the great philosopher Seneca is a safe guide, *Veritatis simplex oratio est* (the language of truth is simple).

Every historic statement concerning a complex event involves two mental operations: first, a concentration of the conceptions, and secondly, a definite selection among them. The historian's expression of a medical event, personality, doctrine or discovery, etc., will vary with regard to three factors which are likely to be uppermost in his mind, and which will probably determine the manner in which he presents his research. Before I state these three guiding factors I wish to emphasize that they need not necessarily be the evidence of a preconceived opinion in the mind of the historian, for frequently they do not reveal themselves until he is well under way with the summation of his results in written or expressed words. These factors are:

1. *The theme.* What does the writer consider most worthy of publication?

2. *The form of his communication.* The more definite the form of expression, the more decidedly will it influence the selection among the facts. Some forms for instance are, as in the present book, principally biographical. Other forms are those of historic chronicles. Still others are official annals. It is evident that the selection of the historical material will vary according to these three forms of historiography.

3. *The individual viewpoint or standpoint of the historian.* This applies to the inner participation and the personal interest of the person who writes history, for or against certain moments and factors in the historic material which he has to record. The personal standpoint, then, of the author may lead to the suppression of some aspects or factors in an event, biography, discovery, etc., while other things are emphasized too strongly, or exaggerated in one direction or another, or minimized in one direction or another. I have spoken of some of these defects of presentation in my psychography and character study of Leonardo da Vinci, *Medical Life,* February, 1924. In this article the defects and incongruities of Freud's psychoanalysis of Leonardo da Vinci are subjected to critical investigation. The personal interest of an author may increase so much that it becomes evident to the point of partiality, and the highest degrees of partiality reveal themselves as *tendencies.* This means that the author is motivated by a definite purpose in his communication, which motive is outside of the scientific purpose of his report.

The liability to disfiguration of historic reports increases with the frequency of their repeated narration because every new reproduction of an historic event opens up all of the causes of misrepresentation that we have dwelt upon. I do not wish to say that these causes actually do cloud the narration anew every time it is repeated, but only that there is a liability of this disfiguring factor. The fidelity of the narration depends very much upon the individuality of the narrator, so much so that Lorenz maintains that it was possible to determine the validity and the dependability of a historic report if we knew the personality and character of the reporter. While this is going a little too far, it emphasizes the importance of this factor.

The care with which a writer distinguishes between immediate and mediate accounts of events is an indication of his exactness and fidelity. The immediate reports are those obtained without intervention or adjustment directly from the original sources. The mediate reports are those based upon the qualification of intervening reports. They are the deduced sources. It is therefore of the utmost importance in historic methodology that in every tradition, report, or transmission of historic facts the investigator should go back to the oldest obtainable form bearing upon the event in question. As the literature of the Old and New Testaments is rich in the descriptions of the earliest forms of medicine practiced in the various periods of time of which these venerable historic annals treat, it is not only highly instructive for historiographers, but a valuable aid in the methods of historiography to study the various modifications which ancient events have undergone by repeated narration and rewriting. E. Kautzsch ("Die Heilige Schrift des Alten Testaments") states in the preface to his first volume, that the historiographer has to translate what there is of tradition independently of the probability that this tradition in many cases does not offer that which was originally true. He cautions, however, that we are not always compelled to choose between tradition and willful changes, but that the comparison of parallel reports or the oldest translations, for instance the Greek from the Hebrew, and from the evidences of palaeography, may give us a definite certitude of the correctness of our reports. One cannot fail to be impressed with the deductions of Kautzsch and his collaborators, who have given us much of the Old Testament medicine that is absolutely dependable, and his works show that the historiographers of those remote periods when the Old Testament was put for the first time in united written form (Ezra, Nehemiah, 444 B. C.) were men who, although working with a religious tendency, nevertheless recorded such facts as came to their knowledge with as great a fidelity as could be expected from human beings at those remote periods, and under such political and social conditions as they lived in.

The form, the theme, the interpretation, and the individual attitude of the writer will determine those facts which he will select for his final statement. The literary form of the presentation which is selected has an influence upon the condensation and concentration of the historic fact-material. As medicine is the product of the brain work of individuals, we are interested in this work mostly in the graphic depiction of personalities, and therefore that which is personal will be brought into the highest light.

When a lover of research in medical history impelled by an intense interest and a fanatical love of truth has eventually reached a point in his investigation of a certain historic personality, medical doctrine or discovery, course, source and geographic distribution of an epidemic, etc., etc., he may find at the conclusion of all his methodical labors that he is confronted with a bewildering accumulation of facts and data. The first thing to do, then, is to separate this summation of facts and data into two groups: *first,* those which are of the greatest importance for the depiction of the personality, or for the connections in the development of a doctrine or a discovery—these he will produce in his literary composition with the greatest punctiliousness, explicitness and completeness; *secondly,* those which are of minor importance, and unessential, he will subject to the greatest condensation, no matter how interesting and enticing it may be to reproduce them in greater fullness. The distinction between these two sets of facts and data depends to a large extent upon the training and individuality of the historiographer. In this effort it will become manifest to what degree he controlled the art and science of historiography.

ARRANGEMENTS IN ORDER OF TIME AND PLACE

The reproduction of the fruits of historiographic research in an accepted literary form requires a certain facility and experience in *the disposition* of the historic results and, also in *the composition* of the historic presentation.

Disposition means the arrangement of far-reaching and many branched historic connections in such a way that the most important facts stand out so prominently that their significance and meaning is readily understood. In order to accomplish this, we must avoid being bound slavishly by the ordering and arranging of facts in time and place. Chronologic arrangement of historic facts is very helpful in histories that serve for school purposes where the genetic development and inter-relation of historic events is not dwelt upon, but if we are depicting the life of a great medical personality, it is our main purpose to demonstrate him as the instrument of some great progress, as the human power by which some great constructive idea is brought into practical effect. Now this work of a great human instrument may occur at many different places and many different times. We may therefore be compelled to desert the chronologic order in order to do justice to the connection of the events which this superlative human power is bringing about in forcing the execution of the great constructive idea, and if there are many branches of human endeavor and conditions, we may have to leave aside the assignment of space, place, and geographic location, for a time at least, in order to do justice to the continuity of the great work that this great personality is accomplishing.

The desertion of the human ordering of things and events according to time and place becomes sometimes necessary as a sacrifice for the better perception of great constructive ideas. Moreover, the purely chronologic and spacial arrangement of events and facts cannot even be adhered to in the tables and annals of general history, much less of medical history. In every arrangement and presentation that wishes to elevate itself above the purely material, and redacting, plane of historiography, the inner connection of the events represents what is genuinely scientific in the presentation. This is a most essential part of all historiography, and in all such endeavors time and space arrangement have to be subordinated to clearness in depicting the evolution of great constructive ideas. It is here where the so-called *"disposition"* according to Bernheim, plays the same rôle as (what painters call) *"perspective"* in painting. Disposition like perspective brings to our comprehension that which is spacially before or behind, that which is near and that which is distant in relation to a definite center of activity. Both are concerned in making comprehension easy by an artificial arrangement of conditions of depth and perspective, on one surface. The historic disposition of material, then, is an artificial arrangement for the purpose of rendering activities, facts, and events following each other in different times readily comprehensible, as integral parts of one continuous process.

In writing biographies, we must avoid the time tabulation of the deeds and experiences of the man whom we are depicting. Chronologic moments demand consideration in the principle phases of the education and development of the person, but here also they have to be relegated to a subordinate position when it comes to making clear the activities, discoveries, great constructive new thoughts of a man, and his relation to his predecessors, contemporary people, and posterity. In the biographies which follow in this work, there was great temptation to enter sometimes into the intensely fascinating political environment of such men as Leonardo da Vinci, Lavoisier, Servetus, William Harvey, William Beaumont, etc., for they lived in periods of tremendous political and national transformations. But if I had yielded to the temptation of the political and national events that transpired in the lives of these men, the instrumentality of their great personalities, the momentous advances which they enacted, and the great constructive ideas for which they stood would have become submerged beneath the tidal waves of historic political disturbances. It has sometimes happened that the political upheaval in which such a great man lived and which had produced revolutions that destroyed hundreds of thousands of human lives, passed away without effecting as much good for humanity in general as one single thought of the great man has effected in producing betterment in the living conditions of posterity.

The great personality in the history of medicine should be made to appear upon the stage of general or political events, and to speak only when he can influence the progress of human development in a determining manner. He may be not visible on the political stage at other times, yet the reader of his history, like the spectator of a drama, thinks of him as acting even though he is not visibly effective, just like the audience thinks of an actor as influencing the progress of a drama, though he may not always be upon the stage. Carlyle, Macaulay, and Gibbon are masters in depicting great personalities in this manner. Schiller, in his history of the Thirty Years' War, von Ranke, and our American Rhodes, demonstrate a special talent for this dramatic historiography. In fact it may be considered an especially fine historic disposition of facts when a dramatic effect is produced, and one that does not produce this effect is, as a rule, confusing. Such an erroneous disposition of historic events and data is seen in all history writing that is written through the motive of patriotic interest. The patriotic historian is only too prone to make his own country the center of historic events, irrespective of the evidences pro and con as to whether his own native country has always had the central importance in the development of the events which his individual disposition assigns to it.

COMPOSITION

In the preceding the author has repeatedly referred to examples which in his opinion constitute good historiography. They have as a rule been taken from the classic standard authors of all nations that have distinguished themselves in human culture. In the literature concerning historiography, one meets very frequently with the idea that history writing consists of the artistic presentation of historic material in the form of narrative prose. As axiomatic as this would seem upon the first reading, I wish to emphasize that this cannot be conceived to be the duty and problem of the scientific historiographer. In previous paragraphs I have endeavored to distinguish between three kinds of history writing: first the simple *narrative,* or recording (feuilleton); secondly, the *pragmatic;* and thirdly, the *genetic* historiography. I also endeavored to illustrate how progress in our understanding of the activities of human beings as social entities can only come from the third form of historiography. If the demand is made that historic work shall above all be artistic without satisfying the genetic and scientific requirements, then it is impossible to be anything but a narrator or recorder.

The conjunction of artistic literary style with scientific genetic historiography is so difficult that one almost precludes the other, for when a faithful research worker applies the necessary concentration to his composition, the manifoldness of the perspectives is so great that the attention is unconsciously focused on the historic material and away from any thought of artistic literary presentation. In his "History of the Roman and Germanic Peoples," Ranke states (*Sämmtliche Werke,* Bd. XXXIII, Preface, p. 7): "One should not demand that free development of a history which theory seeks in a poetical way. Objective presentation of the facts, no matter how conditioned, plain and unadorned though it may be, is the uppermost law without doubt." And in the XXXVth vol., p. 89, he states: "It is not only the connection of the entirety (of historic events) at which we aim, for it is not through this alone that history wins our sympathy. Man should fix his vision with intense curiosity upon single factors, fortunate if he succeeds in comprehending the things in the foundation of their being, and in the fullness of their peculiar appearence."

It is evident that such guiding lines of historic research will require all the personal gifts, patience, and artistic literary expression of a historian, and leave no room for special efforts at polish in style and linguistic adornment.[3] In the composition the writer has to have regard for a suitable manipulation and condensation of the facts. Good composition brings to understanding the principle moments of an event, character, condition, personality, unobscured by unessential detail. Bad composition is characterized by defective and unequal condensation of facts, so that we are led into such detail of unessential or accessory things, that we miss the thread of the connection and we do not recognize that which ought to be important. We fail to understand the essential single things in their significance for the totality of historic factors.

The best way to learn effective historiographic composition is to study three or four talented historians in their description of the identical event, personality, or doctrine, or whatever the historic point of inquiry may be. As an example of successful union of strictly scientific and aesthetic attractive presentation, Bernheim recommends the "History of the City of Rome in the Middle Ages" by Ferdinand Gregorovius. He even points to the artistic means with which this historiographer overcomes the severely rigid historical material of that period. As a sample of historiography with a pronounced tendency, I may quote Macaulay's essay on Frederick the Great. Both of these illustrations, however, are taken from general history. In order to give illustrations from medical historiography, I may refer the reader to the accounts given concerning the discovery of surgical ether anesthesia by Welch, on p. 221, Vol. III, of "Papers and Addresses," and the same subject as presented by Garrison in his "History of Medicine," and by Roswell Park in his "Epitome of the History of Medicine," pp. 300–312. Or I would recommend the disposition and composition concerning the work of William Beaumont on the Physiology of Digestion as presented by three different writers, Osler, Jesse S. Myer, and Hemmeter, or the manner of presentation and the historic composition of the work of the U. S. A. Yellow Fever Commission (Major Walter Reed, James Carroll, and Jesse Lazear), also in Vol. III of "Collected Papers and Addresses" by William H. Welch (p. 387, Howard A. Kelly and Hemmeter). These three subjects, each presented by three different writers, are recommended because they are taken from specifically American medical history and written by American authors, and therefore will probably appeal to American physicians and surgeons. There are numerous incidences of the treatment of the same medical historical subject by three or more authors in other countries which are equally instructive. We find them in the medical literature of Great Britain, Germany, France, Spain, and Italy. Thus for example the history of the discovery of the circulation of the blood has its most important literary studies in the Italian language, and after Harvey had completed his epoch-making final physiological verification by faultless experimental methods, even then the medical literature of almost all European countries presented articles by many authors of high repute concerning this subject. One need only look up the subject of circulation of the blood in the index catalogue of the library of the Surgeon-General in Washington to become convinced of this, but such an extensive literature and multiplicity of literary references cannot serve the heuristic purpose of historiography. It is far better that a few definite articles be singled out where not more than three authors have written concerning the same subject. By this restricted literature the student of historiography will be served in a far more practical manner than by too profuse a bibliography.

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[3] La vraie science est l'ennemie de grands motes.

THE COMBINATORY AND REPRODUCTIVE SUPPLEMENTING OF MOTIVES.—THE ANALYTIC AND SYNOPTIC ASPECT OF MEDICAL HISTORY

Medical historiography cannot be learnt or investigated in laboratories. It has to do with the activities of human beings as individuals and in masses, activities not only in a physiologic or biologic sense, but also in a sociologic sense, meaning the manner and the degree to which medical thinkers have become the instruments for the origination or the wide application of some great constructive idea in medicine. Whilst historiography has no laboratory except the records and sources, etc., already considered, yet the endeavor must be to train the coming historiographer more and more into the methods of modern psychology, the modern statistical methods, and even the methods of mathematics and bio-chemistry. Such methods will eventually construct the laboratory for historiography comparable to a dissecting room, for the analytic research work concerning historic sources. But just as it would be impossible to understand the operations and functions of the human body from a dissection or the pharmacologic effect of a complex organic compound from a chemic analysis reducing it to its elements, so it would be equally impossible to judge a historic event from the ultimate results of the laboratory or dissecting room method. These methods are absolutely indispensable for the verification of the sources and other historic factors, as we assumed before, but the results that are obtained have to be reconstructed again, in order to obtain a living representation of what happened in the times that we are studying. In no domain of human knowledge would it be more destructive to a correct conception of the human transactions of any period or the medical doctrines and discoveries of any period, than to assume that medical history is statical, exhibiting an unvarying rigid form for any definite period. Medical history is a continuous process. It is never a status or a condition for a very long time. We must therefore regard it as a great advance if historiographers of the type of Sudhoff, Neuburger, Pagel, Garrison, Welch, Singer, Richet, Streeter, Wickersheimer, Luciani, and others have infused a revivifying spirit into the body of medical history so that we may see the past rise anew before our mental vision.

In my article on Darwin it will become apparent that one of the great influences of Darwinism upon biology has been to lead the way out of the laboratory, the dissecting room, and the museum into the wide, wide world and expanse of living nature. He led us once more to the study of animals and plants, not by the artificial concepts and rigid positions gained inside of institutions, but in their living reality and in their actual normal environments. We were led to understand living things in their continuous, dynamic function and not in statical, artificial systems. This is what the newer historiography aims at. It abhors artificial schemas and systems. It uses the methods, described in the preceding, simply as verifications of the truth, not as the real end-object of historiography itself. The human mind is so constituted that it cannot often grasp the synoptic view (*Gesammtanschauung—vue d'Ensemble*) without a previous analysis and differentiation of the factors. The study of individual things is thereby facilitated by bringing them under general principles and formulæ. By doing this we may acquire skill to look upon historic phenomena as partial examples of a great general process. This appears to be the only way in which the human mind is able to grasp a great mass of detail, and make its knowledge applicable to individual cases and for all practical purposes. It affords gratification to be able to perceive in those historic factors, which are novel and unknown, traits of that which is familiar and known. It is also a delight of the historiographer to find himself in the possession of a leading idea or guiding principle by the instrumentality of which unknown periods may be investigated and new visions of old historic phenomena be revealed. Lamprecht designates this as the *diapason* in his essay in *Zeitschrift für Kulturgeschichte* (1898). It means the all-pervading symphonic harmony of historic evolution, whether by masses or individuals.

In medical history some of these guiding principles have sprung from anatomy. Men saw the architecture of human and animal organs, for instance of the heart or the lungs, and meditated upon the probable work those organs performed in the living body. Here we have a guiding principle, beginning in structure and leading to function.

The idea of schemas and systems in medical history will be considered in the chapter on medical discoveries and discoverers. The object is at present to emphasize the fascination which attaches itself to such guiding principles and thought-instrumentalities. There is, however, a great danger of over-estimating such guiding principles and employing them as a kind of master key which would unlock every mystery of medical history. The very diversity and individuality of human beings would indicate that general systems, schemas, and guiding principles could have only a very limited application. Whenever we arrive at the end of our investigation along the line of such a guiding principle, we find ourselves in the possession of a few isolated facts, if we are fortunate enough, but these have to be recombined in order to arrive at the totality of historic truth. It is here that the combinatory talent and the methodic, reproductive, supplementing of the historiographer will have its telling effect.

Three thinkers of the very first order have given expression, quite independently of each other, to this reflection. The first is Goethe, who frequently dwells on the subject, but most explicitly in a conversation with Eckermann in the year 1824: "As soon as one belongs in science to a narrow confession, the unbiassed truthful view is at once gone. The decided Vulcanist will always see things through the eyeglass of the Vulcanist, in the same way as the Neptunist and the adherent of the recent 'elevation' theory will do so only through his own. The view of all such theorists, who are obsessed by a special exclusive aspect, has lost its innocence, and objects no more appear in their natural purity. Whenever such students give an account of their observations, we do not, in spite of the greatest personal veracity, receive in any way the truth of the object; we receive things, ever, only with the taste of a strong subjective flavouring. But I am far from maintaining that an unbiassed, correct knowledge stands in the way of observation; rather the old truth stands, that we really have only eyes and ears for what we know. The professional musician hears, in the concord of the orchestra, every instrument and every single note; whereas an outsider remains embarrassed through the massive action of the whole. So also the man, who merely enjoys himself, sees only the graceful surface of a green and flowery meadow, whereas the eye of the observing botanist is struck by the infinite detail of the most various single plants and grasses. . . . In science we meet persons who, by dint of too much erudition and hypothesis, never get back to seeing and hearing. With them everything runs rapidly inside; they are so much occupied with what they are pondering, that it happens to them as to a man in a passion who passes his nearest friends on the road without seeing them. Observation of nature requires a certain quiet purity of the inner self which is disturbed by nothing, nor preoccupied. . . . Would to God that we were all no more than good laborers! Just because we want to be more, and carry about with us a large apparatus of philosophy and hypotheses, we spoil things."

The second is Ruskin, who, at the end of the third volume of "Modern Painters" makes the same reflection, though he applies it somewhat differently: "This comparative dimness and untraceableness of the thoughts, which are the source of our admiration, is not a fault in the thoughts at such a time. It is, on the contrary, a necessary condition of their subordination to the pleasure of Sight. If the thoughts were more distinct we should not see so well; and beginning definitely to think we must comparatively cease to see. . . . It is evident that a curiously balanced condition of the powers of mind is necessary to induce full admiration of any natural scene. Let those powers be themselves inert, and the mind vacant of knowledge and destitute of sensibility, and the external object becomes little more to us than it is to birds or insects; we fall into the temper of the clown. On the other hand, let the reasoning powers be shrewd in excess, the knowledge vast, or sensibility intense, and it will go hard but that the visible object will suggest so much that it shall be soon itself forgotten, or become at the utmost merely a kind of keythought!"

The third important contribution and confirmation of this reflection is to be found in Charles Darwin's own account of the development of his mind in the course of his life and through the influence of his studies: "In one respect my mind has changed during the last twenty or thirty years. Up to the age of thirty or beyond it, poetry of many kinds, such as the works of Milton, Gray, Byron, Wordsworth, Coleridge, and Shelley, gave me great pleasure. . . . Formerly pictures gave me considerable, and music very great, delight. But now for many years I cannot endure to read a line of poetry. . . . I have also almost lost my taste for pictures or music. . . . I retain some taste for fine scenery, but it does not cause me the exquisite delight which it formerly did. . . . This curious and lamentable loss of the higher æsthetic tastes is all the odder, as books on history, biographies, and travels, and essays on all sorts of subjects, interest me as much as ever they did. My mind seems to have become a kind of machine for grinding general laws out of large collections of facts, etc." ("Life and Letters of Charles Darwin," 1st ed., vol. i, p. 100). In the fact of this self-depreciation, this honest and modest confession, we may safely maintain that only through the early delight in nature, the intimate communion with her and the breadth of observation, did Darwin's mind succeed in fastening upon some undiscovered features of her life as a whole: as he indeed has done more to cultivate and encourage the *vue d'ensemble,* the synoptic view of nature, and to counteract the purely analytic and synthetic methods of the earlier natural sciences, than any other naturalist in recent times. And having, from the deep source which the love of nature revealed to him, drawn such a full and overflowing measure, we may feel thankful that he spent his life in dispensing the same for the benefit of science and the delight of his many followers and admirers.

It would be transgressing the limits of the present section of this history to do more than merely hint at this uniting link of the scientific and the poetical aspects of nature. The instances of poetical or artistic minds of a high order being at the same time naturalists and clinicians, in the stricter sense of the word, are not frequent, though they exist. Not to mention Leonardo da Vinci, who belongs to a much earlier age, we have, in the eighteenth century, Albrecht von Haller in Germany, Goethe in the beginning, and Ruskin in the middle of the nineteenth century. In the United States we have Oliver Wendell Holmes and S. Weir Mitchell. On the other hand many of the foremost naturalists of the earlier school, like

Buffon in France and Alexander von Humboldt in Germany, Agassiz in America and an increasing number in more recent times, have displayed not only a scientific but a very keen artistic appreciation of natural objects and of nature as a whole. As quite recent instances I may mention Sir Archibald Geikie and Prof. J. Arthur Thompson of Aberdeen. No doubt my readers will think of many other examples of the close alliance between the poetical and the scientific love of nature. But Goethe stands out as a unique instance of the immediate influence of poetry, both on science and on philosophy. He clearly pointed out that Sight, and not Thought, represents the beginning and first stage both for the poet and the naturalist. The German language has for this a distinctive term in the word *Anschauung* which, although partaking of the meaning of *intuition,* does not necessarily include the mystical element implied by the latter term. It is interesting to see how, probably without much knowledge of German or any acquaintance with Goethe's frequent discourses on this subject, Ruskin, in the 3rd vol. of "Modern Painters" (1st ed., p. 288), uses the word *sight* in exactly the same meaning as belongs to the German word *Anschauung.* As in this sense it has not become current in psychological discussions, Merz has adopted the word *synopsis,* denoting by this term "the power of fully perceiving any natural object." It "depends on our being able to group and fasten all our fancies about it as a center, making a garland of thoughts for it, in which each separate thought is subdued and shortened of its own strength in order to fit it for harmony with others, —the intensity of our enjoyment of the object depending first on its own beauty, and then on the richness of the garland. And men who have this habit of clustering and harmonizing their thoughts are a little too apt to look scornfully upon the harder workers who tear the bouquet to pieces to examine the stems" (Ruskin).[4]

OBJECTIVITY AND SUBJECTIVITY OF MEDICAL HISTORY WRITING

Self-knowledge is one of the most indispensable conditions for a historiographer. What do I know about myself, as far as my entire life's revelation opening up my inner being is concerned? Am I a realist, or an idealist, an optimist, or a pessimist? Am I very subjective, or objectively disposed? Am I religiously inclined, or an atheist? Am I a strict partisan in politics, or an independent who judges the evidences pro and con? Have the trials of my life matured me sufficiently to form judicial conceptions, even concerning those things which at first sight seem opposed to my principles? About all of these inner aspects, a historian must be clear in his own mind. If I am a realist, I must strive to give due credit to the ideal impulses in the actions of the persons that I am depicting. If I am an idealist, I must not presuppose ideal motives everywhere, but must endeavor to hunt up the realistic, practical, and egotistic motives in the actions of personalities I am studying. This is a work on the "Master Minds in Medicine." It can be seen that in some biographies of medical men that are not formed according to a methodology, there is a subjective one-sidedness which occasionally leads to the apparent ignoring of facts that are established by sources or documents, and which are undoubtedly the true characteristics of the personality they assume to depict, their temporary omission giving the entire conduct of life of the great personality a halo which is directly denied by indubitable expressions and witnessed intentions of the individual in question. This matter has been discussed toward the end of the article on Leonardo da Vinci, which is a character study (Chapter XXV) in which I answer the constructions of Freud concerning the defects of biography.

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[4] Quoted from John Theodore Merz, "A History of European Thought in the Nineteenth Century," Vol. III, pp. 608–609, 612.

A long training in historiography will bring it about that the scholar will eventually believe in emotions, feelings, and impulses which he does not find in himself, and thereby the unedifying representation of great men that Bourdeau indulges in, which denies the real truth of their character and personality, will be prevented. The expressions *objective* and *subjective* conception of history are derived from German historiography, and they are not easy to define when transferred to medical historiography. The word *objective* is intimately associated with the type of philosophy founded by Kant, which has been called the theory of knowledge, or epistemology. The matter is explained in Merz' "History of European Thought" (Vol. III, p. 338), but I must avoid entering upon the nature of a theory of knowledge in this connection, and only introduce so much of it as is indispensable for explaining the methods of historiography, especially the method of objective and subjective conceptions. The word objective was used by the philosophers in relation to human knowledge, in order to establish that an adequate and absolute knowledge of the objects of the world did not exist as far as the human subject was concerned. In other words, all human knowledge was subjective. H. Wesendonck ("Die Begründung der neueren Geschichtschreibung," 1877, p. 137) states that there really is no objective historiography because everything is human, and all human nature is imperfect. This would imply that it is expected of history to achieve that which is unachievable by human knowledge. Fortunately the expression objective has never been applied to historiography in this sense, for this designation can only be applied in a *relative* sense to human knowledge. Objective historiography is that which corresponds to the reality of the historic objects as far as that can be made possible by human means. In short, historiography depends upon three things: first, the general human *Anlage;* secondly, the kind or character of the objects; and thirdly, the aids to understanding, which we apply for the comprehension of those objects. In other words, human disposition, historic material, and the method are the pillars of historiography.

The objective historian gives evidence of care in detailed criticism. He avoids generalization. His personal opinion is subordinate to the main point of inquiry. He avoids a presentation that seeks effect. The entire object of criticism is to strip everything from the sources which does not correspond to actuality, and thus to come near to an understanding of reality. This is the objective insight of history. In the preceding we have seen how much of historic objectivity depends upon the comprehension of the historical material, for if there is any capacity in us to modify the material conditions of history in favor of a stricter objectivity, it is owing to comprehension. Where the comprehension does not measure up to its problem, valuable sources are not recognized and historic criticism is led into errors by defective sources. It is a well documented fact, for example, that the first excavations of bone remnants and stone implements which were made by Boucher de Perthes (1836–41) were not believed to be products of diluvial men until they were proven to be such by Charles Lyell in 1850. This reluctance to accept these human remnants as belonging to a definite geological period was due to the prevailing comprehension of the evolutional history of the earth, which at that time was opposed to such an interpretation. (See also Herbert Spencer, in Kosmos, 1886, Vol. 18, p. 261.) This may suffice to illustrate how correct objective interpretation can be prevented by temporarily prevailing conceptions, these conceptions being erroneous at the time they are made use of.

Of the three factors that determine historiography, namely, the material, the *Anlage* of the writer, and the method, the first two, are given or predetermined factors. Therefore it is the method which offers to the investigator the only opportunity to modify the course of historiography. It is one of the principal objects of the method to bring the historic material into such a relation to our understanding as to prevent the undue influence of our natural inner disposition. (This is what by common consent is covered by the expression *Anlage.*) *A method which achieves the utmost that is possible to eliminate the individual standpoint is termed the objective method. It is the one that has the greatest possible agree-*

ment with reality, and gives evidence of the greatest fidelity in conception. The method which materially lags behind and neglects these features is termed subjective. It must be always borne in mind that the terms *subjective* and *objective* are thoroughly relative conceptions.

In the preceding chapters it has been made plain that the character of the historic material is frequently such as to render a correct understanding very difficult, but the influences within us, which we call disposition, or *Anlage,* are much more difficult and numerous and very hard to control when we wish to arrive at correct historiography. But it would be incorrect to conclude that an objective manner of history writing and objective comprehension of history were not possible. This could be made a little clearer by an analogy with an investigation in biology. I know numerous eminent biologists who concede that their psychophysical condition and their individuality is a condition that they cannot get away from during the investigation of reality. This may mean that a priori the biologist renounces the achievement of an absolute objectivity. I dwell upon this matter in Chapter (VII) on the statistical and mathematical methods in historiography when I speak of the defects of stachistoscopic readings. The particular elements going into the making up of those defects are there summed up. But the biologist is far remote from letting his individual presuppositions rule. They enter into his calculations as an unavoidable source of error which he seeks to overcome with all the energy at his command. Thus the historian must deal with the presuppositions of his individuality as a source of error, and seek to eliminate them. There are, as can be discovered in the preceding part of this chapter, a sufficiently large number of methodic aids to attain a high degree of objectivity.

In biologic investigations, the elimination of subjective presupposition for the purpose of attaining results that correspond to reality, is not quite as difficult as it is in historiography, because after all the methods of biology have now reached such a degree of instrumental and mathematical completeness that the effects of human *Anlage* can practically be eliminated, but in historiography we are dealing with man in his social activity, and man's conceptions are dependent upon the social world in a much more intimate and stronger manner than he is upon material nature.

For every human being takes in from his ancestors certain emotions, feelings, convictions, and personal viewpoints which become firmly engrafted in him under the influence of environment, during the entire duration of his life. They are therefore not external impressions and customs from which he could separate himself and this makes it clear that man's reason requires a much more potent control over himself to separate or divorce his personality from these subconscious influences than it would require to separate himself from the influences of nature outside of him. Ranke has stated: "It would be impossible to have no inner opinion among the many struggles of might and of the ideas which contain within them the most potent decisions. Notwithstanding this opinion, the quality of impartiality can still be preserved, for this consists only in acknowledging the acting forces in their proper rôle and relation, and assigning each to its peculiar position." This is particularly true in the conception of historic medical personalities and their motives, where there is great temptation to intersperse sentiments, viewpoints, and efforts of our own, uncontrolled by historic criticism. It is important to become clear with one's self concerning the standpoint which we occupy in regard to a great personality. Holding our individual opinions clearly before us, enables us to draw in to the character-depiction also those moments of historic procedures which are opposed to our own viewpoint. Indeed the historiographer may even reach such immunity of individual opinion that he will eventually acknowledge his own thoughts to be less important and less effective in depicting the course of the life that he is investigating. Self-criticism and self-control in weighing historic facts and evidences in judicial balance is an exceedingly difficult quality to acquire. It might be questioned whether our human *Anlage* enables us to side with those thoughts and ideas that were a priori antagonistic to our own. Is it possible that we can ingenuously and sincerely transpose ourselves to a standpoint opposed to our own? The question must be answered in the affirmative for it is the very state of mind that is demanded of a judge who must weigh the acts of the accused in the balance of justice, irrespective of his personal sympathies and motives. There are human beings of such pronounced and passionate subjectivity that every liberation from the tyranny of the ego is impossible to them. Such individuals are not fitted to be historiographers. Maurenbrecher, and W. Stubbs ("Seventeen Lectures on the Study of Medieval and Modern History," 1886, p. 109) regard only such persons fitted to be historiographers as are capable to transpose themselves into every historic state or standpoint that has ever existed; persons that are capable to live through again the soul life of any human being that has ever existed, and capable of identifying within themselves the motives for the deeds of any historic person. These generalizations of Maurenbrecher bear within them the infirmities that such broad generalizations usually do. They are supposed to express maxims of good historiography. As a matter of fact, they require a mental versatility that is utterly impossible. I must confess that personally I would be unable to think myself into the mental attitude of Pope Alexander VI, or Machiavelli, or of Charles I of England, or Catherine de Medici. In the same manner it would appear utterly impossible to transform our thinking to coincide with that of St. Paul, or St. Francis of Assisi. The uttermost extremes of human character toward goodness or badness, toward conscientiousness and depravity, toward heroism and cowardice, etc., etc., are not attainable to the vast majority of the human species and therefore such a complete personal transposition into the psychic life of such beings is fortunately not possible and not desirable for a historiographer.

In the preceding I have spoken of the marked advantage to the historiographer if he can transpose himself to the utmost of his personal ability into a single phase of historic development as if he were one of the beings that was developed along with that particular period. In connection with this identification of the personality of the historiographer with the personality of the individual he is depicting, or his transposition into the life of the times he is depicting, a very curious spiritual transformation has sometimes been noticed in historiographers, namely, that for short periods of time they have confessed themselves as actually feeling the personality, they were describing, creeping into their own personality, as for instance, like a great actor, Edwin Booth, describing himself as Hamlet reincarnate. Whilst this spiritual transformation is rare, a milder phase of it has been felt by even the more severely objective historiographers, namely, that in place of an opposition or aversion which they had at first held, against the historic development or personality, later, after an intensive study had been made, they came to sincerely admire the personality or the historic evolution which had been the object of their research.

In 1752 Chladenius wrote a work which has an important paragraph concerning this feature of historiography, "Allgemeine Geschichtswissenschaft," chapter 6, paragraph 33: "To give an impartial narrative certainly cannot mean to recount an event without any personal viewpoints whatever, for that is simply not possible. Every narration presupposes an observer and such a person cannot be without viewpoints, and we should not demand of him that he, in his narration, should divest himself of the quality of one who is interested, or of a stranger, or of a friend, or of an enemy, of the event that is being considered. The nature of the human soul does not permit of such an abstraction, which would do away with the conception of the observer, and without observers or pupils or such as wish to learn something, all historic perception vanishes. Only the intentional distortion must be suppressed, but those are very badly in error who demand that a historiographer should conduct himself as a person without opinions, without religion, without a Fatherland, without family."

The most severely objective teacher of the historic method whom I know of, Bernheim, speaks in the following manner of this feature: "Who would expect a human being of healthy flesh and blood not to participate in the endeavors of his or her times, or of preceding times, that no judgment should be entertained concerning that which is humanely to be strived for, that no world conceptions should be formed, that in a contemporary period, filled with the emotions of party spirit, he should be immune to party feeling." All such demands would be exceedingly difficult if not impossible. The best course is to acknowledge the presence of these inner conditions, but to reckon with them as factors possibly influencing the result of historic study, and to control and inhibit them wherever necessary. Such an inner sympathetic condition of the historiographer is actually indispensable, sometimes at least, for the understanding of the motives of the actions of others, for whatever historic sources or traditions we may have of human activities, the motives are rarely given. The historiographer must therefore not only be *human,* but also *humane.* A many-sided and intensive knowledge of human nature is requisite to bring the given data and results of investigations into a uniform connected presentation by means of what has been termed combination and reproduction. He must have for this purpose a receptive and sympathetic understanding of humanity in its broadest sense. To quote Terrence, as I did in the character study of Leonardo da Vinci, "Homo sum, nil humani a me alienum."

CONGENIALITY

That such a concept as congeniality should be implicated in historiography would hardly be expected by the lay reader, and yet it is the only quality that can fit a writer and research student to understand strange individualities. It is a regrettable limitation of a human being to estimate all others according to himself, and therefore the historiographer must divest himself of his accidental individuality and sincerely think himself into the temperament and individuality of other persons. This is a gift that is more or less present in most people, but in its pronounced form it can be no better defined than by the term *congeniality,* which at first sight may appear an unessential qualification. But Bernheim says, that he who does not possess it had better keep away from historiography.

In conclusion, it may prove interesting to demonstrate how different medical historiographers, indeed, great historiographers, differ according to the viewpoints they take of history in totality, and the guiding principles or methods according to which history should be written. The opinions that I shall introduce here cannot be found in any books—they are contained in private correspondence with the men quoted. I shall introduce opinions on historiography by Charles Richet, Professor of Physiology at the University of Paris; by Charles Singer, who is conducting the most elaborate course in the Principles and History of Science at the University of London; by Karl Sudhoff, Professor of the History of Medicine at the University of Leipzig; and by Max Neuburger, Professor of the History of Medicine at the University of Vienna.

Charles Richet, the physiologist, has written a history of the world which he calls "Abrégé d'Histoire Générale," which contains numerous references to the History of Science. However one or another historiographer of different nations may differ with him on critical periods in the history of the world, everyone who reads his book sympathetically must admit that he is a master of method, that he has very clear ideas, and that they have been formed after prolonged and intensive study. In his preface, for instance, he states that he abhors the Reign of Terror and the massacre of the Huguenots. He emphasizes that among the innumerable facts of history two thoughts guided him, *faith in science and respect for the human individual.* Some of his statements are colored by the sentiments of a father who has lost a son in the World War. In other words Richet is human, and cannot but regard the ravages of war as a curse. On page 340 of this book he designates Schiller and Goethe as geniuses above their time, and he credits Immanuel Kant (1724–1804) as being the great philosopher who appeared in the twilight of contemporary times to illuminate the abysmal depths of human conscience, and also that Kant first conceived the idea of an endurable peace among human beings.

Charles Singer, in his very elaborate course on the Principles of History and Method of Science, emphasizes the value of comparison with alternate ideas which were prevalent in previous epochs and have since been abandoned. The value of such comparisons is enhanced by the fact that ideas discarded in one epoch have often been profitably reverted to at a later period, for example, the atomic theory, evolution, and the corpuscular theory of light. In one of his letters to the author, Singer confesses prejudice against the rôle of biography in scientific history, and he states that he has no competence in biography. According to Singer, the one thing important about a man of science is his science. He inclines to the view that individuals have only a small part of interest in the great panorama of the history of the human mind. The contrasting rôle played by the masses and by individuals in the History of Medicine is set forth in Chapter IV on Ideas as Historic Factors, where I believe it will be conclusively demonstrated that the progress of medicine is the work of individuals.

Karl Sudhoff, perhaps the most eminent medical historian of our time, is a man of such breadth, such vision, and erudition that it would take a chapter to describe this man and his encyclopedic knowledge. His biography has been published in America by Fielding H. Garrison, and the reader is referred to this work for further information. Suffice it to say that Sudhoff is essentially a man of facts, and that he insists that the historic facts must be scientifically arrived at. He concedes some importance to the gift of exposition, very little to phantasy, and still less to induction. In him medical historiography attains to the most exact method of presentation that exists at the present time.

Max Neuburger, the brilliant author of the great two-volume "Geschichte der Medizin," separates historiography into two parts: first, the historic research, and secondly, the historic conception. Just as it has been described in our chapter on Ideas as Medical Factors, he stipulates that in historic conception the thought must precede the detail work. This method of historic conception must be in a historiographer by nature; it cannot be acquired by any method. Thus Neuburger differs from Sudhoff in laying a greater emphasis on phantasy, and asserting that the genuine historian must be an artist—mere erudition will not suffice but in the historic research Neuburger lays the emphasis on induction as far as the detail and the proof of actuality are concerned. It is on induction that Sudhoff is not so insistent, but thinks that it often veils and colors our vision of historic facts. In the evaluation of intuition as contrasted with methodology, Neuburger bases his type of research on Goethe's natural science investigation and historic conception. This is brought out in Goethe's article, "Einleitung zur Geschichte der Farbenlehre."

These illustrations of the various viewpoints of eminent historians, together with those that can be found in the writings of American and English historiographers will demonstrate that there is need for the laying down of fundamental principles for guidance of historiographers, and in the foregoing an endeavor has been made to give an outline of these principles.

ACCIDENT, CHANCE, AND FORTUITOUS CIRCUMSTANCES ARISING DURING THE PROGRESS OF HISTORIC RESEARCH

Accident or chance, etc., can be excluded in biologic research because of the increasing accuracy of the chemical and biologic apparatus and also because the experimenter in biology has the power to determine the conditions of any experiment and the *Fragestellung,* but the exclusion or fixation of these factors in historic research is exceedingly difficult. Whenever a long investigation of sources of events, discoveries, and personalities, etc., etc., has been made and our evidences suddenly come to an end in one chronologic period, and then just as suddenly are resumed again at another period in a manner that we sometimes designate as "without rhyme or reason," we are too likely to fill up the gap by assuming that something accidental, something fortuitous has occurred, but with this subterfuge we inject a doubtful and undeterminable element into our entire sequence, or logic. It is best to avoid this assumption as much as possible, simply because of the difficulty of determining it and also because of the liability of encouraging a form of superstition in history which we often find expressed in the terms "Lucky Hour," "fortunate coincidence." We find it dramatically represented in Schiller's "Wallenstein," where this leader of the Austrian forces in the Thirty Years' War permits himself to be influenced in every important decision by the taking of a new horoscope through his astrologer Seni. No causal connection can be attributed to the mere sequence of one event after another event, and so on. There must be in addition positive indication of an inner correlation, or other analogies for the mutual connections of the special events, so that all accident or chance connection is excluded.

III

CAN MEDICAL HISTORY BE CONCEIVED AS SUBJECT TO THE LAWS WHICH CONTROL AND INITIATE GENERAL HISTORY?

AS medicine is both a science and an art, the science being founded in anatomy, chemistry, physiology, botany, bio-chemistry, and pharmacology, etc., and the art being the applied medicine, we have to do with a double aspect of medicine: First, the scientific aspect, and secondly, the pragmatic aspect. Both of these could not be controlled by identical laws. Each has laws of its own. Of science, this must be manifest to every specialist in anatomy, chemistry, physiology, etc., but the laws of art will be found set forth in the articles on Leonardo da Vinci in this volume, in Turner's "Modern Painters," in the "History and Bibliography of Anatomic Illustration" by Choulant translated by Mortimer Frank, and in the beautiful works of art of our American sculptor-physician R. Tait Mackenzie. Concerning a more precise distinction of these two aspects of medicine, the reader is referred to the article on "Science and Art in Medicine: Their Influence on the Development of Medical Thinking," (J. C. Hemmeter) *Journal of the American Medical Association,* January 27, 1906. In my article on "Ideas as Factors in Medical History," I have a section devoted to the use of words in the expression of ideas and there I have emphasized in other terms what the Duke of Argyll asserts in his "Reign of Law" (5th ed., page 63): "Words, which should be the servants of thought, are too often its masters. There are very few words that are used more ambiguously and, therefore, more injuriously than the word *Law*. It may be indeed used legitimately in several different senses, because in all cases as applied in science it is a metaphor and has relation to many different kinds and degrees of likeness in the ideas which are compared." According to the Duke of Argyll there are at least five different senses in which law is habitually used, and these must be carefully distinguished.

"First, We have Law as applied simply to an observed Order of facts.

Secondly, To that Order as involving the action of some Force or Forces of which nothing more may be known.

Thirdly, As applied to individual Forces the measure of whose operation has been more or less defined or ascertained.

Fourthly, As applied to those combinations of Force which have reference to the fulfilment of Purpose, or the discharge of Function.

Fifthly, As applied to Abstract Conceptions of the mind—not corresponding with any actual phenomena, but deduced therefrom as axioms of thought necessary to our understanding of them. Law, in this sense, is a reduction of the phenomena, not merely to an Order of facts, but to an Order of *Thought.*

These great leading significations of the word Law all circle round the three great questions which Science asks of Nature, the *What,* the *How,* and the *Why:*

(1) *What* are the facts in their established Order?

(2) *How*—that is, from what physical cause,—does that Order come to be?

(3) *Why* have these causes been so combined? What relation do they bear to Purpose, to the fulfilment of intention, to the discharge of Function?"

We learn also from these statements that it is not necessary to the legitimate application of law, say "as an observed Order of Facts," that the cause of any such observed Order should be at all known or even guessed at. Least of all do laws in any of the senses mentioned explain themselves. They suggest a thousand questions, much more curious than the questions they solve. The Duke of Argyll, for instance, calls Newton's discovery of the Law of Gravitation (1682) the "highest exercise of pure intellect through which human mind has found its way. It is the most universal physical law which is known to us." The recent arguments and discoveries of Einstein have demonstrated how little we have ascertained in this great thought after all, although it gives no sort of explanation of itself. The Duke of Argyll gives examples of types of law under each one of the classifications which I have quoted from his book. The fifth one is where phenomena are not merely reduced to an Order of Facts but to an Order of Thought. This means it is reduced to a purely abstract idea. There may be no phenomena that actually correspond with such an idea and yet a clear conception of it may be very essential to a right understanding of all the phenomena around it, and in the "Reign of Law" (page 108), he gives a Law, which in the science of mechanics is called the *first law of motion.* The Law is, that all motion is in itself uniform in velocity and rectilinear in direction. "According to this law, a body moving and not subject to any extraneous force would go on moving forever at the same velocity and in exactly a straight line.

This is an abstract idea of motion which no man has ever, nor can ever, see exemplified. It was a great effort of the mind to conceive it, because not only is there no such motion in nature, but there is no possibility of an artificial means of producing it. The first law of motion is purely an abstract idea. Like many other such laws of the same class, it was discovered not by observing, but by thinking." In the chapter on "Who Is The Real Discoverer" (VI), I shall dwell more fully on how these mental processes are either conducive or detrimental to medical discoveries and inventions. I can say of the medical mind what the Duke of Argyll states of the human mind in general, that in the exercise of its own faculties and powers, sometimes by careful reasoning, sometimes by intuitions of genius, unconscious of any process, it is able from time to time to reach one of those purely intellectual conceptions which are the basis of all that is intelligible to us in many fields of chemistry, to which belongs the field of immunity and prophylaxis, also in physiology, biology, and anatomy, as well as in the special adaptations of chemistry to pharmacology and therapeutics. One of the finest examples of this method of thinking is the theory of immunity first announced by Paul Ehrlich and known as the side chain theory. In personal conversation with Ehrlich himself, I called his attention to his own state of mind when he first laid out the plan of this great thought structure and that he was in a similar state of mind as Goethe was when he first conceived the *Urpflanze* and which led to that memorable conversation between Goethe and Schiller which I discuss in Chap. XXIX. In other words, Ehrlich was looking for an ideal Order, for a simplicity in bio-chemic law, and it is due to this amazing introspection, evidently at its inception nothing but an abstract idea combined with what Vaihinger terms the *fictive* thought method, that the world has been blessed with a number of the most effective chemical means for the cure of disease. Ehrlich announced the side chain theory in 1897, but he and von Behring had already brought out the diphtheria antitoxin in 1894. I ascertained, however, that Ehrlich admitted having the train of thought that the action of an antitoxin was a stochiometric chemical process at the time that he was working on the diphtheria antitoxin, so that really a discovery that came out three years before the side chain theory became clarified in Ehrlich's mind, owes its advent to the incentive received from this conception which is, in short, an explanation of the processes of immunity by assuming a definite relation and connection between toxin fixation and anti-body production.

The law of electrolytic dissociation which was gradually elaborated through the minds of Van't Hoff, Planck, Arrhenius and applied to human cells by H. J. Hamburger is even a more striking illustration of such a human thought experimentation. (The entire literature of the evolution of this magnificent thought structure is given in H. J. Hamburger's work *Osmotischer Druck und Ionenlehre,* Vol. I. Ostwald calls the idea of Planck and Arrhenius "Ein Glücklicher Gedanke," in his article on the theory of electrolytic dissociation, Zeitschr. f. physik. Chemie. 2. 1888. S. 270.) I will discuss in the chapters on Medical Discoveries, Chapters VI and XXXI, to what extent such an expansion of human knowledge can be attributed to a "lucky thought," but in the various essays in this book, it will become evident that great medical minds in their profound meditations and interpretations of life processes are guided by those intuitions which arise in finely organized intellects, and when brought into close relations with the mind of nature, yield brilliant results as if by revelation;—to the uninitiated this appears a fortuitous process, or a lucky thought. It seems that the operation of a highly trained mind and one that is experienced in the exercise of speculative thought may produce advances which appear to be guessed at before they are proved to be true, but the point I wish to bring out here is *that those "guesses" had their origin in abstract ideas of the mind, which eventually turned out to be ideas that were really embodied in the order of nature.* Whewell, in his "History of the Inductive Sciences," says that this has occurred so often in the history of science that it is not to be considered an exception, but the rule. He also states "it is manifest that in this, as in other cases of discovery, a clear, steady progression of abstract ideas and an aptitude in comprehending real facts under these general conceptions must have been leading characters in the discoverer's mind." (Whewell, Vol. I, page 389.) Now, when we come to ascertaining or denying the laws observable in the evolution of general history and of medical history, we have to face the fact that the majority of such laws must concern human psychology. Therefore, they are not strictly speaking laws of history, but laws of psychology. Are there any laws of history per se? Edward P. Cheyney, in his Presidential address, delivered before the American Historical Association at Columbus, December 22nd, 1923, argues that in general history such laws exist. He states (American Historical Review, Vol. 29, No. 2, January, 1924, page 235): "Examined closely, weighed and measured carefully, set in a true perspective—the personal, the casual, the individual influence in history sinks in significance and great psychic forces loom up. Events come of themselves, so to speak. They come so consistently and unavoidably as to rule out as causes, not only physical phenomena, but also voluntary human effort." Further on he declares that "history, the great course of human affairs, has not been the result of voluntary action on the part of individuals, or groups of individuals, much less of chance, but has been subject to law." Men, on the whole, have played the parts assigned to them. They have not written the play. Man is simply the part of a law-controlled world. And then, he discusses seriatim six laws, *first,* a law of *continuity; second,* a law of the *impermanence of nations; third,* a law of the *unity of the race,* meaning an interdependence among all its members; *fourth,* a law of *democracy*—this means a tendency of all government to come under the control of the people; *fifth,* a law of *freedom* —this means a law of necessity for free consent; *sixth,* a law of *moral progress.*

It must be evident at first sight that some of these so-called laws of Prof. Cheyney exclude others of the same series, for example, how is it possible that the law of continuity can exist or the law of the progress of morality, when there is continually active through all history an assumed law of the impermanence of nations? If there is any record of history that is reliable, it is that the human race is extremely slow in complying with the dictates of morality. To the present day the ten commandments are not complied with voluntarily, but have to be enforced by civil law upon the great majority of the race. As a matter of fact, nations and races that are reported as having existed at the very dawn of history are still with us. The Persian nation and the Chinese, even the Greek nation, still exist. A law of the impermanence of nations should not be founded in the self-destructiveness of the human race. I cannot concede it to be a law of the impermanence of nations, for example, when Scipio Africanus in the Third Punic War from 149–146 B. C. completely destroyed the Carthaginian nation with fire and sword, for the majority of the inhabitants of Carthage were burnt up or consumed in a conflagration. In other words, human barbarity and destructiveness is no ground upon which to base a law of the impermanence of nations. One might as well speak of the impermanence of the red-wood trees in California, if some vandal should take it into his head to set fire to them. By a natural law, I understand *something above human control* and history reveals that the impermanence of nations is due to the self-destructiveness of man, great terrestrial or volcanic convulsions, or extensive epidemics, etc. All of these catastrophic disasters man must learn to avoid. He can avoid the earthquake by not living in countries where earthquakes occur. He can learn the cause of epidemics and become immune to them and war must cease, but over and above these catastrophes, there is an *everlasting causality in history, which the human mind has not yet grasped and is perhaps not yet fit or enabled to understand.* At any rate, it is evident that not all of Dr. Cheyney's laws are adaptable to Medical History. The causality of history which is inseparably connected with that of law in history, is complex beyond any reach of our present mental analysis and yet in order to reach an intelligible scheme of things, we must never cease in the constant quest for causality and therefore for law in history. David Jayne Hill in a review of Henry Osborne Taylor's work on *Freedom of the Mind in History* admits that there are *non-mental* influences which fix the path of historic process and determine the greater part of human activity. (The Amer. Historical Review, April, 1924, page 526.) Our present question is, are there any laws which we can apply to Medical History? Without doubt, the so-called psychological causality is such a law, but as I said before, it becomes evident to us, only in its effects, that is, pragmatically. There are certain constant processes of consciousness which can become historic causes. One of these is the identity of human sensation, willing, feeling and forming conceptions. The most comprehensive fact of human existence is that these processes frequently repeat themselves in the type of their combinations and consecutiveness. Every perception of motives and activities, all understanding and analyses of their outer and inner complexes are founded in this psychologic causality which I have spoken of in the chapter IV on *Ideas as Factors in Medical History.* It is a psychologic causality that is rooted in the uniformity and specificity of human willing, thinking, and perceiving. Now, if such a psychologic basis for human activity is accepted, one might assume that it would be possible to deduct the historic phenomena as effects of such psychic cause of laws just as physical phenomena can be deducted from physical laws. This would be a natural science interpretation of history, but the historic transactions, evolutions and events in general history, or more especially, in Medical History, cannot be interpreted in this manner. It makes no difference how psychic processes are explained in themselves, the history of nations and the History of Medicine cannot be ascribed to the effects of biologic-psychologic laws. The utmost that we accomplish by such laws might be expressed by the term *generalities.* The general constant forms and processes of historic transactions, the general motives and forms of activity of human beings, the general requirements and biologic necessities of life and certain properties of the genus or family might be deduced from psychologic causality and if done expertly, a great deal could be accomplished, but we could not thereby penetrate to the heart of history, which is the content of different cultural stages. By this I mean in general history, the content of the activities of men as social beings, in Medical History, the content of the aims and desires to achieve a conquest of disease and relief of suffering. In the chapters devoted to "Methodology," I have referred to a certain law of *contrast reaction,* which means that if a large number of equally disposed and equally reacting human beings come under the influence of equal incentives or excitants, they do not necessarily always react in the same manner. One cannot say, for example, that the continuance or the repetition of the same influences on human beings of the same psychic characteristics will always produce the same reactions. It has been proven in history that continuance and repetition of the same influences are a cause of *new and different* reactions from those that were elicited when these influences or excitants were at first applied. This has been called the *Law of Contrast Reactions.* This law is only a description of the process in a general way. It cannot be stated when and how the contrast effect will come about. There is a law of contrast reaction in the more special domain of physiology, particularly in pharmaco-dynamics. For example, such a pure substance as adrenalin does not always cause constriction of the vessels, but sometimes may cause dilatation. We have another and similar example in the so-called positive and negative phase of blood coagulation. Here we have in one leap passed from the variability of effects of the same influences on the same human beings in history to the narrow field of the pharmacologic effect of the same chemical compound upon the same tissue, without, however, eliciting any law that was applicable to or could be derived from either phenomena.

When we are dealing with masses of human beings, it has to be borne in mind that the irritability of the individuals composing the mass is not a determinable factor. It is not determinable upon groups of beings of the same time period, much less so among masses of different historic periods. We might ask ourselves how is it that the Catholic religious authorities of Vienne, France, tried Michael Servetus, but did not execute him (burning his effigy) and apparently connived at his escape. But the authorities of the Church of John Calvin at Geneva, who were claiming a greater liberalism and enlightenment, after a long legal pseudo-trial, condemned and burnt Servetus at the stake. This is a problem of general history that extends into Medical History and is perhaps explainable by the different degree of mental irritability of the two groups that tried Michael Servetus. I said before that the identity of human sensations, willing, feeling, and thinking, as it repeats itself uniformly in its combinations and consecutiveness, is a factor of psychic causality. The understanding of human beings among themselves and the understanding of their activities depends upon this, but yet, there are qualitative differences of the individuals, which are as important factors in the evolution of human activities as the identities and uniformities in feeling, willing, and thinking, just referred to. These qualitative differences of human individuals bring it about that change of causes does not always produce a corresponding change of effect and that the continuance and repetition of causes does not invariably produce the same effects. These psychic differences once understood, it must become evident *that it is impossible to predict historic effects even from well-understood causes.* It is only possible to judge of causes from their actual realization, which means that the historian can endeavor to elucidate causes only regressively, namely by their effects. In very few instances can they be foretold "a priori." It is also incorrect to state that there is a special social-psychologic causality, which compels and determines individual activity and conduct for the reasons above stated. The article of Cheyney, which we have quoted, is an example of many essays and articles, which proclaim the necessity of reducing the facts of history to general laws, before history can prove its right to be called a science. "To find the laws of history," this is a slogan which runs like a yellow thread through much of the modern literature concerning this subject. In most of this literature, the limitations of the concept *law* and of biologic knowledge is not clearly defined nor is there any effort made to explain that the human intellect is not capable of grasping and comprehending the endless manifoldness of the phenomena of this world. All that human beings can do is to make an ordered selection and arrangement of those aspects that are essential for the observing intellect and the mind does this by the formation of concepts and judgments. The most primitive men had to form these concepts and judgments without any thought of causality. They formed their ideas exclusively according to external similarities and dissimilarities or according to the accidental juxtaposition of things. But the more the intellect becomes conscious of its activity, the more purposeful and intentional we find the conception of causality being introduced as a helpful, explanatory factor. All perception based on experience and science then becomes a perception of causal connections. It is not meant to include in this statement that the intellect believes that it can find a cause of all and every manifoldness of the phenomena. It only means that it has *faith* that such cause or causes must exist, even where for the present they cannot be traced. Causality in history means an assumed lawfulness. It does not mean that historic knowledge, like natural science knowledge, is based on a mechanical lawfulness. Natural science knowledge, which at the present time seeks a mechanical and physical causality, and the causality that is back of all historic activity are not identical. Causality means that the intellect assumes for everything that is or can be thought of as *becoming,* there must be a definite reason. The main difficulty why the laws of natural science cannot be applied to history is this—historic research does not direct its efforts to mere processes per se, but to the contents of human activities. And these contents, history wishes to reveal in their very development, not as concrete and finished things, but as processes in the act of evolution. Nor does history desire to reveal simply the features in which these activities are in agreement, but rather the features in which they differ. In seeking an expression which would describe this kind of knowledge that exact history aims at, I find as the nearest one *idiographic* knowledge (E. Bernheim). Whilst this is a characteristic of historic knowledge as distinct from natural science, it forces itself into biology wherever the mind is dealing with the evolution of living things, *for biologic evolution is history,* but wherever else the naturalist deals with living things, he abstracts from reality and all the qualitative differences connected therewith, and tends to average or general perceptions. This is noticeable in what natural science designates as classification. Here the individual living thing is only the average example of a species. The biologist is interested only in that which is in general agreement, that which is typical in the individual. The differences interest him only when they are sufficiently marked to determine a new species. So history insists on emphasizing the differences, whereas natural science tends more to emphasize agreements and uniformity. Wherever general laws of the forms and functions of life have to be established, the differences of the individuals are made to disappear in biology. This is a technical trick in order to establish special results of scientific investigation. Sigwart ("Logik," Vol. II, page 642) designates this procedure as the *methodic directive.* Historiography, as we have seen, has its methodology also and although these methods are far from perfect, yet they are so distinct from those of natural science that historians cannot consent to a hypothetical consummation of agreements, likenesses, and uniformities in historic processes, etc., for the sake of classification. For history deals with human beings, the qualitative and quantitative differences of which cannot be ignored, but on the contrary it is the very aim of history to recognize and understand them.

The conception of lawfulness as it is used in the natural sciences is not applicable to history because historic phenomena, events, particularly the processes, discoveries, doctrines, etc., etc., of medicine are conditioned individually and are exceedingly complex. All of the laws referred to in the "Reign of Law" by the Duke of Argyll could be condensed into two groups: first, natural laws and, secondly, empirical laws. Natural law taken in the strictest sense of natural causality signifies a conclusion that is generally valid for a definite domain of phenomena. Natural law means a generally valid judgment under which the perception of constant causes, or dependence of phenomena upon causes, or the constant relation of phenomena to *known and recognized* causes is summed up. Secondly, empirical laws which should preferably be termed "rules," are observations of constant connections between phenomena and causes without any indication or evidence of the nature of the connection. These judgments of constant observations of cause and effect are matters of experience and therefore are called empirical. The difference between natural and empirical laws is that in the first the causes are known or supposed to be known and in the second they are merely assumed. We have up to the present time not even developed an idealogy for operating with the conception of laws in history. I mean that before such a problem can be undertaken there will have to be much research into the philosophy of history, into psychology of masses and of personalities, in order to coin indispensable concepts and to formulate the whole problem into perspectives, sufficiently clear to stimulate research on this subject. What have been called laws of history hitherto are in reality a few limited rules of human psychology. Perhaps as the race advances, there may be experiments in human history, which either will require the consent of human beings who are in adult age or they will have to be formed upon the young of the race before they have gathered experience. In other words, they would have to be taken in a stage which is described in the story of Kaspar Hauser (Regensburg, 1883) and isolated upon islands separated from the general community of mankind, but supplied with the means of cultivating the soil and under the tutelage of masters in psychology, who would conduct this human experiment in order to discover first of all, if possible, what the human mind would develop into, if entirely separated from and ignorant of the conventionalities of human society. At our present time such an experiment appears preposterous; it seems unthinkable and yet such an experiment would be more humane than many of the social and governmental experiments knowingly carried out at the present time in spite of the fact that they have been followed by disaster and almost separation of the countries in which they are being practiced (I mean separation from the rest of the civilized world). This experiment would include that the teachers in charge of these adolescent human beings would tell them nothing of human history whatever and regulate the relations between them along lines agreed upon between the experts who outlined the *Fragestellung* and the special problems to be solved. Experimental human history so far has been unthinkable and, of course, not feasible for that reason, but as Charles Richet emphasizes "L'histoire n'est qu'un long martyrologe." And if it has been a martyrology, certainly such an experimental history as I have suggested could be formed under conditions in which it would be congenial and a matter of pleasure to the human beings experimented upon. It would require many generations of subjects and experimenters. It would be really an experiment of, with, and by ideas, the ideas in the plan of the experiment itself, the idea spontaneously evoked in the mind of the subjects experimented upon, and the idea produced by the reciprocal effect of these two. Life is short, but art is long!

IV

IDEAS AS FACTORS IN MEDICAL AND GENERAL HISTORY

THE psychic qualities and constitution of human beings are such that in the reciprocal reaction between the mind and the influences and impressions of environment there must necessarily result ever new combinations, increments of psychic activity, from which again new and qualitatively different purposes result.

Evidently, ideas precede purposes and eventuate in purposes. But the new combinations and increments of psychic activity that occur in response to impulses from environment require undergoing of another mental process before they can result in purposes; they must be selected and coordinated and this requires a high degree of thought training, a broad experience in methodic thinking, a type of thinking of which medical men are generally intolerant. As a class, the medical profession is generous in its appreciation of what is conceived as *a thinker.* But the appreciation is based more upon the results and objects of thinking than upon the process of methodic thinking. The same intolerance I have found associated with the thought content of the words—*idea, experience, belief,*—so that it becomes a compelling necessity for any research worker in history to make it clear how he intends these words to be understood.

As I have said elsewhere that Medical History is essentially the evolution of great constructive *ideas;* it becomes very important to explain how I prefer the word idea to be understood. Especially important does it now become to ascertain whether the concept "historic ideas" of the scholars of General History is identical or not identical with the same concept in Medical History.

The word *idea* comes from the Greek έἶδος or ιδέα, meaning the form of anything as conceived by the mind. The emphasis of this definition is to be placed on the rendering of form, shape, or constitution. This was not the original thought content of the word *idea* as Plato and Aristotle understood it.

But to review the history of this word from Plato and Aristotle, Philo, Origen, to Descartes, John Locke, Hume, Kant, Hegel, and Wundt, would lead me too far away from the purpose of this article. Further on the use of the words έἴδος and ιδέα as they occur in the works of Hippocrates will be given. For physicians, as well as philologists, this wisest of ancient medical writers, commands intense interest. Psychologists at the present time restrict the use of the word *idea* to INNER complexes that do *not* arise from direct outward impressions. Ideas, accordingly, are memory images. For thought complexes formed from *outward* sense impressions they generally employ the word perception.

The philosophically untrained medical mind will probably not be impressed with the importance (if any) of this distinction. No less a modern psychologist than Wundt states that there is no valid difference between memory images and so-called sense perceptions.

It is almost impossible to give a definition of any of the important words I have used without necessarily using other words which themselves require definition, especially so if unanimity or agreement as to the meaning of words is aimed at, as a necessary prerequisite to mutual understanding between the author and the reader or listener. And if there are several meanings to a given word it is necessary that the author make clear the particular meaning which he prefers to be accepted as underlying his personal exposition and presentation of thought.

In the preceding, for example, the word *perception* was used as designating thought complexes arising from outward sense impressions as distinct from such *ideas* or memory images, which are thought complexes that *do not* arise from outward sense impression.

In this definition the word *perception* was used as if the reader were already familiar with its specific meaning. This assumption of previous understanding by medical readers of most essential words is imperative, if this work is to accomplish some of its purposes, more particularly, that of creating a fondness or taste for the philosophy of history and at the same time of keeping within certain limits the composition of this work. Going from special definition to special definition is not conducive to understanding if the object is to outline certain causal factors in Medical History; πολυμαθίη νόου ἕχειν οὐ διδάσκει (Heraclitus aphorism XVI: Too much learning does not teach understanding.)

No word in philosophy or psychology has been productive of more confusion than the word *idea*. In the sense of some historians and philosophers it is the key to their entire system and unless it is understood as they understand it, their whole argument is lost.

According to Plato all insights that precede experience are derived from the world of ideas, and this also is Plato's statement, namely, that the relations between experiences are intermediated by ideas.

Ideas then, according to Plato, precede experience, but experience is the sum total of conscious events which compose an individual life. All that we know and feel and do, all of our facts and theories and emotions, even our ideals and purposes can be included in this term *experience.* The content of experience is made up of *percepts* and *concepts.* These two are distinguished by the following: *percepts* are sensual or sensuous (requiring use of the senses), *concepts* are intellectual.

We now come to a momentous antinomy (conflict between two laws or truths), between ancient and modern philosophic psychology. Bearing in mind the view of Plato that ideas precede experience, we learn from G. F. Stout that "the existence of ideas is *subsequent* to that of percepts and even implies some complexity of perceptual cognition."

As students of history we must all desire clearness as to whether ideas precede experience or experiences precede ideas.

In order to operate with the content of the word *idea* as factors in medical history, and as the first step, we shall have to understand that the thought content of the word *idea* is twofold, viz. Ideas represent (1) symbolism or (2) significance.

Ideas of things as well as ideas of relations can be considered merely as symbols, then they are products of the mind. But considering the significance of ideas, that is, their meaning, they are realities. Here we have at last reached a point of contact between history and this complex psychology. All ideas that have a meaning are realities and can induce action, if not inhibited. They represent potential forces that can pass into action, whether they be connected with consciousness or not. It must be intelligible then, that ideas can become historic factors.

WHAT IS THE FUNCTION OF IDEAS IN GENERAL HISTORY AND CAN ANY DEDUCTIONS BE MADE THEREFROM APPLICABLE TO MEDICAL HISTORY?

Here again, it is necessary to distinguish between general and individual forces and factors that are effective in history. A. D. Xénopol, in his work, *Les principes fondamentaux de l'histoire* (1899), is perhaps the French historiographer who is most felicitous in the presentation of these reciprocal relations. According to Xénopol all historic phenomena are the effects of two kinds of forces, first, general forces which Bernheim designates as "factors," and secondly, individual circumstances ("Circonstances"), and that these two are mutually or reciprocally interacting, sometimes coöperating, sometimes antagonistic, sometimes modifying each other. Thus historic phenomena of the world are manifestations of forces (factors) in their modifications and transformations by particular circumstances. The factors are the *general* causes, the particular circumstances are the *special* causes.

It has never seemed intelligible that masses could originate great constructive ideas. In spite of such expressions as the "Spirit of the times," "Zeitgeist," "Public Opinion," "Einfluss Allgemeiner Zeiströmung," whenever it is possible to trace the idea that has actuated or is actuating great masses to its source, it is found that it is either an old historic idea rejuvenated, like the League of Nations, which was clearly preached by Christ and formulated by the great Königsberg philosopher and historian, Immanuel Kant, or if it is a new idea, it is invariably the product of some eminent individual. Even in such a materialistic conception of history as is embodied in the socialistic-economic theories of Karl Marx, K. Kautsky, Bebel, Lafargue, and Engles, this is true. The relations of material production are the constitutive factors of all historic development, according to these men. History evolves according to production—the state of the technical means (i. e., machinery) necessary for the elaboration of the material requisities of life and the manner in which the products are appropriated, distributed, consumed and the contemporaneous technical methods of production—that all of these factors are transformed into ideas in the heads of men (*in den Köpfen der Menschen zu Ideen umsetzen*).

One cannot mistake these assertions as far as the origination of ideas is concerned—the material economic factors come first and it is they that secondarily are transformed into ideas. Xénopol's conception of the consecutiveness of general factors and special circumstances is exactly reversed by Marx, Kautsky, etc. For it is plain from the French historiographer's argument that by individual circumstances he means *ideas*. He distinctly says: "In the higher organized phenomena the element of *individuation* gains a continually increasing influence."

Individuation means the development of the individual from the universal or general. This can only be effected by the evolution of great ideas inherent in or rather produced by the individual himself or herself. Kautsky and Marx are not speaking of history of the sciences but only of General History. According to Marx, ideas are supposed to be the "reflex" of economic-social conditions, and according to Kautsky, the individual has no power of initiative in historic causation—"the individual is the servant of economic social relations."

One has to read the history of Socialism to become convinced that it is itself a product of evolution. The central idea appearing long before any orderly system of historiography or any philosophy of society was recognized. There are distinct ideas of Socialism in Plato and in Sir Thomas More's *Utopia*. So that in spite of the denial of all initiative to the individual by Marx, Kautsky, and others, the very idea for the economic-material conditions forming the driving force of all social life is itself the product of the mind of individuals who have lived at different times. Occasionally the leading socialistic thinkers speak of *reciprocal effects between human mind and economic affairs,* so that perhaps unconsciously they do concede some influence to the ideas of single individuals. But in carefully studying the historic development of Socialism I did not find the evidence of a great constructive idea born out of the masses. It is always the leaders of the mass that announce the new idea and give the incentive to coöperative and collective action. Nor does it appear possible by any modern method to ascertain whether great groups of men in some way could produce a collective great idea, I mean, originate a great idea as distinct from receiving it secondarily from a leading thinker. I have selected the economic-materialistic doctrines of Marx, Kautsky, and others, without approving or disapproving them, simply because these doctrines assigning all stimulus and incentive to historic action to economic forces and the technical instrumentalities of production, and divesting the individual of all initiative and denying to individual ideas any influence on the evolution of history, should give evidence of the power of initiative of masses, of the historic proofs that large groups of mankind have ever spontaneously originated a great constructive idea.

The history of Socialism furnishes no such evidence. On the contrary Marx's assertions that ideas are nothing but reflexes (reflections?) of economic relations and that ideals are nothing but the transformed material elements, and that it is not consciousness of men that determines their being, but reversely, their social existence determines their consciousness (Karl Kautsky—*Wass will und kann die materialistische Geschichtsauffassung leisten?* in *Die neue Welt,* 1896) make any methodical critique of such a political and historic hypothesis impossible. A further barrier to a logical and methodic abjudication of these positive assertions is erected by Kautsky when he writes "in so far as historiography enters upon the activities of individuals it becomes a mere Art, for the investigation and representation of historic development can only become a science in so far as the limit of the material method reaches."

Kautsky goes so far (in his *Geschichte des Sozialismus in Einzeldarstellungen,* 1895) as to give such great historic developments as the German Reformation under Luther, which the Protestant population of Germany accepts as a purely religious upheaval, the appearance of an economic reaction of the German people against their financial exploitation by Roman Catholic Italy and the Rulers of Spain and France connected with it. Even the German princes and rulers who protected

Luther are typified as having been actuated by economic, material, and selfish motives, among these, the Elector Frederick the Wise, who figures in the historic records of the Reformation as an altruistic ruler.

IDEAS AS FACTORS IN MEDICAL HISTORY

The entire history of Socialism is a record of the ideas of individuals, as can be seen from the Article on the subject in the *Encyclopedia Americana,* and in the *Encyclopedia Brittanica,* and also in chapter nine, volume four, of John Theodore Merz's *History of European Thought in the Nineteenth Century.* Merz entitles this chapter *"Of Society."* It is a marvel of historic exposition, but it nowhere gives the slightest evidence that masses can originate an idea. We shall see that the economic materialism, in spite of its many defects, deserves credit and has earned the merit of having enriched the scientific study of history by directing constant attention to economic conditions and influences, and to the more real interests of social groups as far as they are active in historic life, but the defects of the socialistic-naturalistic directions of history consists in taking one single aspect of human activity, namely, the material-economic, and representing it as an independently effective power and as the fundamental cause, not only of the total social development, but of all human historic activity, in short, a condition is interpreted as an effective cause. As there must be the greatest emphasis laid on the distinction between the effects of the ideas and activities of individuals and the activities of the masses, I hoped that the ability of the masses to originate ideas, if any, would be evidenced in the doctrines of socialism which I have here called by various terms, the most frequent one being the designation *material-economic* doctrines. As a guiding line to emphasize this distinction, it will be profitable to briefly abstract the viewpoints of some of the predecessors of modern socialism. Prominent among these was Condorcet in his book "Esquisse d'un tableau historique des progrès de l'esprit humain," 1795, which he wrote as a Girondist (déjà proscrit) and was published after his death. It is exceedingly impressive to learn the insight with which Condorcet distinguishes so-called factors that condition the course of history. He distinguishes four such factors: 1. Human *"Anlage"* or disposition; 2. External natural influences (environment); 3. Reciprocal influences of human beings upon each other; 4. First achievements of culture. These are his basic factors of historic evolution. He conceives as the aim and content of this evolution the obliteration of all inequalities among nations and within all nations, and the perfection of mankind and of human abilities. All nations, even the wild tribes of Africa, he hopes, will at some future time take up the paths of enlightenment which the most cultured nations have already entered upon. He adds very significantly: "Up to the present time historiography was merely the record of a few individuals. That which really goes to make up the human race, the mass of beings, and families which live from their own work, has been forgotten. The historiographers have only had eyes for the leaders."

One can readily perceive that Condorcet was an apostle of the French Revolution, whose main aspiration was to call attention to the rôle and significance of the masses, the great bulk of humanity in history, and he places this in the center of the historic panorama, and from this standpoint he views as the most comprehensive, the most inclusive conception of historic evolution, the progress of social equality, and from this standpoint also he asserts that the first general cultural achievements which can be discovered, humanity owes to the nameless masses. From this standpoint also he concludes a portentous union between socialistic and natural science observation of history. Condorcet here reveals himself as a profound historic thinker, for his deductions are not accidental, but well founded and inwardly, convincingly, and logically connected. Whenever the great mass of humanity is placed in the center of the stage of historic transaction, observation is directed to human activity as a totality. The onlooker and the student of this process is no longer a student of history exclusively, but he becomes a student of natural science, of biology, and in doing so he receives the impression of regularities, constants, and processes that are calculable, and from this viewpoint to the conception that the activities of the total human race are ruled by constant mechanical law is only one step, and consequently the next step appears to follow as a natural issue from this historic philosophy, namely that the principles of natural science, the general laws which condition the phenomena of the universe are just as valid for the evolution of the intellectual and moral activities of the human race. It is for these reasons that this form of historic thinking has been called the socialistic-naturalistic direction of history, or, as it has been recently designated, the *collectivistic* tendencies. The conceptions of Condorcet were elaborated later on by Auguste Comte in his *Cours de Philosophie Positive,* specially in the fourth volume which appeared in 1839, and which contained the foundations of a scientific sociology and therefore, also of a philosophy of history. In the further development of conceptions concerning the specific and relative effect of the power of collective humanity, the masses, and the effect of individuals, there developed in France two schools of sociology which could actually be defined as the School of *Individualism* and the School of *Collectivism.* The main representative of the School of Collectivism bears a German name, M. Dürkheim. The representative of individualism is Gabriel Tarde (1843–1904). According to Dürkheim, the essential character of the social or mass influence is that it actually controls and coerces the individual. Tarde, on the other hand, explains this social influence, not by the effect of masses on individuals, but by the influence of one person on another person, by what he calls the *inter-cerebral* relation of two minds, the reflection of the one by the other. He agrees with Dürkheim in maintaining that the primitive fact of social life is some kind of control or coercion under which the individual is placed, but whilst Dürkheim finds this in the coercive action of the *collective* over the Individual, Tarde explains this by the more intelligible influence of two individuals on each other. The psychologic processes that underlie this influence are suggestion and imitation. In Germany there are also two schools of social and historical doctrine, namely, the *Idealistic* School and the *Realistic* School of History, and nothing can be more characteristic of either the French or the German personal psychology than the true inwardness of their theories as to the conception of history. In France that tendency has finally conquered which makes life of society the main subject, in other words, *collectivism* is supreme. There is still a controversy which turns on the question as to what extent an individual influences this life of society. Up to the year 1918, the conclusion of the World War, in Germany the conception of history had not been uniformly fixed in reference to this matter, and if it has inclined more to collectivism since 1918, this is to be explained from the fact that historiography in Germany to-day is largely under the suggestions of the school of Karl Marx, because the present German government, 1925, is socialistic. But there were eminent historians, prior to the World War, who were opposed to the individualistic interpretation of history.

The most able of these were E. Bernheim in his *"Lehrbuch der historisschen Methode,"* and Carl Lamprecht in his "German History." Bernheim does not deny the power of individualism, he admits, but also qualifies it. In the German individualism, as a principle of historiography, the single person, not only as an awakener of the whole of the society in which he lives, not only as a typical representative of the members which form society, but as a unique, not repeated individual, is to be the subject of the historian's attention; all earlier German historians, except perhaps Lehmann, must be assigned to the individualistic side. Bernheim and Lamprecht have asserted opposition to the older individualism and the latter has taken great pains to establish a theoretical foundation of collectivism (K. Lamprecht *"Alte und neue Richtungen in der Geschichtswissenschaft"*—1896). Lamprecht has defined the difference of his more modern point of view as contrasted with that of his predecessors in Germany. He has systematically laid down the psychological foundation of an eminently original treatment of history, but he has not been so original as to deserve to be designated as a Pathfinder or Founder of a new system of historiography as Prof. Harry Barnes (The New History) conceived him; for most of the important insights have been gained by him from his study of Ranke's *Ideology*. If, instead of studying what Lamprecht says about Ranke, we should study Ranke himself, we should get more directly the inspiration from one of the greatest historiographers of all times. No other historian of modern time has given the evidence of such profound erudition was critical acumen in handling the enormous volume of documentary evidence which became accessible when the archives of European governments were opened for the first time. But Ranke was also an artist in his historiography. Many of the other historians of his country produced works in which the clearness of the exposition suffered by an unnecessary weight of material and by abstract philosophical reflections. A certain degree of abstraction is unavoidable in historiography in order to bring historic panoramas and phases of activities that extend through long periods, sometimes centuries, within the comprehension of the reader who seeks information within a limited time. Further on I shall have more to say of the advantages and disadvantages of abstraction. And it will also become evident that what medical thinkers understand by the word *idea* is form and structure, whereas what the general historians understand by idea is the exemplar of a conception extending to the meanings belief, opinion, and doctrine. The term idea is the most celebrated word in philosophy and it is impossible for an author to decide upon a definition that will be agreed upon by medical men, historians, and philosophers alike. All I aim to accomplish is *first* to show the great philologic history of this word which necessarily would be a factor by its very evolution, and *secondly* to point out that medical men will find it expedient to be constantly aware of the meaning of the term in medicine and its meaning in history. The relation of the individual to the life of the community is the secret characteristic of any age in the history of medicine as well as in general history. Lamprecht, in his *"Alte und neue Richtungen in der Geschichtswissenschaft,"* states that in history, freedom of the will and necessity are intermingled, and in conflict, freedom appears mostly in personalities and necessity appears in the life of the community. E. g., the definition which Edwin Grant Conklin gives in his book *"Heredity and Environment,"* page 313, "Freedom is the more or less limited capacity of the highest organisms to inhibit instinctive and non-rational acts by intellectual and rational stimuli and to regulate behavior in the light of past experience. Such freedom is not uncaused activity, but freedom from the mechanical responses to external or instinctive stimuli, through the intervention of internal stimuli due to experience and intelligence." Now freedom of will and necessity are also very important factors in medical history.[1] It was through necessity that the modern methods of anesthesia were perfected. Artificial unconsciousness for the purpose of painless operation was practiced by Dioscorides in 64 B. C. The history of this subject will be considered in Chapter VI. I wish here to emphasize that it was necessity that constantly forced the human mind to concentrate upon this desirable method. Considered in this aspect we can say that necessity expressed itself from the needs of the masses. Here, then, a historic factor might be constructed as having arisen in the community. Other events could be cited to demonstrate the same occurence.

The two characteristic points in which Ranke's *Ideology* can be summed up are: first, an idealistic creed, inspired by the German philosophy of his day; and secondly, a very broad, almost universalistic conception of history. Here it behooves us (Merz, Vol. IV, page 574) to recognize how the comprehensive idea of humanity which inspired German thought since Lessing, Herder, and Goethe was replaced in the course of the 19th century by the idea of the aims and interests, not of humanity at large, but of the German people in their national existence. I introduce this thought to show how the idea of nationality can be broadened and narrowed down by various influences which Xénopol classes as *circonstances,* and these again are defined and announced by individuals. An idealistic creed, enforced by a ruling philosophy and a universalistic conception of history, powerful as they are in general history, are in medical history not the principal acting factors, but simply collateral forces. Medicine is the product of the mind of great thinkers, and whilst these are sometimes under the domination of the two factors just announced, this domination does not extend to a complete control of the methods of thinking of great minds in medicine, even where it seems to exist.

In Germany, as in France, there are two schools of thought in modern philosophy and also psychology. The first is the school of Wundt, who has devoted himself to the psychology of the collective mind. Secondly, the school of Dilthey, which devotes itself to fathoming the hidden depths of the individual mind, as revealed in the life and work of the creative genius. It will be seen that these two schools in Germany correspond to those of Dürckheim and Tarde in France. Up to the year 1926 the problem under discussion cannot be considered as having been solved conclusively.

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[1] I am using the word "necessity" here not as meaning a physical law but in the only sense that the masses can feel it—namely that of recognized desirability or urgent need.

As far as general history is concerned the interests of the best thinkers still oscillate between two mutually complementary problems, namely, first, the problem of society in its historical development and its effect on the individual; and secondly, the problem of individual life as it appears in great personalities. Lamprecht belongs to the school of Wundt, and all of his thinking leads to the subsumption that the highest cultural ages are without exception the expression of psychic phenomena of human communities, that is of the masses. This statement shows exactly where Lamprecht stands in the problem of collectivism or individualism. A decision between these two, as to which is the most influential factor in general history, is not possible. It seems to me that the difficulty will be found in a third factor which is a result of the constant interaction and reciprocal effect of these two on each other.

In medical history, however, the following chapters will demonstrate that progress has been due to individuals, to their great constructive ideas.

Paul Barth, in his Philosophies of History as Sociology, comes closer to the correct point of inquiry than any modern writer. He asks, how do the ideas originate which influence the actions of a great personality? Does he draw them out of himself in opposition to his environment? Or must they be strongly represented in the environment in order to become firmly located in him? He believes that the latter is the case. An idea which germinates in an individual mind, or is implanted by education, is not in itself capable of guiding him steadily in his activity. If the whole environment is opposed to it in its thought, or mainly moving in a different direction, then a definite idea can at best only generate an isolated activity which will soon be extinguished by the weight of what is foreign to it. Paul Barth is here manifestly speaking of ideas of general history, political ideas, economic ideas, not of the ideas of science, for in science great constructive ideas, far from being extinguished by opposition, are frequently fanned into flames by it. Opposition is very often the very food upon which a great scientific idea grows, for where there is no opposition there is no interest. Herbert Spencer entertained similar views, but the fact that both Barth and Spencer frequently used the word "hero" indicates that they are referring to political ideas, or military ideas.

We must descend from these expansive philosophic thoughts to more definite notions if we wish to make any progress in solving the problem of collectivism or individualism. And the conviction has been forced upon me that the study of the history of medicine offers the most promising field for the investigation of this profound problem, but our point of inquiry, the *Fragestellung,* has to be made as precise, clear, and definite as human thought and expression can make it. Now the question then is, is the evolution of medicine due to the brain work of indivduals, or to that of communities at large? And in the subsequent chapters it will be shown, not only that it is due to the brain work of individuals, but that the masses, so far from aiding in the practical application of great constructive ideas, have frequently actually opposed and inhibited such application.

The Use of εἶδος and ἰδέα in Hippocrates

Prof. A. E. Taylor, in his *Varia Socratica* Part I summarizes the different meanings of εἶδος and ἰδέα as used by Hippocrates under four headings. The nucleus of all these meanings corresponds to our English words *form* and *structure* and there is no distincton made between εἶδος and ἰδέα which appear to be used interchangeably. (The use of εἶδος and ἰδέα in Hippocrates by G. M. Gillespie in the Classical Quarterly 1912, VI, pp. 179–203.)

I. The first meaning is taken directly from popular usage. The words are here applied specifically to human beings, in the sense of *bodily shape* or *form,* primarily as it presents itself to the eye, but extended to the structure and constitution of the body as a complete thing, not as merely seen. *Physique, body, constitution* are the proper renderings of the words in this application.

II. The words are also used more widely for the form of things other than the human body. This group of meanings contains, as might be expected, considerable developments. (1) Sometimes the outer visible structure is emphasized, when the words may be rendered by shape, "in shape like a trough." (2) Or the inner structure may be included: form, nature, like φύσις. (3) They may be extended to objects which are not in themselves bodies, and in this connection may have a semi-classificatory sense, forms or types of disease, etc. But Prof. Taylor insists that they never mean class or species, as εἶδος does in Aristotle.

III. This use is specifically medical. It emphasizes and extends the appearance-side of the meaning, till the words stand for symptoms of disease, the form, shape, structure, etc., of the observed body or limb as it appears to the observing physician. I gather that Prof. Taylor regards this usage as underlying the employment of ἰδέα by Thucydides,

IV. In the fourth use, the bodily side of the first and popular meaning is emphasized. (1) έιδος is used of the stuff, matter, substance, of a thing, e.g., of the heart and kidneys. And more especially it is Professor Taylor's object to prove that it is used (2) in a highly technical, metaphysical sense to denote the ἁπλᾶ σώματα, the elementary bodies. To say that ξήρ is an εἶδος is to say that it is one of the ultimate unanalyzable bodily constituents of the κόσμος. It is thus nearly equivalent to the later οτοιχεὶον. This meaning he supposes to have come into medicine from the φυσικοι; and to explain how the word εἶδος can advance from the meaning of form or structure to that of primitive substance, he suggests as the connecting link the Pythagorean doctrine that the difference between one kind of matter and another is that of geometrical figure (p. 246). Because the Pythagoreans regarded geometrical figures as the ultimate substances, the word was extended to mean things or substances. I am not quite sure, but I think that Prof. Taylor considers the use of the word as the matter or stuff of an ordinary empirical object to be derived from this. (See also John Burnet, *Greek Philosophy,* Part I, Thales to Plato. Macmillan, 1914.)

It has been stated that the term idea is the most celebrated word in philosophy, and to emphasize this, I will introduce a historic episode from the life of two of the greatest historic thinkers, poets and naturalists that the human race has produced. Both of these men had studied medicine, and yet were naturalists, poets, philosophers, and historians at the same time. They were the German poets, Goethe and Schiller. A separate and important chapter (XXIX) will be devoted to Goethe as a naturalist. Schiller was a doctor of medicine. In 1778 he had won the first prize in Anatomy in the Carls-schule in Stuttgart (see Carl Hoffmeister—Schillers Leben—edit. by Heinrich Viehoff, Stuttgart, 1846. I. P. 53). In Dec. 1779 he was awarded 3 further prizes—one in Therapeutics, two in Medicine and for a time served as a regimental surgeon, being later appointed Professor of History in the University of Jena. His opening lectures bore the title *Was heiszt und zu welchem 'Ende studiert man Universal Geschichte?* (Jena, 1789.) It would lead too far to even mention his many historical studies, most of which, like the history of migrations and the crusades, and his history of the Thirty Years' War, in fact all of the contents of the tenth and eleventh volumes of the Cotta Stuttgart edition are historical classics. These two illustrious thinkers, psychologically of entirely different temperaments, socially of different strata, had *been very intimate friends*—a *friendship based upon a mutual understanding* that had to go through a gradual evolution of many years, and yet they became alienated and for a time the friendship was severely strained because of one word, and that was the word *idea*. As these two men have played such a great rôle in history, as well as in science, and as this controversy that separated them for a time throws such a brilliant light on the significance of the word *idea*, I consider it important to introduce the entire event.

Goethe remarks that Schiller was thoroughly unsympathetic to him in the beginning, and Schiller's essay concerning Grace and Dignity (*Über Anmut und Würde*) had been a consequence of his study of the philosophy of Kant, and in this essay he had represented the attitude of man to nature in a way which did not compliment Goethe's conception of the same relation between man and nature. Goethe remarks as follows: "Certain hard passages I could interpret as applying directly to me, and they showed my confession of faith in a false light. It indicated a tremendous chasm between our two modes of thinking. There seemed to be no conciliation. No one could deny that between two intellectual antipodes a difference existed, greater than the diameter of the earth." Rudolph Virchow in his article on *Goethe als Naturforscher,* Berlin, 1861, page 43, narrates that Goethe had come to Jena to hear the lectures of Prof. Batsch, who had founded a natural history society, and in walking out of the lecture hall Schiller and Goethe met as if by accident and began to converse. This occurred on Nov. 2, 1794, but it is certain that both poets had met in the Spring of 1794. Schiller criticized the manner of presentation of Prof. Batsch, designating it as fragmentary and disrupted, and that such a style of treating nature could not appeal to the layman, whereupon, according to Goethe's own report of the incident, "I replied that it (Nature) might perhaps remain a secret even to the initiated and that it was probable that there was another way of studying nature, not by separating it into its parts and taking them up singly (abstraction), but to consider it in the act of effecting things, in working things (Wirkend), in the act of living, that is presenting it as working from the totality tending into the parts. He (Schiller) wished to be instructed on this view, but he did not conceal his doubts, especially he could not confess that such a method of observation emanated out of experience, as I asserted. We reached his house and the conversation enticed me to go in, and there I presented the metamorphosis of the plants in a very vivid manner and with many characteristic pen delineations, I created a symbolic plant before his eyes. He listened to and looked at all this with great sympathy and evident power of comprehension, but when I had finished he shook his head and said *That is no experience; that's an idea*. I was *dumbfounded* for an instant and somewhat displeased, for the point that separated us was thereby indicated in the most exacting manner. His assertions from the *Anmut und Würde* occurred to me again and the old aversion wanted to rise, but I pulled myself together and replied: 'That is very pleasant to me, that I have ideas without knowing them, and even see them with my eyes.' "

The sharp contrast between these two men could not have revealed itself better than in this dramatic conversation, and Goethe could not have been wounded deeper than by Schiller's poignant objection, that he was building up a symbolic plan which was a product of his idealism and for which there was no evidence that it ever had been or would be materialized in nature. Here we have a new understanding of the word *idea* and one that fits into modern medical history very well. Schiller contrasted it with experience. As a result of all of our conceptive faculties we can form experiences, but these are conceptionally more real and above ideas. In making experiments in biology, the investigator, as he formulates his point of inquiry, is guided by a number of ideas, which he follows up consecutively, first the one and then the other. Should one or more of these ideas become verified by the objective experimentation, it results in experience. Sometimes the entire experience is a negative one, meaning that none of the ideas have been verifiable. For us as historians, then, we see here a beautiful demonstration of one of the latest meanings of the word *idea*. Incidentally, and connecting with my article on *Goethe as a Naturalist* (Chapter XXIX) we are learning that up to the time of that conversation with Schiller, which took place in the fall of 1794, Goethe was actually conducting his nature studies with an unconscious naïveté. He had trusted to his sound five senses, deceiving himself that all his results had been won by pure experience, and had underestimated the ideas and the thinking operations which had been necessary to attain these results. His imaginary realism had gone so far that he had attempted to find his *Urpflanze* (typical aboriginal plant) actually in nature. And now he was unpleasantly shaken up out of his illusionary manner of observation. Nearly ten years he had been occupied in studying the single phenomena of different plants, and then he had begun the attempt to resynthesize all these so-called experiences; but that this process, which was analyzable into induction and deduction, was really a thought-operation had not become clear to Goethe. He had lost himself so far that he imagined he could see the plant metamorphosis with his eyes. Schiller prove to him, by that portentous remark, that his law of plant metamorphosis started from experience but that the conception of a universal typical plant was an abstraction by Goethe's thought-operation, and that Goethe's deduced morphologic connection of all plants was really an idea.

The effect of this historic conversation on Schiller was as striking as the effect it had on Goethe's manner of thinking. While Goethe was submerged in the method of thought-abstraction and not even conscious of it, he nevertheless executed well planned experiments. For instance it was he who planted seeds and watched their growth under glass of various colors, he who measured the length of the roots, stems, and the leaves, he who allowed plants to grow in the dark, when no chlorophyll is formed, and was one of the first to observe what Sachs and Jacques Loeb studied under the name of *Heliotropism*. Nevertheless, and in spite of such experiments, his results were derivatives of thought operations. Of this he was gradually convinced. I am narrating all this because it is not only a part of the methodology of history writing to understand what thought operations are and what the modern meaning of an idea is, but because both Goethe and Schiller had had a medical education, and the former had made very important contributions to anatomy and morphology. We can learn more from this controversy if we apply its lessons to the process of history writing instead of opposing the concepts *idea* and *nature investigation* or, let us say, instead of the latter word, *historic* investigation, which is really a part of nature investigation. Now the idea is independent of time and place, but historic investigation is limited and restricted in time and place, therefore, in an idea we may have an intimate combination of things or events that are both simultaneous and successive as regards time. We can picture to ourselves by ideas things that happen immediately, in a specified time, and things that took long epochs for their development; but in historic research that which occurs simultaneously and that which occurs successively must first be held apart, and if we wish to derive a comprehensive understanding from such simultaneous and also successive events we have to resort to an idea, so that in that case the operations with ideas as a method coincide with the operations of historic research.

Later on Goethe placed himself without reserve on Schiller's standpoint, and in 1817 he declared that his so-called aboriginal plant or the *urtypus* of a plant was nothing but an artificial schema which would serve as a tentative architectural plan for the comparison of all other plant forms. On the other hand, Schiller by the combined experimental and introspective methods of Goethe was torn out of an exclusively speculative method of viewing nature, so that he in turn was greatly benefited by his discourses with Goethe. It is of interest, however, that when Goethe's manuscript on the Metamorphosis of Plants was completed, the publisher who had brought out his former works declined to undertake the publication. He seemed to be correct in his estimate, for the public who had received Goethe's poetic works with appreciation could not understand such broad versatility, and met his *Metamorphosis of Plants* with the remark of Apelles to the cobbler, "Ne sutor ultra crepidam."

What Plato meant by *idea* is not the same thought content as we of the year 1925 understand by the term.

John Burnet in his Greek Philosophy, Part I, from *Thales to Plato,* London, 1914, p. 154, presents a scholarly exposition of what Socrates and Plato meant by this word. But Burnet does not give this chapter the title "Ideas," very significantly he entitles it, "The Forms."

On page 155 Burnet introduces Plato's *Phaedo,* where the theory of ideas is said to be first clearly expounded. In *Phaedo* the participants of the symposium (Socrates, Simmias, Kebes, Echekrates Phleious, Appolodorus, and Crito) in this conversation bring out the following—

A sharp distinction is made between objects of thought and objects of sense. Only the former are said *to be* or have *being,* the latter are only *becoming* (tending to be). If we now substitute our modern conception of *idea* in place of *form,* we get the meaning that only the ideas are real or have being. The things intermediated by the senses are not real—they have no *being*—they are only *becoming.* Thus in Plato the world of conceptions usurps the place of the objects of nature, which he implies are in constant state of change.

Objects of sense change continually, but the content of thought is permanent, not affected by change, nor afflicted with inherent contradiction. So conceptions and ideas are given reality and permanency. Objects of the senses are transient, changeable, and not real. This view of the terms has undergone many modifications and the history of the theory of ideas is one of magnificent and heroic striving of the human mind to attain clearness about itself and the world in which it exists.

For pragmatic purposes we might make the inference that the idea of a thing or event is more real and has a more enduring existence than the thing itself, and in history this is borne out by the observation (as well as experience) that the ideas of things, events, and processes, etc., continue to exert their influences and act as historic factors long after the things, events, etc., themselves have passed out of existence.

Fortunately so for the history of medicine, for some processes of thought, or ideas, or problems of the past, revealed themselves of such momentous laboriousness and difficulty that the greatest medical thinkers, generation after generation, failed to solve them, and the record of ceaseless endeavor on the part of these generations of thinkers constitutes not only a glorious monument to the altruism, ingenuity, and courage of the asclepiads of those days, but also the greatest encouragement to humanity as a whole to have faith in its own destiny—this also is an object of thought, because of its historically proven reality and persistence.

When I discussed the Hippocratic meaning of ἰδέα and εἶδος (such a discussion in a work on medical history is not merely a necessity, it is also a duty), it became necessary to call attention not only to what Hippocrates meant by idea, but also to a fundamental tendency of the human intellect not only of his time, but of all historic times, namely the tendency to formulate words and consequent concepts in terms of *abstractions*. An abstraction is a mental expediency, operation, that consists in leaving out one or more qualities of a complex object to concentrate attention upon the remaining ones. If I may be pardoned for explaining a process by employing this same process, I should say that abstraction is the quality-pruning of things, followed by mental stenography.

In these mental expediency operations, however, we generally imagine that we have grasped the complete thing or event itself, whereas, in fact, we have only intellectually stenographed an abbreviated type of it. In historiography this is frequently done unconsciously when writers take their ideas not from original sources, but from other authors who in turn may not have studied the original sources. An author is sometimes compelled to resort to abstracts because of limitations to the size of his work, or to the endurance of his readers. The danger of abstraction consists in the fact that the abstraction is clothed in the same verbal garb as the objects of perception themselves, and therefore, the "pruned down," "stenographic" skeleton of the object, event, or thing is without further discernment taken for the real object in its entirety.

Object and reality are designated by the same Latin term, the word *reality* coming from *Res,* meaning *thing* or *object.* Hippocrates in his περὶ τέχνης argues about Medical *Art* as a reality, meaning and implying at the same time that this Art has the quality of objectivity or objectiveness.

This confirms what I stated in the chapters on Methodology concerning the indispensable requirement of complete control by the historiographer of medicine of at least three modern (English, German, and French) and two ancient languages (Greek and Latin). Indeed a fourth modern language, namely Italian, and a third ancient language, Hebrew, should be added.

The spoken and the written words in every language constitute a treasure house into which uncounted generations of nations and races have deposited the results and products of their mental activities in formulating concepts. I do not believe that we have at the present time even, reached perfect unity of expression by words, with the thought content we intend to present by these words. In medicine, for example, we still preserve forms of words to denote anatomic structures, words, coined by Aristotle or even before his time on the basis of conceptions that were grave errors—for instance, the word *artery* (ἀρτηρία) because the ancients considered these vessels as filled with air (ἀήρ=air). It took a millennium of research and experimentation to demonstrate that arteries normally carried blood and *away* from the heart, but still the use of the erroneous word persists.[2] There is undoubtedly a slow evolution of language and a struggle for existence among words, whereby eventually the word will coincide entirely with the mental concept. Medical historiographers should aim assiduously to promote and accelerate this process, so that medical language shall represent a classification and order of concept-names and their relations, in order that meaning, concept, and word shall coincide. An enormous amount of mental energy and time are lost because concept and word do not exactly supplement each other. We have escaped as a race from the blind alley of medieval scholasticism in which unmeasured amounts of mental energy were lost without any productive advance whatever, only to find ourselves in a labyrinthine confusion of terminology in modern and medical and biologic specialization where almost every advanced research worker feels privileged to invent his own words and often show three greatly detracting defects: first, the new word does not correspond to the mental concept; secondly, different investigators use the same word in different senses (two or more concepts are covered by the same word); thirdly, disregard for the historic evolution of the word.

I prefer not to make specific references (to works already in print) but nearly the entire new literature of the last fifteen or twenty years on *Hematology* and also on *Genetics* exhibit these tendencies. The modern publications on Psychology demonstrate an inclination to production of terms with these defects, and what is more regrettable, the new words are often not necessary, and the intentions of these authors would be made clearer by the use of words already in common usage.

THE CONCEPTION OF THE SOUL, INDIVIDUAL AND COLLECTIVE, AND ITS RELATIONS TO HISTORIC IDEAS

In a work on Medical History it is impossible to enter into the history of the thought complex, designated by the word *soul*. The readers are referred to the treatises on psychology, ancient, and modern, for the various meanings and concepts embraced by this term. One of the best, concerning the Greek thoughts on this subject, will be found in John Burnet's *"Greek Philosophy,"* and in the splendid work by John Theodor Merz, entitled *"History of European Thought in the 19th Century."* Here there is a chapter extending from page 192 to page 293 (of Vol. III), and in the end of this highly instructive article, Merz informs us that the highest psychological problems lead us out of psychology into other and more general regions of thought. Hitherto we have believed that the subject of soul could be approached only by introspection, now we are told that psychology proper has nothing to do with the soul, that is, with the essence of the inner life, but that the modern psychologists find it necessary to approach this subject from an outside, not a purely psychological, point of view. We are, in fact, referred to the question of what constitutes reality, and this is evidently a problem of metaphysics. The medical reader is confused as to how he is to understand the dilemma of the psychologist, for while they refer us to a discussion of reality, a metaphysical problem, they admit that the thinking mind imports into its consideration of every matter with which it deals such terms as *unity* and *continuity,* and Merz, accordingly, states that psychological analysis (l. c. page 293) leads on the one side to metaphysical discussions and on the other to a more minute examination of the methods of thought, and the processes by which we acquire our knowledge. The problems of knowledge and reality lead the historian too far, except in such knowledge and such realities as are distinctly and technically historical. Notwithstanding this, the soul plays an important rôle in what I may call the etiology of history. Some still hold that the soul is nothing but an auxiliary conception for the individuation of our consciousness. This definition carries with it an unexpressed factor, to which a large part of medical readers will instinctively object, I mean, those medical readers who are religious, for the definition implies that the soul is produced by our own conceptions. Another definition, and one of the most recent, is that the soul is a living entity inherent in man, bearing within itself the processes and conditions of conception, feeling, as well as the activities of thinking and willing, and above all, that it is conscious of these conditions and acts which are its life processes.

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[2] This word αρτηρία may have a different etymology. It may not come from άήρ (air) but from the verb ἀίρω to lift up, sustain, to bear or carry. This meaning is clearly found in Greek Old Testament in word ἀτήρ as used by the Septuagint (Nehemiah 4:17) καὶ οι ἀίροντες εν τôις ἀρτηροιν εν ὅπλοις.

In historic research the soul is a reality, by which, I mean, that the individual along with the collective soul is an integrating and productive factor in the creation of history. I cannot agree with Taine, *Sur L'Intelligence* (Livre IV, Chapter III), who disputes the continuity of the soul and rejects all investigations concerning the activity of the soul, terming them "Phraseologic Illusions," but he admits in a not very intelligible way that there are rudimentary souls, in different phases of development, which he conceives as living in the ganglia of our body (?). William James, in his "Principles of Psychology," has brought together almost everything of importance concerning immediate consciousness and self observation that can be consummated by thought operations exclusively, and that which is also the result of psycho-physic investigation. Professor James was eminently fitted to treat this subject in a felicitous, sometimes even humorous, manner. He denies that the feelings of pleasure and displeasure, the ethical and esthetic principles, can be analyzed by the apparatus employed in psycho-physics. He also does not acknowledge a free will, but admits that there is something more than the products of the apparatus of sense and of reason, but this must not have an individual origin. I have not been able to grasp definitely what he wishes us to understand by this "something more," but he does not admit a "thought continuum," a ceaseless streaming of thought. This is a general or collective thought stream which sets into operation the machinery of thought in our brains. Just as I spoke of Mozart's method of composing, by expressing it in the words "something is composing within me," so I understand William James as meaning "something is thinking within me."

INDUCTIVE, DEDUCTIVE AND FICTIVE THOUGHT METHODS,—THEIR SUITABILITY TO HISTORIC PURPOSE

In the continuous world process, medical science has developed from prehistoric beginnings to its present status for the purpose of preserving the human race. Objectively considered, everything that medical men do or have done in the never ceasing changes of this process has aimed to produce results that were intended to be of enduring utility and value. All such results can be designated as *conforming to the purpose* of medicine and it is not always necessary that the medical workers and thinkers that are tending toward this purpose shall have the consciousness of purposeful action in their minds, but it is absolutely necessary that such workers should be mindful of the contemporary and preceding status and course of the medical changes which they desire to influence. This is another characteristic in which medical history and general history differ, for in general history great changes are often effected by persons who appear to act instinctively, unconsciously, without a knowledge of the inner connection of their action with the needs and intentions of their period. Many historians have remarked about the sureness and the unreflected practice of what is called *common sense* in general history, where apparently untrained persons accomplish purposeful ends without long previous reflection, but in medical science such practice is so extremely rare that historically it must be classed as exceptional. The medical worker and thinker must be proficient, not only in his own proper science of the day but must control to a considerable extent the collateral sciences; and when we get to the writing of medical history we have to become familiar with the methods of logic, with a view to deriving inferences and conclusions, and testing them. There are three such methods the meaning of which will have to be repeated here because they are of value where principles of medical history writing are concerned, as well as to reach some concrete understanding about the methods of medical science.

The three methods are *induction, deduction,* and the *fictive method.* Induction is the act or process of reasoning from a part to a whole, from particulars to generals, from the individual to the universal. Deduction means reasoning which is drawn from premises that are fully understood. These two methods are contrasted as follows: Induction is the inference viewed from the side of differences and deduction is inference viewed from the side of the universal with regard to seeing agreements; but both methods lead to inferences only, not to conclusions. The third or fictive method does not proceed directly to its object; it is a method of thinking that leaves the orthodox direct paths of induction and deduction and proceeds by a circuitous route. It is "detour" thinking. It builds many provisional bridges and scaffolds which are broken down later when the result has been accomplished. These bridges and scaffoldings are the manifold fictions which human thinking makes use of and which have to be very carefully studied, for fictive thinking is one of the most important methods, not only in medical historiography, but even in experimental medical science. First of all it must be strongly emphasized that the fictive method of thinking is not to be mistaken for a system of deliberate falsehoods or deceptions which would justify a critic to say "This kind of thinking is fictive and therefore it is valueless and it is not worth while to consider it." Fictive thinking constitutes a very great part of human thinking for the simple reason that man has to resort to it after inductive thinking and deductive thinking have been tried and have failed. Some critics of the fictive method of thinking have declared that it is incapable of leading to a perception of reality. That would indicate that the purpose of all thinking is to lead men to knowledge of reality.

How incorrect this conception is we find out when we reflect how few the realities are which can be ascertained by the thinking process alone. It is my personal opinion, that the process of thinking is an instrument that has been given to human beings for their own practical purposes. Its purpose is not exclusively the acquisition of knowledge of reality, its purpose is to enable man to make practical calculations of the life factors for the purpose of technical and moral orientation in the world in which human beings must live. By orientation I mean adjustment to ascertained direction; in other words orientation by thinking constitutes our mental compass on the seas of life. Thinking has been arranged into the order of the world process to be a guide to us. It is the highest blossom of the world process. It may serve for the purpose of knowledge of realities, but the far greater purpose it serves is the one that I have described as orientation. Heraclitus (513 B. C.) had one thing in common with the religious thinkers of his time; it was his insistence on the idea of the soul and he emphasizes that the soul is not a ghost or a shade but the most real thing of all. He also said *"You cannot find the boundaries of the soul—even if you wander every road to its end—so deep and full of profound meaning is it."* (Quoted from Bywater, Heracliti Ephesii Reliquieae. Oxford, 1877, Fr. 71.) From Heraclitus' time until the present day all the arguments and philosophies of many brilliant thinkers have not been able to efface the concept of soul from historic causality. The most modern psychophysic investigations tend to eliminate all material of experience, that is not capable of subjection to experiment by apparatus. And thereby also the self-consciousness—the "ego-awareness"—and all self-observation is threatened with elimination.

Practically the human soul—collective and individual—cannot be eliminated as historic factors. Lotze (1817–81) following Leibniz had the rare and eminent quality of combining common-sense aspects with metaphysical philosophy. I refer to Lotze not only because he was the only thinker of his time who through individual taste, thorough education and ingenuous devotion to psychologic and philosophic problems became the possessor of encyclopedic knowledge and at the same time of a brilliant gift of exposition.

I also refer to him becaues he approached philosophy from the side of medicine. His earlier writings counteracted the vagueness of expression of contemporaneous medicine by introducing the clearer definitions of mechanical science. Perhaps all historiographers should nurse the purpose of preventing the work of such eminent pioneers of thought from sinking into oblivion even in domains of knowledge with which their labors apparently have no immediate contact. Lotze in addition to the mental characteristics just mentioned was an equally balanced personality evidencing to a high degree what Plato designated as σωφροσύνη—the psychic balance, the self-culture and self-training that is born of a feeling and conviction of ones own personal dignity. In the *Sophrosyne* there is not simply an outward dignity or noble reserve and repose, but also an ever-ready willingness to enter into the thoughts and perspectives of others and to be guided by them. This *Sophrosyne* is the great desideratum of all medical historiographers, not possessed by all, but consciously or at least unconsciously aimed at by all.

What does this medical philosopher conceive the Soul to be? Lotze sees in the soul a *thought* of the infinite (*einen Gedanken des Unendlichen*), but such a thought that in every individual soul develops into the manifoldness, just as if into its natural consequences.

The question is: Can the historiographer operate with such a concept? The answer is that some of the most effective modern historiographers have operated with it and very successfully.

Many biologists have taken exception to the distinction between the terms natural and supernatural. To quote one of them, my own teacher, W. K. Brooks, stated "Nature is all there is." This excludes the concept of the supernatural, but it does not necessarily exclude the meaning of the spiritual principle in nature. Nature is a texture in which the mechanical warp is shot through everywhere by the spiritual woof. The term *supernatural* has, therefore, lost its meaning in the opinions of some modern biological thinkers. The spiritual principle is not above or something superior to nature, but everywhere permeates it. Kant's "Unity of Apperception" refers all the phenomena of consciousness, our subjective experience, to an inner unity of thought and personality. No one could assert that this was a purely mechanical process. It is, as much as anything else, a natural and everyday phenomenon. That which is spiritual is not necessarily supernatural. It may be conceived as being inside and all through that which is mechanical. It underlies and permeates the seeming mechanical connection of things. The spiritual, according to this view, is also perfectly natural, being an integral part of the whole creation.

"The main philosophic tendency of Lotze's writings may be best expressed by the term used, and suggested by one of the many happy and illuminating dicta of the late William James. That small portion of the universe which is accessible to our experience and observation is in Lotze's sense a *texture in which mechanism* is the *warp*, the *threads of which are everywhere to be seen or looked for, whereas the spiritual is, as it were, the woof which weaves into the system of mechanical threads the actual pattern or living image.*"

It is not necessary for historic causality to accept a manifestation of the Divinity in every individuality that possesses a soul. Such a mental attribute is an after effect of transcendental philosophy which gave to the complexes of individual soul life an ethical and psychic evaluation that was equivalent to reverence.

It is also unnecessary for the understanding of soul as causality in history to enter into the philosophic discussions about Monism, Dualism and Pluralism, etc. Let it be said, once for all, that the historian cannot get away from the conception of soul as actuating human beings. In thinking of man as a material-spiritual unity we have to use a term for his physical entity, namely, the term *body* (corpus) and another for his spiritual being, namely *soul.* And in studying *Master Minds in Medicine* historically these conceptions are of equal importance.

In an essay by Wilhelm von Humboldt on the Problems of the Historiographer (Kgl. Akademie der Wissenschaften zu Berlin 1820–22, s. 307) this famous historic philosopher states, "Everything depends on the assimilation of the researching power and of the object about which the research is made." The fuller and the deeper a historiographer penetrates into humanity, the more thoroughly he understands the effects of humanity by genius and study; and the more humane he is by nature and circumstance, the purer will his own humaneness work out his problems, and the more perfectly will he attain the aims of his endeavors. Comte has used the word *Milieu,* by which he means the total ensemble of exterior circumstances of any generation necessary to the existence of each individual organism in it. The Germans call this *Die Umwelt.* In this "Milieu" or "Umwelt" are contained the forces that produce history, but in this milieu historiographers find various factors and there has been a tendency to emphasize at one time the natural environment, at other times the material requirements, at still other times the ideas of the masses as the principal causes of the historic process. It depends upon the man who writes the history. A mathematician like Comte might emphasize reason and calculation as the main factors; an economist like Marx might emphasize the economic factors of production, consumption, and the means to effect these two, as the principal factors; a geographer like A. V. Humboldt might emphasize the physical conditions of the territory of a race or nation; and the medical historiographer unfailingly will emphasize all those conditions and requirements that preserve the health of the race and cure disease.

In speaking of the soul and acknowledging it as an indispensable factor in history I have to call attention to the fact that two well-known historians namely, R. W. Emerson and Count Gobineau speak of an *over soul.* Houston Stewart Chamberlain also has such a notion. However one can hardly call any of these men historians—they are pseudo-historians. In Emerson we miss a convincing argumentation, a logical connection, and even the order of his assertions is not that which could be called historiographic. To what extremes such historiographers may go can be demonstrated in the following sentence quoted from Gobineau (*Ungleichheit der*

Menschenrassen, Vol. IV, page 299): "Thus creative elements are developed which are above the individual and the masses. They are independent of these. There exists a controlling, dominating circle in the world of immaterial things—activating, vivifying forces move therein, which are in constant and reciprocal connection with the individual as well as with the masses, the intellectual and spiritual being of which contains entirely coincident particles, whereby the individuals as well as the masses are equipped and fitted to receive incentives from this circle of dominating forces." Further on, Gobineau compares this Over-Soul of human society to the cosmic forces like electricity, or magnetism, which, like the great Over-Soul, cannot be ascertained in themselves or by themselves, but only can be judged according to their effects. The Over-Soul is not an issue of human or political bodies, it lives *for* these bodies and the Over-Soul can live only as long as it has a purpose for existence. By this, he means, that as soon as human society dies out the Over-Soul must die also.

In its essence such a conceptional hypothesis has been expressed by the ancient Greek philosophers. In fact there are pronouncements concerning it to be found in Plato. In my search for factors that could be used in explanation of the causes of medical progress and regress, of medical discoveries, and of medical originality, it was very difficult to remain within regions of knowledge which the medical man could command and immediately make use of for an insight into the method of operation of great medical minds. The consummation of the studies that we have made concerning ideas and the soul would lead me to incline to the view that *truth,* which is also one of the great things that medical science endeavors to reveal, however it may be conditioned by the facts, discoverable to analysis, is, in the *last resort, to be attributed to the operation upon man of a spiritual power that is above him,* and *once having acknowledged such a spiritual power it is only one step further to conceive a sort of control or guidance of this power in the development of civilization.* In brief, this is a view of Plato.

In other chapters concerning medical genius and its psychography, it has been stated that the great mind has the vision and the inspiration, but these very words suggest that what he sees and feels and thinks seems given from without, at least so far as it appears as a vision of the truth. *The truth is something that accords with the nature of things and in it this nature is self-revealed and the great individual is simply its minister and interpreter.* Such views as I have so far expressed are helpful in reaching an understanding as to what inspiration is, but it is doubtful whether such views have any place in science. Science does not deal with ultimates, but is concerned, as it seems to me, with the discovery and putting together of relevant, observable facts, in other words, with the analysis and synthesis of observable things and conditions. When, therefore, for example, it treats of the development of ideas, it proceeds just as it does when dealing with the development of any physical thing; it collects all the relevant observable facts bearing on the problem, takes account of the instances of the same or similar ideas already announced, of the cultural conditions—situation, endowment, social relations, etc.—of the individual who makes his contribution, his epoch-making discovery, let us say, and having done so much is content to let it go at that. *But there is always a residuum, a mystery.* The psychic *Anlage* of the individual is not completely exhausted by details of heredity which modify it, and certainly the mind of the concrete individual is more than the sum-total of the circumstances which make up his environment.

I have repeatedly stated in the foregoing that some of the greatest minds that the human race has ever produced have expressed the conviction that ideas are the truly effective factors of historic life. This has been emphasized by Kant and his School, also by Wilhelm von Humboldt, by Fichte, by John Dewey. The sub-conceptions of these men may differ, but in the essence their meaning is that the development of history is largely a consequence of the evolution of ideas. Kant and his School think that ideas are effective mostly in a conceptional and constructive sense. Wilhelm von Humboldt inclines to the belief that they are effective in an organizing direction, whereas Fichte conceives them as ethical factors. Bernheim states that the Parliamentary expression for ideas is *Imponderabilien* (having no weight). One comes across this term in the records of the transactions of foreign parliaments. The same records actually demonstrate the effectiveness of ideas in having too much weight. Ideas are actually proven to have inhibited and checked parliamentary movements that were well under way when they met with an established idea as an obstruction. In the history of medicine this power of ideas has sometimes been portentous, for example, the Galenic idea of the permeability of the septum of the heart which retarded heart physiology for many centuries. It is my personal conviction that the tyranny of the Galenic fetish frustrated Leonardo da Vinci and Vesalius from recognizing the truth concerning the structure and physiologic action of the heart. This intensely interesting effect of the obstructive action of an idea has been dwelt upon exhaustively in the chapters on the Discovery of the Circulation of the Blood, in the chapter on Lavoisier and the Physiology of Respiration, and in the chapter on The Physiology of Leonardo da Vinci. At the present time (1925) historiography is inclined to neglect the importance of ideas and ideal moments. As a result of a special study of medical history I am forced to agree with Bernheim (l. c.) that ideas, even in this practical and strenuous age, are not to be regarded as phantoms or ghosts, things that have no weight (*"Imponderabilien"*), which one might believe or disbelieve as one wishes. But they are highly real and effective historic factors. See: H. Berr, *Le problème des idées dans la synthèse historique;* Herbert Spencer's *The Principles of Sociology,* Vol. I, Chap. 8; Lazarus, *Die Ideen in der Geschichte* (Zeitschrift für Völkerpsychologie und Sprachwissenschaft 1865, Bd 3, S. 385–486). These writers, of course, refer to ideas in general history. The medical thinker will find abundant evidence of the effectiveness of ideas as historic factors in the preceding part of this chapter and in the other biographic and psychographic essays of this volume.

When one reads the slow ascent of medical practice from ignorance through gradual illumination to such large systems of medical history as that of "Handbuch der Geschichte der Medizin," by Max Neuburger and Julius Pagel, or in the many volumes of Karl Sudhoff's "Archiv für Geschichte der Medizin," or in Garrison's "History of Medicine," a thought sometimes springs up in the searching reader which is not expressed in any of these volumes but yet is more important than any thought they do express and is as follows: Practice demands that the phenomena should be briefly narrated and the development of the use of therapeutic expedients should likewise be briefly stated. Brevity here is the essential feature and it is forced on the teacher of the history of medicine particularly. This has the advantage of avoiding too many differences of opinion, but it has the disadvantage of giving no incentive to personal afterthoughts of a student of medical history. Nor can it possibly give an incentive to individual creations in medicine or more especially in the history of medicine. My reason for giving such a long chapter on ideas as historic factors was that all real creation in medicine springs from ideas and, according to Goethe, it can be said that whoever has no more ideas will soon have no more conceptions. It would have been much simpler to content ourselves with an arrangement of historic conceptions and an orderly recounting of medical phenomena from which these conceptions are deduced, but in the chapter on ideas, it must have become evident that ideas lie at the very foundation of conceptions and also extend far beyond them. Another difference between conceptions and ideas is the following: Conceptions are accessible to everyone and they are more likely to yield the same thought in everyone, but ideas are not accessible to everyone or perhaps more correctly (See Chapter IV) everyone is not so accessible to ideas. One human being sees much further than another in ideas. In fact, the evaluation of different human beings consists in the power and capacity for grasping ideas, and genius is not measured according to conceptions, but according to ideas, and geniuses among themselves are measured by the number and the extent and expansion of their ideas. There is no better way to understand the manner of creation and of working of pathfinders in medicine than the immediate acquaintance with their ideas and then with their methods of investigation. There is a very specific attractiveness in studying this in the master minds of medicine and the historiographer has not been born who could reproduce that lifelike freshness and originality and clearness with which the minds of great men operate. One can get a glimpse of (some of) these qualities in some of the "Vorträge und Reden" by Hermann von Helmholtz, especially the one entitled "Das Denken in der Medicin, 1877," and also the essay on "Die Thatsachen in der Wahrnehmung (1878)." Here we have a star of the first magnitude illuminating prophecies in stars of the other magnitudes. Here we learn that ideas can really extend far beyond what can be grasped by the conceptions. They begin before the conception and extend after it. Medical discoveries, new doctrines, medical advances of all kinds, when precisely defined, present to us that which has been accomplished, that which has been achieved, but that which has been striven for escapes us, although it has a far higher cultural value and if every master mind of medicine had left a record of what went on in his brain while he was wrestling with a problem, something like what Mozart left to us in his divining of great musical compositions, we would not only have an increase of our knowledge of medicine, not only a connection of the phenomona of medicine, but we would learn to understand the paths by which these increases of knowledge were obtained, and we would learn to know the laws that underlie these profound mental processes. Now this present book is not intended as a simple record of medical events or doctrines, but as an incentive to the study of mental processes which preceded those events. It is really only half knowledge to be satisfied with an innovation, a new doctrine, or a new discovery of medicine. Real knowledge must seek to understand the ideas that underlie and extend beyond these innovations.

Hippocrates says somewhere *ίητρὸς φιλόσοφος ἰσόδεος*, "God-like is that physician who is a philosopher." Among the readers of this book there may be some who are of the opinion that I have devoted too much space to the consideration of Ideas as Factors in Medical History, and also to the problem as to whether there are Laws that govern General and Medical History. My reasons for going into details concerning these questions are that I am convinced that further progress in Genetic History depends almost exclusively on the recognition and understanding of the constructive ideas of our advanced medical thinkers, past and present, and secondly, because the concept of law must be first clearly understood before any historiographer can begin to investigate whether or not there are laws that control history. If this cannot be established in General History, we need not search for such laws in Medical History. Every natural law declares that in consequence of antecedent conditions which are alike, results must follow which are also alike. In other words, as things and events that are alike in the world of our sensations are indicated by like signs, therefore the natural law sequence of like consequences to like causes would correspond to a regular consequence of our sensations. I mean, for example, if experience has shown that great hunger and lack of protection to exposure among people has brought about a revolution against the contemporaneous government in the year 1789, similar conditions are likely to be expected when hunger and exposure bear down upon a people in 1830 (French). In an analogous manner, we might say that the desire to relieve human suffering led Edward Jenner to investigate and find the protective action of cow-pox contagion against human smallpox in 1796. Similarly the desire to relieve human suffering led the American army surgeons, Reed, Carroll, and Lazear, to investigate the transmission of yellow fever by the sting of an infected mosquito (see article on the United States Army Yellow Fever Commission). In both of these instances we are qualifying the motive which is the desire to relieve human suffering as the causality leading up to the result of a therapeutic and prophylactic achievement. There is no logical reason why a motive should not be thus described as a causality, but it could not be defined as a law of medical history, because the definition of law in that sense would mean that like antecedents should always produce like consequences. For instance, heating of a metal causes expansion of the metal, but in the experiments of medical science the consequence is not always the same though the incentive and the motive may have been the same. In the cases just quoted, for example, both Jenner and the United States Army Commission succeeded in their experiments, but there are numerous other experiments in medicine where several investigators started from the same point of inquiry and yet one failed and the other succeeded. Therefore, motives and causes in science are not necessarily rules or laws.

What leads the historian and the scientist to presume that there is a causality law? What led most of the thinkers, long before there was any science and history established according to accepted principles, to assume a causality? It is nothing but a kind of faith in the lawfulness of all events, in the lawfulness of everything that happens and transpires. Every inductive conclusion is based upon the faith that lawful development that has been observed in things hitherto will also be confirmed in all other cases that have not come under observation. Lawfulness is also a condition of conceivableness or comprehensibility. Faith in lawfulness is also faith in the comprehensibility of natural phenomena. If, however, we should proceed a step further in our intense love of history and its causes and endeavor to trace historic activities to a final cause, it will be revealed, that the comprehensibility and conceivableness of historic transactions can be worked backward, i. e., regressively, until we have to ask ourselves, what is the final, unchangeable cause of human activity? Very few historians even raise this question, being content with a simple narrative and chronologic arrangement of historic events, transactions, personalities, and changes, etc. For ordinary textbook history or the didactic discipline of history, this is sufficient, but to the historic philosopher and to the physician who is philosophically inclined it is not sufficient. We have to assume that there is a regulative principle of our thinking which drives us to the belief that there must be a final, unchangeable cause. From my own viewpoint I can see no escape from this conclusion, because the causality law expresses confidence or faith in the complete comprehensibility of the world by man. I have before this referred to an article by Helmholtz in his *Vorträge und Reden,* Vol. II, page 165, in which he speaks of *"Thinking in medicine," Das Denken in der Medizin.* On page 243 of this volume the sentence occurs, "Comprehensibility in the sense in which I have described it, is the method by means of which our thinking, subjects the world, arranges and coördinates the facts, predicts the future. It is the right and the duty of thinking to expand the application of this method and it really has reaped great achievements, but we have no guarantee for the application of the causal law except its own success." In other words, the expediency of application of the causal law is found in pragmatism. Elsewhere he says, "The causal law is, in reality, a transcendental law." A proof of this law from experience is not possible, for the very first steps of experience are impossible without the application of inductive conclusions, and these are impossible without the causal law. So, in medical history, as in general history, if there is any law, it is that every change and every problem and every discovery and every great medical mind must have a cause, or is subject to the causal law, but what that causal law is, in its essence we cannot explain, but, for that reason, we must not cease to keep on, generation after generation, to extend our search for causal law, for among a race of living things with whom a "thousand years are as a night watch" new laws of mind may be discovered which will open further and further prospects of understanding the mind itself and therefore of understanding history. We have seen, for instance, that Kant made grave errors in his *Kritik der Reinen Vernunft* because of defective development of the special sciences of his time. A clearer insight into Kant's ideas has become possible by the work of thinkers since Kant, and a clearer insight into causality and laws of history will be the problem of the future historic philosopher.

HYPOTHESES IDEALISM AND REALISM IN MEDICAL HISTORY

The future investigations concerning causality in history will have to clear up a great deal of the controversy that still exists between idealism and realism. An admirable step forward has been made by E. B. Holt, Walter T. Marvin, William P. Montague, Edward Gleason Spaulding and others in their work entitled "The New Realism." For medical men it is unavoidable to acquaint themselves with what is meant by these terms. *Idealism* is any theory which affirms that the universe is an embodiment of mind or that reality is essentially psychical. Thus idealism denies that things can exist apart from an experience of them.

Realism is the doctrine that objects of sense perception (and hence, in extended use, the physical world generally) have objective being, independent of mental or psychical fact. The realistic hypothesis trusts the verdict of ordinary self-observation according to which the changes that follow an action have no psychical connection with the antecedent impulse of the will. It regards the objects of our daily perception as independent of our conceptions. The material world is a world "per se," apart from what we can conceive and think about it. Undoubtedly this hypothesis is the simpler of the two and has been sharply defined in the work on *The New Realism,* to which I have referred. All these definitions have an important bearing on what I shall have to say concerning laws in history because it will there be revealed that if there are any laws of history at all, they are so deeply rooted in psychology that they cannot possibly be designated as belonging to history. Now as regards the distinction between idealism and realism, I wish to say that from the standpoint of the historian absolute truth cannot be ascribed to either of them. They are both, particularly realism, practical hypotheses, and merge into each other. For example, that which is lawful in our sensations, feelings, thinking, and willing we could express in about the following manner. "The acts of consciousness that occur with the character of perceiving things, transpire in such a manner AS IF the world assumed by the realistic hypothesis really existed." But it will be noted here that there is always an "AS IF." These hypotheses have their scientific justification. For those who are more especially interested in this subject there is an excellent article in "The Studies in the History and Method of Science," Vol. II, page 414, entitled "Hypothesis" by F. C. S. Schiller. Historic science must investigate the carrying power and helpfulness of all hypotheses in order to secure a complete survey or possible explanation. The historian cannot always wait until a sure scientific decision has been reached, for this would compel him to leave great gaps in his account of causal-analytic historic factors. For any time period it is desirable that the continuity of the narrative shall not be broken unnecessarily and it is for this reason that the *fictive method* of Vaihinger is unavoidable because human thinking rarely proceeds in a straight line. It did not do so in our ancestors—it does not do so to-day in us. The reason of this I cannot explain, and therefore, in tracing the activities of human beings long past wherever the records and sources desert us, rather than leave long gaps it would be more expedient to bridge them over by the temporary use of one or another of the hypotheses which Helmholtz (l. c.) refers to, but the cardinal point in their use is the ever-present consciousness of the investigator that he is using a hypothesis, or a fictive method, fully aware that it is only a temporary expedient. Hypothesis is a theory assumed but not proved. In the hypothesis, however, there is a trace or possibility of reality, but in the fictive method this reality is knowingly excluded from the start. The dogmatist says *This is so—it really exists,* the one using a hypothesis says *This is perhaps so, possibly so* IF such and such conditions are fulfilled. The fictive method says *I am going to proceed* AS IF *this were so* in order to use it as a practical or regulative expedient—I may discard it later, on finding something better.

SUPPLEMENT

The problem of the reciprocal relations between the ideas of individuals and the motives of masses was discussed personally and by correspondence with a number of historians and the letter of Dr. Katherine J. Gallagher on p. 118 is there inserted as presenting in a clear manner one of the most plausible viewpoints.

I have some hesitation of speaking of *the idea* of the masses, for I have found it exceedingly difficult to ascertain by the best historiographic methods whether a large mass of human beings ever harbors a unified central idea as an incentive to action. Even where historic data would most certainly indicate such an idea, the expressions of this idea in words either by the various leaders of the mass, or in the declarations and resolutions to which the mass eventually gives assent, or even in the so-called public mass utterances—such as party cries, religious dogmas, platforms, national or racial poems and songs—are so divergent that they suggest rather the unlimited variety of a multitude of individual opinions than the psychic momentum of a unified homogeneous idea, an idea of the type which I have referred to as constructive.

Historic sources give dependable accounts that the masses are indeed actuated by a central idea and by the same idea at epochs widely apart, but the subsequent development of that idea leaves the historian in doubt whether or not that idea was really constructive, or whether in the long run it served the best interests of the people.

As the best test of the constructive idea we can accept only the furtherance of the moral, intellectual, and economic improvement of the people which makes the effort to carry that idea into effect. For example: The Jews cried to the prophet Samuel: *"Give us a king to judge us"* (I Samuel, Ch. VII, v. 6). In spite of religious and personal misgivings the prophet had to anoint Saul—result, temporary success against their enemies; in the long run—disorganization of the people, destruction of the unity of the race.

The reverse of this: The English people on Jan. 30th, 1649, after prolonged struggles between their monarch and parliament, abolished monarchy by executing Charles I. Twenty one years later in May, 1660, they restored monarchy by placing the son of the decapitated King Charles II on the throne. This king, a confirmed sensualist, his reign an era of dissolute morals, made himself independent of parliament. In almost all European countries a similar history of contrast between monarchy and democracy shows that the masses, before the Declaration of American Independence, had given little evidence of capacity for unification around a central constructive idea, and persistently carrying it out.

When really great constructive ideas are concurred in by the masses and the great thoughts are traced to their origin, it is found that they emanated from the brain of an eminent thinker. The question is: Who originates the great constructive idea—not who and what are the popular proceedings that are necessary to bring that idea into pragmatic application. For the execution of the idea, a coöperation between the many and the leader is necessary. The interpenetration of the conditions of preparation of the many and the carrying power of the constructive idea of the one, according to Dr. K. J. Gallagher, must be taken together as one phase—not separately. This is the manner in which a capable historian speaks but such a broad outlook is not readily comprehensible to the average mind, which has difficulty in synthesizing into one phase such events as can be shown to have developed far apart chronologically. In the processes of history the element of time is important enough for classification and duration of events, activities, etc., but for estimating events and realization of ideas "in the act of becoming" (*im Werden* as L. von Ranke expresses it) time must be conceived differently than as a convenient method for establishing landmarks for memory or classification.

In classification and recording historic events time is nothing but part of a general mechanical schema for orderly arrangement, coördination of data, etc. But in studying the evolution of great ideas time becomes an integral part of the process of this evolution—this much is evident from the known truth that ideas require time for maturation and hence the great constructive ideas of Science—say that of the circulation of the blood or of Einstein's theory of Relativity—are known to have been harbored by many different minds, independently of each other and required varying lengths of time for maturation. To show how time is to be conceived in these two manners, I may for example cite the three Punic wars—the first dealt with the possession of Sicily and lasted from 264 to 241 B. C. = 23 years; the second was a death struggle between Rome and Carthage and lasted from 218 to 202 B. C. = 16 years; the third was a wanton war for the destruction of Carthage 149 to 146 B. C. = 3 years. Thus the actual war lasted 23 + 16 + 3 = 42 years. But the entire historic phase 118 years was a conflict between the Semitic and the Latin race for the possession of the land in and around the Mediterranean Sea. The time figures of each of the three Punic wars are now mainly the means for historic coördination and arrangement of a great number of land and naval battles—treaties, migrations, leading personalities, events in the Council chambers of both the Romans and the Carthaginians, etc. All of these belonging to one great historic transformation—just mentioned and which required 118 years—this is the duration of the phase which required that time for its completion.

Dr. Katharine J. Gallagher's viewpoints on the function of the idea and group coöperation in history are as follows:

"It is impossible to separate intellectual from general history. One is part of the other, and they operate in the same way. Any apparent difference is due to the fact that general history seems to take cognizance only of institutions and events, while a special branch of intellectual history—like the history of medicine—can be more obviously seen to concern itself with ideas. But this difference is more apparent than real. The truth is that in history an idea does not usually force itself within the historian's field of study until it has been materialized into an institution or a definite act. In this form it is no longer the work of an individual mind, but it is an individual ideal in operation upon, and accepted by, other minds. The length of time that elapses between the generation of the idea and its acceptance by the group can seldom be calculated. There seems to be, however, some real relation between the crude utility of an idea and the rapidity of its acceptance. But no idea will be historically successful until the masses are economically as well as intellectually prepared for its acceptance. In this way the *economic* condition of the masses contributes to the transformation of an idea into an historical act. It is in this sense, for instance, that the economic condition of Germany was a cause of the Protestant revolt. Ideas similar to Luther's had been generated before Luther's time, but the group was not intellectually or economically ready to accept them and convert those ideas into historical realities. The concrete facts of general history are not caused by ideas alone, but by ideas working in and through human beings whose response to them is conditioned by their political, economic, and spiritual state of development.

"One school of historians, seeing that historical facts materialize only when there is preparation for the acceptance of ideas, emphasizes the *conditions* of *preparation,* and states these are *the causes* of historical acts. Another school which generally includes the intellectual historians,—seeing that no act is without its basic ideal, though that ideal may have been evolved long before the event, emphasizes *the idea* as *the cause* of human activity. The truth includes both of these interpretations which must be taken together and not separately. The general historian sometimes—though not always—fails to emphasize the service of the originator of the idea, in his interest in the material conditions which incline the masses for its acceptance; and the intellectual historian (in his absorption in the struggle of the idea against the mental inertia of the mass) sometimes fails to recognize the importance of material conditions in preparing the way for the final triumph of that idea, and its conversion into a concrete historical act."

This graphic description gives the manner of operation of ideas—*after* they have been formulated.

As we have been greatly favored by Dr. Gallagher's clear exposition concerning the interpenetration of the ideal of the individual and the acceptance of the masses—it may appear unnecessary to comment any further on these causative factors of history. As far as general history is concerned, her viewpoint is faultless, but in scientific history the overwhelming evidence of the past points to the frequent rejection of new scientific ideas advanced by great individuals even, where after many years of education and ever renewed opposition, the new idea was proven to be productive of the greatest blessings. The problem of this chapter concerned the origin of ideas not how their effects were brought about.

What can be more convincing of the inertia in and opposition of the masses to high ideals and great ideas for improvement of the human race than the following two historically proven truths. First, To the present day the indispensably preserving laws of the decalog are not carried out by coöperation of the masses, but require the application of force. Secondly, The great forward or even backward movements of our race were only exceptionally brought on by conviction of the correctness of ideas, which means an appeal to reason, but in the great majority of historic transactions by appeal to emotion and impulse.

In the history of Medicine no evidence can be found that the masses, large groups of humanity, ever originated a great constructive idea. In the nature of things such a phenomenon appears impossible. Some times a great new thought appears to set the receptor apparatus of a number of capable minds in operation at the same time—even when separated by great geographic distances. But they are always demonstrated to be minds of exceptional capacity and then never more than two or three of them, as in the discovery of oxygen (see Article on Lavoisier who was guilliotined *"Because the Republic did not need Savants"*) or in the history of the periodic (atomic) tables of Mendeleff or of the prediction of the discovery of an unknown Planet—see Chapter VI.

The idea of the formation of the skull as viewed by analogy to the structure of the spinal vertebrae was "in the air" to such a degree that several brilliant minds were "led to it" independently of each other (see Chapter XXIX on Goethe as a Naturalist.)

Ideas not only have their periods of incubation or maturation in human minds wherein some minds mature them almost instantaneously and others after various longer or shorter periods—but the evolution of ideas gives indication of mutations both in the sense of de Vries and that of Waagen (see Chapter XXX on Darwin). Therefore new ideas that are announced approximately at the same time, were not necessarily conceived at the same time.

Goethe (Sämmtl. Wercke, Bd. 40 S. 460) expresses this beautifully. "And yet certain ideals and thoughts seem to pass in the air so that several minds can grasp them—or to speak less mystically, certain conceptions mature through a period of time."

Immanet aër sicut anima communis quae omnibus praesto est et qua omnes communicant invicem. Quapropter multi sagaces spiritus ardentes subito ex aëre persentiscunt quod cogitat alter homo.

V

THE RÔLE AND FUNCTION OF GREAT MEN IN MEDICAL HISTORY

IN judging the rôle and function of great men, particularly great men of science, we can reach a degree of clearness in understanding by holding in mind three perspectives: First, the degree in which the work, labors, and endeavors of the great men have made the hitherto uncontrolled forces of nature subservient to man. Secondly, the extent to which the work of the great man has advanced human culture. Thirdly, the extent to which the work of the great man has advanced human civilization.

Here an exact definition of the term culture and civilization becomes necessary. It will be impossible to bring unity and agreement into the expressions that have historically been recorded as meanings of culture and civilization. As it is in the present instance of importance to know how the author wishes these terms to be understood, an endeavor will be made to unify many definitions given by historians quoted in the chapter on Methodology and in the chapter on Ideas. There are certain effects and spontaneously developed products of nature, with which man's activity has nothing to do and with these spontaneous changes of environment, culture and civilization have nothing to do. Culture refers to the changes that are produced in a purposeful manner by man himself and in a more restricted sense culture refers to man's achievements and endeavors to make nature serviceable for the life purposes of humanity. Here we note that the first and second classifications, just given, pretty well coincide. Civilization refers to the achievements and endeavors of humanity in the control of humanity, but as humanity is a part of nature one can see without much stretch of the imagination that all three definitions previously given coincide *in substantia*. The greatest errors have been committed by a misunderstanding of the so-called individual activity and the generic uniformity in the result of mass activity. The relation of individual will and individual purpose to the general will varies and changes during the process of civilization and the changes are even noticeable within the same generations. There is a *purpose-consciousness* and this may change in groups and communities which externally exhibit the impression that all members of the group are acting uniformly.

I do not refer simply to political bodies, church groups, patriotic and military groups, but even to universities and colleges. At no time in my own personal life have the form, conditions, and the spirit of corporations and institutions changed so manifestly as in the last decade from 1915 to 1925. I have elsewhere spoken of the latent period of incubation of great ideas. In political history, this latency is due to the fact that the more powerful thinkers, or a few, or one individual only of the mass, who have hitherto apparently or mechanically followed the purpose and aim of the totality of action of great groups, have reflected and meditated upon these purposes and the direction of the mass activity until it has come to the full consciousness of the leaders that they either must oppose them or they personally must give this entire social activity a new direction. Men who make history are always individuals who are capable of imparting to their ideas the impelling force of mass action, men who provoke a conflict so that the great social groups must decide for or against an alternative. Many times, leaders, history-making men, pathfinders in science have gone to destruction during this effort (Julius Cæsar, the Grachii, Wallenstein, Lincoln in political history. In the history of science, Servetus, Lavoisier, the three martyrs of the United States Yellow Fever Commission, Reed, Lazear and Carroll). But the majesty of the thought and the penetrating power of the idea which such men have stood for, has continued the struggle to victory even after their death.

The distinction between historic science and biologic science is so important that I have emphasized it in several places in this treatise. Because there are ever appearing new writers who insist that historiography and historic science should be carried out along the lines of the biologic science. The chief distinction between the two is this: historic science deals with human beings as *social entities*. This means that by the psychic make-up or constitution of human subjects, they choose their activities in a purposeful manner but with an ever-increasing and expanding consciousness that they are themselves setting their own aims. A. D. Xénopol, in his work *La Notion de Valeur en Histoire, en Revue de Synthèse Historique* (1906, T., 11, p. 129.), inquires of Bernheim why he separated the organic development of nature from the historic development, especially with regard to the logical character of historic development. This question is answered in the definition I have given in this paragraph, where the distinction is made that the purposeful character of the activities of human beings as social entities is a process that is distinct and separate and above any organic bio-genetic evolution of nature.

This insight will become more convincing the deeper the student of history and the student of natural science meditate over it. The biologists may ask, "Well, isn't a man a living thing, and therefore, do not all of his activities lend themselves to the methods of the biological sciences?" The answer is, "Yes, and No." As far as he is a biological creature, everything that he does can be helpfully approached by the methods of biology, but as soon as man's activities ascend into the realm of social activities, with definite purposes set by man himself and with even the consciousness that he is the creator of his own purposeful activities, the latter rises above purely biological science. Upon these basic facts rests all knowledge of human activities and human affairs, and the recognition that biologic connections and rules are not sufficient in the observation and study of historic acts. New and highly specific conditions actuate man as soon as he exercises his powers as a social being. Ludvig van Ranke (*Werke,* Bd. XXIV) states that "it is not the office of history to bring together a dead collection of facts and string them in chronologic order, but that its aim must be to bring about a living understanding by helping us to learn toward what aims the human race has directed its efforts in every period, what it strived for, and what it really achieved."

To accomplish this the historiographer has a right to make use of the most effective means taken from any science or art outside of his own. When I stated that biologic sciences were insufficient to encompass all the social activities of man and that new and additional means and ways of investigation and study had to be added to those of biology, I did not reject the scientific biologic treatment of history. In a work that treats of the *Master Minds in Medicine,* it is the biological method which is bound to be used until it is exhausted, but the methods of psychology and of historiography will assert themselves whenever we have come to the end of our wits in the endeavor to make biologic rules and aspects encompass the totality of history. Every single event in medicine, every transaction, every discovery, every evolution of a great constructive idea holds the position of an outstanding monument within a great city. I mean by this, every individual event in a social aspect must be brought in connection and relation with the totality of the development of the human race. In this effort, which is not always successful, it is exceedingly difficult to separate that which is due to the individual and that which is due to the totality. Very often these two entities interact reciprocally, and no matter how desirable it would be to keep them apart it frequently cannot be accomplished.

The most comprehensive and all embracing totality of things with which the individual achievements have to be correlated is humanity in its total development and whilst A. D. Xénopol emphasizes that we must hold apart the various evolutions and developments accomplished by single individuals as parts of a totality, in the study of the development of medicine this is often impossible of execution, and it is for this reason that I believe it to be a most effective discipline to speak of the great individuals who have been Pathfinders in Medicine as exhaustively as practical and leave the correlation of their work with the aims and achievements of the race as a totality to the broader plan of the general medical historian; for, let it be said, in scientific medical history, like in general political history there are special and general historiographers. The work of the great Pathfinders in Medicine is, as a rule, very specific and circumscribed and requires much circumspection to be fitted in and coördinated with the already established facts, truths, and practices of medical science which existed at the time any great, specific contribution was made. It would exhaust the medical historiographer if, in addition to this, he had to correlate any new complex of medical facts with the totality of contemporaneous medical history. This endeavor would lead the medical historian too far and it is just as well to confess that certain formal historic problems really have to be left to non-historians, for instance to diplomats, to political economists, to philologists, and philosophers.

Those collectivists (see chapter on Ideas as Factors in Medical History), who argue that the masses "potently influence the course of medicine and as group factors are effective in initiating events in medicine," will have to explain how a great group of human beings would be able without training to accomplish some of the great evolutions in medical history, and furthermore, if the great discoveries of medicine are not due to individuals, but that all advances are derived and dug out of the deposits of the past, immediate or remote, the collectivists must prove that the manner of thinking, the discernment, the formation of conceptions was the same in the past as they are at the time any great medical discovery was made, for, be it understood, any new doctrine, discovery, or event is couched in the vocabulary and clothed in the conceptions of its own contemporaneous period, and if history reveals anything, it reveals a gradual transformation of the manner and style of thinking at various historic periods. There is a continuous change in all human relations and affairs. Historians are aware that the unified perspective from which all the individual changes could have been brought in continuous relation was missing in antiquity. In the Middle Ages the newly formed perspective began to develop, but even to the trained minds it was so remote that the changes, political, religious or scientific, were not observable.

Monod says in the *Revue Historique,* T. I, No. 1 P. 8. "Les hommes du moyen âge n'avaient pas conscience des modifications successives que le temps apporte avec lui dans les choses humaines." When a great medical discovery was made, contemporary human beings looked upon it as an abnormal or exotic production, because they did not have the mental equipment for understanding nor the perspective just referred to. This did not occur very often in medical science, but it did in philosophy, politics, and above all in the history of religion. The most regrettable conflicts with the advanced ideas of master minds are those where scientific men obstinately oppose the introduction of new scientific revelations from members of their own science. James J. Walsh is correct when he states that scientists have never been over-ready to welcome new truths that contradicted the old ideas which they had accepted for a lifetime (*Science and Religion,* page 18). The most classic instance of this opposition of scientists to a great scientific discovery is the tardy recognition given to Harvey's demonstration of the general circulation of the blood (see Chapter XI on the "Discovery of the Circulation of the Blood"). Harvey's book announcing this epoch-making discovery appeared in 1628. He was opposed from the beginning. His medical practice fell off. He himself complains that his patients considered him "crack-brained." The medical profession was largely against him. As late as 1671, forty-three years after the announcement of his discovery, it was refused acceptance by one of the greatest anatomists of his day. In France it met with bitter and protracted hostility from the University of Paris. Official recognition did not come in Paris until 45 years after the announcement of the discovery.

This story of misunderstanding runs through all the history of science, not only misunderstanding but prejudice and unreasonable opposition. In a letter to Kepler (1571–1630), Galileo complained of the hostile reception accorded by distinguished men of science to his new invention of the telescope, the foremost teacher of the University of Padua even refusing to look through it at the moon and the stars. Vesalius (1514–64) was very much opposed by the contemporary physicians and surgeons because of the anatomic discoveries described in his *Fabrica.* The opposition to Michael Servetus (1511–53), is narrated in a separate chapter XX in this volume. Let me repeat that his great attainments as the first logical therapeutist and his discovery of the pulmonary circulation of the blood did not save him from the pious and personal animosity of John Calvin, who proscribed in Servetus the guilt of his own rebellion. Referring to the law of contrast, mentioned in Chapter III (on Laws), one could assert that throughout the history of religion the so-called Patriot Reformers were severer than the tyrants whom they had dethroned. The true historian will qualify the word "tyrant," for it is variously construed by these very reformers. The article on Servetus which I present in this volume, chapter XX, does not include the account in the "Dictionnaire Critique" of Chauffepié. Gibbon (Rome) states: "I am more deeply scandalized at the single execution of Servetus than at the Hecatombs which have blazed in the Auto-dafés of Spain and Portugal" (Vol. V, p. 552 of the edition of John E. Morris & Co., Philadelphia, Pa.).

The discovery by Stensen (1648–86) that the heart was a muscle was met by an opposition not as amazing as it was pitiably amusing. Physicians contemporary to him believed that the heart was the seat of the emotions and they could not accept such a prosaic description of it as a mere muscle. Stensen was regarded by them as not quite sane, so much so that they discarded him and his new theory. Coming to the method of percussion for pulmonary diagnosis which Auenbrugger (1722–1809) discovered, we have a statement of his own as follows: "I forsee that I shall encounter no little opposition of my views; envy and blame, even hatred and calumny have never failed to come to men who have illuminated art and science by their discoveries." Fifty years after Auenbrugger, Laennec (1781–1826) completed the development of diagnostic methods for chest diseases by the discovery of auscultation. He anticipated the same opposition which he had seen in the case of Auenbrugger. In his preface he wrote: "For our generation is not inquisitive as to what is being accomplished by its sons. Claims of new discoveries made by contemporaries are likely to be met by smiles and mocking remarks."

The discovery of vaccination against smallpox brought to its originator, Jenner, the bitterest kind of opposition (1749–1823). He was made to suffer intensely by his contemporaries. Thomas Young (1773–1829), one of the greatest scientists of the nineteenth century, the decipherer of the Rosetta stone, introducing us to an understanding of the hieroglyphics, who explained to us the nature of the light waves and the existence of the ether to carry them, was made a subject for scorn by his fellow scientists and had to quit his priceless investigations in utter disgust. The discovery of the law of electric resistance, by Georg Ohm (1787–1854), met with severe criticism from the professors of the University of Berlin. All of the great men portrayed in Wilhelm Ostwald's *Grosse Männer,* except perhaps Helmholtz, had to experience similar discouragements and hostility. A rare similarity of adverse experiences is shared by the two discoverers of the infectiousness of puerperal fever, our American Oliver Wendell Holmes and the Viennese Semmelweiss. The latter met with such persecution that his mental distress produced insanity. Examples in abundance are at hand to demonstrate that great discoverers of any period do not meet with minds prepared to receive them, even within their own narrow circle of confrères.

A great exception to this rule, an encourager of any new work, was that magnificent universal scientist Alexander von Humboldt. Without his encouragement we should never have been blessed with the far-reaching value of the work of Justus Liebig, for it was Humboldt who actually created Liebig's academic positions; without Humboldt even Helmholtz would not have succeeded as well as he did, for Humboldt effected Helmholtz's appointment as Professor at the University of Königsberg (1855), and Humboldt was also the discoverer of the great talent of Alexander Aggassiz and protected it until it reached its full development. In this way the influence of this great German *savant* becomes apparent in the entire school of W. K. Brooks, the first Professor of Biology at the Johns Hopkins University, for he was a pupil of Aggassiz. I mention Humboldt as an outstanding example of a man who was ever ready to encourage talent, and as an exception to the general rule narrated in the examples before mentioned. All of these historic examples of great discoverers failing to meet appreciation of their times simply serve to illustrate that a special mental condition is necessary for the appreciation of new historic discoveries, namely, the minds of contemporary persons must become emancipated from the tyranny of tradition.

On page 303 of his *Forderung des Tages,* Wilhelm Ostwald asserts that to favor new discoveries and new advances in science the mind of the young intellect must not be burdened with traditional knowledge and he explains this by arguing that a kind of pseudo-reverence for old historic thoughts deprives young investigators of the necessary courage and initiative to follow up ideas which, though new, appear to be in absurd contrast to those which history had taught him to respect.

If such mental preparedness is necessary for the reception of new discoveries, for free initiative in experimental verification of original thoughts of scientific men of our own times, who by chronologic proximity and cultural affinity ought to be very open and unbiased in the reception and encouragement of new discoveries—how much more unintelligible and irreceptive should we find that people of ancient periods reacted to the revelations of their own contemporary leaders, when reading and writing were only understood by a few, and the fastest communication between people was by foot or by horse? How much less can we of the present time understand them when not only their language was entirely different from ours—the Latin of the Middle Ages has to be specially studied by the best Latin scholars of to-day—but their conceptions of political, religious, economic, and military affairs were so fundamentally different from our own that even the best experts of the history of those times have to leave great gaps in their interpretations of the advance of science?

The study of the history of antiquity and of the Middle Ages for the purpose of throwing light upon the gradual evolution of medical ideas is intensely interesting, absolutely necessary for the structure of historic medical conceptions, but to imagine that all the great medical discoverers of the 18th, 19th and 20th centuries are dug out of "deposits of the past" is not productive of correct insights. This much will become evident by the study of the chapters on the Circulation of the Blood, on Lavoisier, and the Physiology of Respiration and Metabolism, and other specific researches into physiology, where Ostwald's assertions that the overburdening with traditional historic conceptions of the past is a hindrance to new discoveries appears to be verified. In attributing so much concealed or embryonic prevision to the ancients we would be establishing a hypothetic historic mind—*entelechy*—I mean, we would be assuming an uninterrupted and ceaseless activity of the ancient intellect directed to the same specific solution of problems as we conceive and employ them to-day. Aristotle defined "entelechy" as "the higher self-determining energy which contains the purpose in itself." The best that we can attribute in the way of deposits of medical knowledge among the ancients is a kind of vague speculation, presentiment, foreboding, and anticipation of structure and processes, without any precise and punctilious method of verifying them. Sometimes a rare great man forces his views through to public recognition because he goes a little further than pure speculation. Such prophets are found among those of the Alexandrian School, Herophilos and Erasistratos. Some of the disciples of this school really employed narcosis by means of an extract of mandragora and used arterial ligation to stop hemorrhage.

There are other men of this type whose history will be found described in a fascinating manner in *Geschichte der Medizin im Überblick mit Abbildungen* by Meyer-Steineg and Karl Sudhoff. But if the scientific men themselves were so reluctant to accept new medical discoveries in recent times, the unscientific masses of remote times were much more difficult to win over even to such discoveries which concerned their own welfare. This entire difficulty is one that concerns the manner of thinking of remote periods. It isn't at all possible that they could have conceived physiology, anatomy, pathology, and chemistry in the same manner in which we do, because they did not have the sciences themselves and when later they did acquire an embryonic knowledge of these sciences, the masses did not possess the culture and mental equipment to assimilate them. It means that the acuteness and fineness of discernment in the formation of scientific conceptions develops exceedingly slowly and gradually, in all peoples, with rising culture, and in regressively transferring the fine distinctions of a highly cultivated recent period to the manner of thinking of antiquity or the Middle Ages, we must not disregard the fact that the fitness and capacity for the understanding of such distinctions were not present in the minds of those of by-gone periods.

There are three types of thinkers with regard to the question as to whether great achievements in culture and civilization are due to the masses or due to individuals. This classification appertains only to *general* or political history, not to *history of science*. The determination of the correct value of each of these two factors, the *individual,* and the *general totality of human beings* in the participation of every great advancement, is one of the most important occupations of the historiographer. First, there are those historiographers who assign the chief work of progress exclusively to great individuals. Among these are Wilhelm Ostwald in his *Grosse Männer,* Charles J. Whitby in his *Makers of Men,* Dilthey in *Beiträge zum Studium der Indivualität,* Carlyle in his work on *Heroes and Hero Worship,* Century edition, 1897, Cousins in his *Theorie der Grossen Männer,* all of which, however, exaggerate the importance of the individual as far as general history is concerned. Secondly, those who conceive that the masses initiate all that is progressive in history. Among these are Comte, Condorcet, Buckle, Bourdeau, and possibly Lamprecht, Dürkheim, and Tarde. The third school is represented by men like Seignobos and E. Bernheim, who whilst they deny that large groups can produce uniform effects and progress, yet emphasize that the relation between the individual and the masses in history is a reciprocal one. This relation is compared to that of a tree in a great forest. The forest exists only by virtue of the individual trees, of which it is composed, but a forest has more and different effects than a single tree could exert. A forest, for example, can protect large farms from icy north winds, which a tree could not do, and the forest also affects each individual tree that composes it. Any individual tree has its being and its effect by virtue of its own entity, but it receives other qualities by virtue of being a constituent of a great forest. When we apply this comparison to human beings, however, the elements of immensely complicated intellectual, social, and psychologic manifoldness enter. We have to study without prejudice the effect of individuals on the social communities and reversely, the effect of these on the individual, in the manifold relations of culture and civilization. Single examples of either of these great factors, torn out of their connection in historic development, cannot replace such a broad inter-acting and mutually penetrating process as that continually going on between the individual and the mass.

Bourdeau (*L'Histoire et les Historiens,* etc., 1888) goes so far that he denies the importance of the individual altogether, stating that if an English bullet had killed Napoleon Bonaparte in the trenches of Toulon, there would have been Hoche, Kleber, Desaix, Marceau and many others not less great than he to take his place. Napoleon found himself there and because the circumstances were great he became great through them. Mougeolle (*Les Problemes de l'Histoire* 1886, page 135) asserts that the achievements of eminent personalities are nothing but the resultant of an infinite number of little energies rising up from below. The political historians and the people in general incline more to an appreciation of eminent personalities, but the cultural historian is disposed more to the social psychic influences of great groups. Recently, according to Bernheim and Theodore Merz (l. c.) a conciliatory attitude is noticeable between these two groups, individual genius and masses, individualist and collectivist, which brings them under the perspective of a variable, reciprocal process, assigning to each element the proper influence. In the History of Medicine, however, the laurels of achievement cannot be wrestled from the individual. The same is true of the history of the natural sciences. To the present time (1925) it is impossible to speak of a continual reciprocal interaction between the masses and the individual in the achievements of science, or more especially in scientific medicine. The cause of this is most probably the high degree of special cerebral localization of intellectual capacity, which has to be acquired through many years of training and which it is unreasonable to assume as present in the masses. Whether or not talents and eminent abilities are transmitted by inheritance is still an unsolved problem, specially occupying the attention of the geneticists in the last ten years. The subject will be found considered in Chapter XXXII on Heredity. It is a curious thing that the epithet GREAT is not assigned by the contemporaries of an eminent thinker and worker, but is given by posterity. It has happened that great men in political history were given the designation GREAT by their contemporaries. This was the case with Pompey, whom the Romans called Great, and with Louis XIV whose sycophants gave him this title, but history did not respect these decrees. Few men are irreplaceable, but the very few that are, in spite of this apothegm are great. One of the most ennobling attributes of greatness in medical men is their amazing unselfishness. Medical genius never seeks its own. Schopenhauer states: "The ordinary men have their own interests in mind and as a rule know how to advance them, because they adapt themselves to their own times, ready to serve the requirements and notions of contemporaries. For this reason they generally live in happy circumstances, but the genius sacrifices his personal welfare to a great objective purpose. He cannot do otherwise because his heart and soul are embodied in this purpose. The great mass does otherwise. That's why they are small, but the genius is altruistic, that's why he is great." Other characteristics of the great man are 1. his receptivity to inspiration, 2. his power to plan a scientific, experimental verification to his rich intuition, 3. his gift of orientation, 4. scientific and artistic memory and automatic notation, 5. universality, 6. love of truth, 7. ready distinction of objectivity from subjectivity, 8. punctiliousness, 9. love of order, 10. modesty, 11. industry, 12. artistic phantasy. Further characteristics are given in the Chapter on The Psychography of the Investigator, Chapter XXXI. It will be expedient to refer also to the Ideas of Plato in the chapters on Methodology, Chapters I and II, as well as in that on Ideas, Chapter IV. There the view is expounded that truth is something outside of man and can appeal only to certain psychic organs which may be termed the receptors and transformers of truth just as the retina of the eye is the receptor and transformer of light. This conception explains that not all men are equipped to be great investigators in medicine, and that the hypothesis that all great medical discoveries, events and doctrines are in their essence attributable to the initiative of masses, lacks not only the proper anatomic and psychologic instrumentarium, but lacks historic verification.

As we have compared the relation of individuals to masses by analogy to the relation of a tree to a forest (Bernheim), I might compare the rôle and effectiveness of great men in medicine to the function of two instruments that have become popular in modern radio apparatus and in the control of direction of automatic vessels at sea, like the torpedo. Great men are those that have the psychic *antennae* for the reception of the truth. They are the men that are *in tune* with at least parts of the truth, and *secondly,* great medical men and great men in general are like the gyroscope in a torpedo. The gyroscope is an instrument for the control of the direction of a moving body. It has been used for this purpose in controlling the direction of ships and in the gyro compass. Great men are comparable to the gyroscope, because of their marvellous sense of orientation concerning specific scientific tendencies. Whenever the mass or even the profession deviates and errs from its correct direction, the great man undertakes to bring them back to the right road. This is not only true of science, but also of religion and philosophy. It is not necessary for great men to produce a great discovery, originate a doctrine, write great books to become great. Some of the greatest medical men are those who have left no such records.

A type of this kind of medical man is Karl Friederich Kielmeyer who left no great work, made no great discoveries, did not originate any new doctrine, but he produced Georges Cuvier, the great French comparative anatomist. Cuvier is usually understood to be a Frenchman but he was born in Mömpelgard, Würtemberg, in 1769, and studied in the Carls-Akademie (Weimar). He was a school-mate of Schiller. Alexander von Humboldt dedicated to him his Observations and Comparative Anatomy. Goethe was also for a time his class-mate, from 1784–1788. Johannes Müller declares that Kielmeyer was the one who really first perceived Comparative Anatomy in its true inwardness, and gave the new science its intellectual precision and that later Cuvier followed out the inspirations of his teacher, Kielmeyer, through the entire animal kingdom. Müller (*Zur Vergleich, Physiologie Des Gesichtssinnes,* Leipzig, 1826). This reference to Kielmeyer I felt it my duty to introduce, to show another eminently rare capacity of some type of great men, namely the gift of the conversion of their pupils into great masters. Not all great medical men possess this gift,—the so-called "Classicists" of Wilhelm Ostwald were poor teachers (see *Grosse Männer* by Ostwald).

VI

THE CRITERIA OF WHAT CONSTITUTES A GREAT MEDICAL DISCOVERY OR INVENTION

WHO IS THE REAL DISCOVERER OR INVENTOR? THE HISTORIC EVIDENCES ESTABLISHING CLAIMS OF PRIORITY

THE great advances in medicine which are comprised under the terms given in this heading, do not spring into existence complete and perfect like Minerva from the head of Jupiter; they are the result of many years of unrecorded thought work. Just as we can speak of the pre-historic activities of man, so we can speak of unrecorded thought work of man. Indeed, some of the greatest of all discoverers have never been recorded in human history. Who, for instance, was the genius that first discovered a means of producing fire at will? It is said that in 320 B. C., Theophrastos described a natural fire producer which consisted of a piece of hard wood revolving quickly on soft wood. In a hymn to Mercury, the invention is attributed to this god, but there must have been means to produce fire before this because the investigations concerning the first occurrence of iron weapons go back further than this. (Under Thutmosis III, 1475 B. C., steel was used. Homer describes steel weapons 800 B. C.; 2220 B. C. steel was known in China under Emperor Yu.)

There are many great discoverers and inventors whose names are not recorded in history, and this leads us first of all to speak of the rôle of great men in history. In medical history, as well as in general history, the influence of that which has been called the *spirit of the times,* the *Zeitgeist,* is occasionally traceable with distinctness. There are penetrating changes in the conception of the nature of disease, which mark off different periods of medical history, and these periods have even forced their signatures upon certain phases of medical history, so that we can speak of the signatures of medical times. The greatest of all intellects are those that stand above the sphere of interest and of the spirit of the times. They belong to no particular period; they belong to all times. As soon as a personality becomes strictly identified under the signature of his time, his importance as a pioneer worker or discoverer immediately becomes limited. Goethe has said, "Great men are connected with their century by a weakness."

In the article on "Methodology" I have explained the views of various eminent historiographers regarding the relative importance in history of the eminent individual and of the influence of the masses, and in explaining that Thucydides was not only one of the first general historiographers, but the first medical historiographer, I endeavored to bring out three of his great methodic inventions in history writing, and one of these was the method for spreading the influence of the eminent personality, although it conflicted with the conviction of the masses to such an extent that if the great personality himself had announced his reforming truths in an *argumentum ad hominem,* it would have involved great danger to the reformer. In all of my work on the psychographies of great thinkers, I have distinguished, wherever it was of importance, between the effects of the individual on the masses, and vice versa, the effects of the masses on the individual. Carl Lamprecht (*Deutsche Zeitschrift für Geschichts-Wissenshaft,* 1896, N. F. Bd. 1, S. 120) conceives that the dependence of the individual upon society is absolute in its essential points (l. c. p. 116). He furthermore states that "the individual psychic element is permanently included in the social psychic process." These conclusions of Lamprecht do not concede to the individual any personal freedom of will. His arguments, though very sophistic, are not convincing. They remind one of the curious statements of Herbert Spencer about Beethoven, that he could not have originated in a tribe of cannibals, and that Newton couldn't have arisen from a clan of Hottentots. Seignobos regards this attitude of the great personality to the masses, and vice versa, as a reciprocal relation, the different effects of which are most intelligibly compared with the relation of a tree to the forest. (*La Méthode Historique Appliquée aux Sciences Sociales,* Paris, 1901.) The analogy is about as follows,—The forest exists only by the combined effect of many individual trees, but the forest has more and different effects than the single trees have, effects which express themselves exteriorally, for example, influence on climate, protection to adjacent fields, etc., but which also have effects interiorally, that influence every individual tree, although the tree is not given its being and effect by virtue of the forest, but has it *per se.* This analogy is interesting as far as it goes, but it cannot go far enough because it is impossible to assign human activities in all their wealth of influence and mutual inter-action to plant life. The entire world of thought and intellect cannot be conceived of in this relation of individual trees to forest. In truth, the vast extent of the processes and effect of soul life cannot even be comprehended. We are standing to-day on the same spot as did Herakleitos of Ephesos, when he said *Φυχῆs πείρατα οὐχ ἂν εξεύροιο πᾶσαν ἐπιπορευόμενος ὁδόν οὕτω βαθὺν λογον ἔχει.* Heracliti Ephesii Reliquiae (Oxford, 1877). "The limitations of the soul thou couldst not find, even if thou dost wander through every path to its end, so deep and full of meaning is the soul."

In general history, the great personalities can be correctly understood only in their reciprocal relation to the masses. The great man of general history is a mystery if he is studied as an isolated individual. Carlyle's treatise on *Heroes, Hero Worship, and the Heroic in History* (Century edition, 1897, Vol. V), impresses the biologist and the clinician as an overestimation of the individual. The same can be said of Cousins' *Theory of Great Men.* On the other hand, the activities of the masses as described by Buckle, Comte, and Condorcet, are exaggerations in the other direction. In the article on "Methodology," I have already emphasized that in the history of medicine, the masses have had no influence in the evolution of great ideas. If anything, their effect has been one of inhibition and retardation of progress. It is therefore surprising that an eminent general historian could declare that political historians incline to a high valuation of personalities, whereas historians of culture are disposed to a high valuation of the social psychic influence. This subject has been treated by W. R. Thayer, in his article on Biography as the Basis of History, in "Atti del congresso internazionale di scienze storiche" (April, 1903) Roma 1906, Bd. III, S. 574 ff.

The relation between individual genius and the masses can be correctly designated as a reciprocal relation in general history, and to each of these factors due importance can be attributed, but in medical history this is out of the question, for here the progress is entirely due to the great personality. Kautsky, though a very caustic and materialistic historian, does not deny the significance of leading intellects (See *Die Neue Zeit,* 15, Bd. 1, p. 20) and another severely materialistic historian, A. Labriola, Essais sur la Conception Materialiste de L'Histoire, Paris, 1897, expresses an appreciation of eminent persons in the following words: "Trying to eliminate them is pure fatuity," so even in general history, the appreciation of the leading thinker had held its ground.

PARALLEL DISCOVERIES FOLLOWING EACH OTHER IN CLOSE CHRONOLOGIC ORDER

The discovery of the infectiousness of puerperal fever by Oliver Wendell Holmes and Semmelweiss is an example of this.

Even general historiographers, investigating a problem from their special and specific viewpoint may end their research at a point where it appears concluded to them and yet it may not be concluded for the medical historiographer, for the former may have committed errors themselves; or authorities and experts of fairly equal renown may contradict each other on essential fundamentals; or an authority of acknowledged scientific ability and even international fame may omit to read original sources, become satisfied with transmitted or quoted information, to lighten his work, to save time; or he may be under the ban of an inescapable patriotism at a historic time when his country can no longer endure being deprived of the honor of a particular medical discovery because his country has been deprived of almost everything else that makes life worth living.

Such an attitude is that of the distinguished Prof. Paul Zweifel of Leipzig in his discussion of the priority dispute between Oliver Wendell Holmes and Semmelweiss concerning the infectiousness of puerperal fever, in *Archiv f. Geschichte der Medizin B. XII. 1920—P. 181.*

I do not intend to enter into this epochal polemic, which began with a brilliant clinical study by O. W. Holmes eighty years ago, in 1843. Zweifel considers himself the champion of Semmelweiss and on occasion of the fiftieth anniversary of the Announcements of this clinician in 1897 he delivered an address in defense of the priority of Semmelweiss. This year is signficant because fifty years from 1897 brings the date of the claim to 1847. But the date of the discovery by Oliver Wendell Holmes is 1843, as correctly stated by von Herff, Zweifel's own countryman, in *Handbuch der Geburtshilfe* (Winckel), Bd. 3, Teil 2, S. 333. The other arguments, namely, first, that Holmes failed to realize the significance of infection by direct contact of the infected fingers of the accoucheur or nurse, and that he believed in infection through the air only; secondly, that the mere mention of the word "nurses" (Pflegerinnen) by Holmes, who, in Zweifel's argument, never made digital examinations of obstetric cases, proves that Holmes meant infection through the air; thirdly, that the refusal of Holmes to discuss in particular the exact nature of the infection or contagion proves that he missed the significance of the immediate transfer of the contageous agent to the absorbing surface, had best be read in Zweifel's own interesting article (l. c.), but I doubt very much whether the historiographers of broad experience and conservative critical judgment who are not influenced by either American or German patriotic motives, will consider these arguments "überwältigender Beweis" (Zweifel, c. p. 187). Too much importance is attributed to the idea of infection *durch die Luft*—a most universal thought in the writers of eighty years ago, but which was not generally meant to exclude contact. Holmes does not say *through the air,* but distinctly explains "By the person of physician and nurses," i. e., contact.

Zweifel also misstates Holmes' middle name, calling it Mendell instead of Wendell.

American clinicians of our present era know how to value the exact and objective clinical researches of Semmelweiss; they fail to appreciate why the luster of his fame should suffer even if Holmes did anticipate his main ideas. A similar, almost simultaneous, discovery has occurred in other fields of science and by savants of different nationalities without diminishing the scientific merit of any of them in the estimation of historic connoisseurs of the present day. In the discovery of oxygen, for example (see my History of Lavoisier and the Physiology of Respiration, etc., *Johns Hopkins Hospital Bulletin,* vol. xxix, November, 1918), see Chapter XXI. Oxygen was discovered by the Englishman Priestley in 1774, by the Swede Scheele in 1775, by the Frenchman Lavoisier in 1775, by Hirn in 1781. But as early as 1681 John Mayow recognized that only a portion of the air was concerned in sustaining respiration and combustion. Further back, Leonardo da Vinci (1452–1519) stated that air is not in condition to sustain a flame, cannot sustain the life of any living creature (in his "Codex Atlanticus"). Leonardo da Vinci has expressed definite ideas on the importance of air in burning and breathing (see Leonardo da Vinci as a Scientist, Chapter XXIV).

The History of the Physiology and Circulation of the Blood also demonstrates that most great achievements in science are not reached suddenly, they do not spring completely (de novo) from the mind of a single individual, but they reveal themselves more or less gradually to several minds, sometimes partly to one and partly to another mind, sometimes many minds at different historic periods are—one might almost say—unconsciously building up by bits the integral parts of what has at a later period become the ground architecture of a great intellectual insight.

The controversy concerning the priority of either Holmes or Semmelweiss is in my opinion unprofitable as it leads nowhere. What is there in priority controversies anyway that a broad-minded historiographer should respect? In cases like the present the scientific merits of the work of each claimant should be ascertained by authorities that are nationally neutral, and after the *suum cuique* has been assigned the matter must be allowed to rest there. The weakness of the entire controversy lies in the fact that the contestants are as a rule countrymen of either claimant, and hence national bias cannot be excluded. It must be remarked finally among those Germans favorable to Holmes are von Herrff and Baruch, and likewise the Englishman Cullingworth. This entire polemic is introduced here, first, because it concerns an important contribution to Hygiene and Prophyllaxis that is widely known and therefore capable of being correctly judged in the broadest circles, and, secondly, because the element of national or patriotic feeling is very evident; viewed in this light it is not patriotism, but *pseudo* patriotism, and pseudo patriotism is a form of self-deception.

In comparing the pragmatic results of the publications of Holmes in 1843 and those of Ignatius Philip Semmelweiss in 1847, we have to concede a greater effectiveness to the prophylactic and antiseptic recommendations of Semmelweiss than to the simple methods suggested by Holmes. For the Viennese clinician intoduced compulsory washing of the hands of all those who came in contact with puerperal cases, and the disinfectant agent was calcium chloride. He brought about a reduction in the mortality from 9.92% to 1.27% even with the imperfect hospital arrangements of his obstetric clinic. He claimed that not only the hands of the accoucheur and nurses should be disinfected but all the instruments and bandage material. This, in reality, establishes him as the originator of that process which was later designated as asepsis.

Another parallel series of discoveries in which a great many participated is anæsthesia, or the artificial production of unconsciousness by various means for the purpose of executing an otherwise painful surgical procedure. It is a good rule in historiography before entering upon any discussion or research not to assume too much to be known. Even if the definitions necessary may tire an occasional advanced student of this special subject under consideration, still the great majority of other readers, even though they know the subjects discussed in one form of definition or another, yet they will appreciate being made familiar with that particular definition which the author has in mind, therefore, may I be pardoned for defining the terms, Discovery, Invention, the Founder of a Science, and the collateral conceptions that accompany these. The differences between so-called inventions, discoveries, first formulations of doctrines, findings in anatomy and histology, etc., vary according to the nature of the medical subjects.

Invention from *in,* meaning in or upon, and *venere,* to come, meaning to *come upon,* to *contrive.* 1. To frame by thought or imagination, to fabricate mentally. 2. To find out, devise, or produce for the first time, as by study, applied commonly to some serviceable mode, instrument, or machine. A musical composer may invent a theme or melody, or a new form of harmony. 3. To originate, elaborate a hither unknown object, instrument, plan, doctrine by the systematic working out of a mental process. *Dis-*

covery, from the French *decouvrir,* to uncover, to lay open to view, to reveal, to show what has been secret, unseen, and unknown. 2. To manifest, to exhibit. 3. To explore, examine, and reconnoitre. 4. To obtain for the first time sight or knowledge of, as of a thing existing already, but not known or perceived; to find out, to ascertain. The difference between the two processes, may be made clearer by a few examples. One discovers what had existed before but what had remained unknown. Goethe discovered the intermaxillary bone in man. Many anatomists, whose names are applied to various structures of the mammalian body discovered these ducts, bones, fissures of the brain, foramens. They did not invent them. Whitney invented the cotton gin; Galileo invented the telescope; Helmholtz (1851) invented the opthalmoscope. They did not discover these things. The laryngoscope, invented by Manuel G. Del Vincente Garcia, a singing teacher in London, in 1855, had many previous inventors, Cargniard de la Tour invented it in 1825, Benjamin Gay Babington in 1832, Liston had used a primitive laryngoscope in 1840; but it was not until Turck of Vienna and Czermack of Prague in 1857 thoroughly applied it to the clinical study of the larynx, trachea, and bronchi that it was introduced into scientific medicine. Bescherelle, (*Dictionnaire Natl.*) in referring to the manner in which discoveries are made says "Le moyen de faire des decouverts est de chercher toujour." Newton, when asked how he came to make so many discoveries, replied, "By always searching and seeking." There is no difference in the eminence of the great discoverer, or the great inventor—the talent required for either is equal. *The Founder of a Science* is the one who first recognizes a great complex of problems as a connected object for a special discipline and study, and who consciously determines and formulates both the extent and the limitations of that study.

QUALIFICATIONS AND LIMITATIONS OF DISCOVERIES; INVENTIONS, AND THE FACTOR OF CHANCE; ACCIDENT, FORTUITOUS CIRCUMSTANCES AND LUCKY THOUGHTS

There are two ways in which the factor of chance, fortuitous circumstances, or accidental coincidences demand consideration in connection with the subject of this chapter. One is in the way that these doubtful factors seemingly interrupt the clear cut consecutiveness of historic sequences in any medical historic research. The other is the element of fortuitous circumstance, chance, or accident preceding or during the immediate making of a discovery, invention or the formulating or conceiving of a new doctrine in medicine.

Most all of the great general historians call attention to the fact that there are causes and effects everywhere in history. The connection between this appears to be interrupted, abolished or at least clouded by other cause and effect actions that have no connection with the primary ones. This disturbing factor of historiography is described by Lewis, in a "Treatise on the Methods of Observation and Reasoning in Politics" London, 1852, vol. I, p. 434, and Lacombe, "Histoire Considéré Comme Science," 1894, p. 54. Here is a very pretty illustration of a methodic combination based on intuition to help us over such difficulties. That which we call accident or chance certainly does not mean "without cause." Rather does it signify that causative elements enter into a historic complex in a manner in which they are not recognized as necessarily causative. Sometimes later evidences prove that their entrance, although unknown and unrecognized, was really a very important driving factor. (A. Cournot "Considérations sur la marche des idées et des événements dans les temps modernes," 1872, vol. 1, p. 1.) In the special investigation of the feature of accident, chance, etc., in the making of medical or scientific discoveries in general, we have to be just as careful as we are in the research of general history. I have said elsewhere that Ostwald called the theory of electrolytic dissociation *ein glücklicher Gedanke,* or a lucky thought, but in his own essay on Svante August Arrhenius (born February 19, 1859, at Upsala) in "Die Forderung des Tages," Leipzig, 1910, p. 324, he himself describes the colossal amount of brainwork that must have preceded this lucky thought. All human beings of intelligence have what they call "lucky thoughts" at times, but this eventuates into nothing nine times out of ten, because the lucky thought does not fall on a thought complex already preëxistent in order to crystallize it into the coördination of a great conception. In other words, these experiences of great men which are said to follow so-called accidents, chance, lucky thoughts or to be the consequence of some fortuitous circumstance, are nothing but the final catalytic stimulus which sets into form a mass of conscious and subconscious thoughts and ideas that have antedated the supposed lucky thought. This will be more completely described in the chapter on the *Psychography of the Investigator.* Chapter XXXI.

When Goethe found and contemplated the bleached skull of a ram on the Lido at Venice in 1790, he said that the idea that the bones of the skull were simply modified vertebræ shot through his mind like a flash of lightning. The sheep's skull had been so macerated by water that the bones had separated and his intuition revealed to him that these greatly modified skull bones must be metamorphosed vertebræ. (See article on Goethe as a naturalist. Chapter XIX). This was not an accident or a chance. Goethe had entertained such thoughts for years before 1790. Without going into further illustration, I simply wish to emphasize that the assumed factor of fortuitous chance, lucky thought, etc., is nothing but a releasing element, a form of mental catalist which crystallizes out, as it were, a preëxisting heterogeneous mass of thoughts and ideas, hazily entertained opinions, and transforms them into a clarified, definite, orderly conception.

WHO IS THE REAL DISCOVERER OR INVENTOR?
WHO IS A FOUNDER OF A SCIENCE?

Not only the person on whose mind the hitherto unknown thing, truth, fact, process, flashes for the first time, but also the one who sees the connection of the new things with other things and truths before and after it, and their correlation with the truths and things of the past, present and future, also the one who recognizes the meaning and signifiance of a new fact—especially the one who elevates and makes the new thought shine forth with precision and clearness.

We must contrast here—

1—Mere collection of facts, a kind of pedantic (*πολνμαθιη*) "knowing much" which serves only for a personal reaching out and pressing forward—a tendency not in itself reprehensible as far as it can serve altruistic, but not personal aims.

2—We must contrast creative work that produces superiority with knowledge, which instead of becoming power becomes unyielding and sometimes even impeding and inhibiting mental ballast.

The real criterion of a new truth, thing or fact, etc., lies in its usefulness and its power for higher cultural advancement to mankind.

Culture is the summation, the totality of the values which man has created for himself; therefore any new truth or thing that does not add to this totality of values is not a genuine discovery, but only a pseudo-discovery. Where the latter is due to pardonable error, false premises, lack of precision, it may sooner or later be abandoned and thereby another possibility of another error avoided.

I do not mean that such pseudo discoveries which are often presented at first only in the form of hypotheses are always harmful, useless, or dangerous. Some of them, history shows, have for a time, at least, proved a stimulus to further investigation. I refer to Darwin's idea of pangenesis—which he himself later on called "regrettable," or to Goethe's theory of color vision, to Stahl's *phlogiston* theory, which at one time was accepted by the father of modern chemistry Lavoisier (see Chapter XXI).

The crucial question with such new thoughts is not simply whether they are ingenuous errors but whether they are disingenuous errors—advanced by the authors with full awareness that they could not be right. As yet there is no way of ascertaining this point in Medical Historic Methodology. Bernheim gives highly valuable maxims and rules of procedure in the 4th chapter (Kritik, p. 324) of his *Lehrbuch der Historischen Methode* and the system of procedure which Sudhoff uses in many of his *Studien* are of exceedingly great erudition. What is needed very urgently is a practical Syllabus or compendium of the Methods of Medical Historiography, such a compendium may unavoidably exhibit all the defects that such concise systems usually do, but it would be of great helpfulness in preventing useless writing of casual Medical History—"entertaining causeries" which serve merely the purpose of *Feuilleton,* light medical literature, tales and sketches. There is too much of this form of medical narrative; in its appeal to the popular taste it is frequently full of inaccuracies—quoted and requoted errors and devoid of personal investigation of original sources by the writer.

Even *Feuilleton* can be written so as to be free of these faults and whilst the free play of the imagination is entertaining, it is also liable to merge into the fantastic—in historiography at least it must be severely controlled. One of the tests of ingenuous error is the facility with which the author or originator corrects it—the readiness with which he receives arguments to the contrary. Volumes have been written and a lifetime wasted in futile efforts to convince a stubbornly erroneous pseudo-discoverer of his mistakes, so that the inference is not far from being right, which suggests that the vehemence of a man's defense and the heated persistence upon his ideas is inversely proportional to their correctness.

In Æsop's Fables, types of men are compared to characteristics of animals—men are likened to the lion, the fox, the ass, etc.—later on, even the invertebrates are drawn into the comparison—men are spoken of as lobsters, jellyfish, etc.—presumably to intimate that they are devoid of backbone. Now some pseudo-discoverers might be likened to the squid whose main defense is the squirting out of ink into the sea water. Many a human squid aims merely to confuse and obfuscate the situation by abundant use of ink in order to prevent recognition of the falsity of his views and theories.

THE TECHNIQUE OF INVENTION

Ostwald, in *Die Forderung des Tages,* page 155, quotes a letter of Schiller to Goethe in which the former makes the remark that it was possible to make inventions according to certain preëstablished principles. This inventing of things according to definite technical rule might concern the dramatist and the musical composer. I have great doubts whether it is applicable to scientific men in general and to medical investigators. I base my opinion upon a personal experience with great investigators in medicine and biology in this country, England, Germany, France and Italy, but as Ostwald is very insistent upon this conception, it will be instructive to learn how he acquired it. He selected the research of the celebrated botanist W. Pfeffer who found that the swarm spores of certain algæ have an independent motion in water and enter female blossoms with great certainty. Pfeffer asked the question whether the movement of the swarm spores could be determined by a chemical substance sent out from the blossoms of the female. He rubbed up in a mortar a number of female blossoms and brought the extract into glass tubes and discovered that the swarm spores went just as readily into these glass tubes as into the blossoms. The next question, he asked himself was what chemic substance is this? A chemical analysis of the ashes of the female flowers did not promise any exact results because these flowers contained many dozens of organic chemical compounds which were difficult to isolate. He had to approach the problem from another side and take a number of known chemical substances to test them for their attractive power on the swarm spores, but as there are thousands of possibilities here, it would have required thousands of experiments and much loss of time. Pfeffer then proceeded in what I have called the synoptic method. He took all of the chemic substances on the upper shelf of his chemical rack and mixed them together and made an experiment with a minute quantity of this composite material. Result: failure. He proceeded in the same way with all the substances on the next shelf, and so forth, until he did find a mixture which had an attractive power on the swarm spores. Assuming that he had a hundred different substances on the shelf which gave the positive result, he divided them into two equal parts, fifty to the right and fifty to the left. This way he discovered which fifty did contain the attracting substance and he divided those fifty into two groups of twenty-five each and those groups of twenty-five he divided into two groups, until he had eventually narrowed down to the possibility of one substance; it was malic acid. Now, Ostwald claims, this is the whole secret, namely, divide the whole field of possibilities into single parts which can be controlled technically and then investigate every part one after the other in its relation to the problem. Thus one cannot avoid finding that part in which the solution of the problem is contained. He even endeavors to express the technique of invention in form of an equation and claims that he has practiced it in his own home by giving his sons all kinds of technical problems to solve and ordering them, "Now find the solution of this problem," and he added that they learned to make inventions so quick, that he couldn't save himself from the many inventions that were made at his own table.

The weakness of this entire scheme consists in the fact that it really is nothing but a scheme, and schemes have only a very limited application in scientific investigations. Take for instance such a problem as the United States Yellow Fever Commission of 1900 undertook to discover the cause of yellow fever. Meditating over the field of possibilities and dividing it into single parts would not be at all applicable in such a research, nor could Ostwald's technique of investigation be at all serviceable when the problem was to invent a new operative technique, say for removing parts of the digestive tract. It might be serviceable in an investigation like that which Ehrlich undertook to discover an effective treatment for infection with spirochaetae, or for any disease of human beings caused by flagellate organisms. A similar process seems to have been pursued in the discovery of a new and effective treatment of trypanosome infections. "Uber das neue Trypanosomenheilmittel 'Bayer 205' und seine Bedeutung für die chemotherapeutische Forschung," by Martin Mayer in "Die Verhandlungen der Gesellschaft Deutscher Naturforscher and Ärzte," Leipzig, 1922. Some may say, these cannot be inventions and discoveries that are made in such a mechanical way. The answer is that, in truth, a great many inventions and discoveries have been made in that way. The weakness of the method consists in that its application is very restricted. Not everybody, as Ostwald says, can become an inventor in that manner. In order that a schema can be successfully applied, there must be an abundant measure of previous knowledge, plus the gift of fantasy, in order to equip the investigator for this type of plan. It is too frequently assumed that genius can grasp the correct solution of a problem intuitively. This is also an error and is one of the unfortunate inheritances of metaphysics.

Much depends, in all investigation, upon the guidance and direction of a very experienced teacher. It was a very portentous mistake of Ostwald to assert that men after fifty years of age were absolutely useless. My personal experience is that some of the finest scientific work that appears under the name of young investigators is in reality an idea or an inspiration given to them by some old master advanced in years, a man who can create enthusiasm for scientific work, a man of mature experience and magnetic temperament, such a man as W. K. Brooks, for instance, a teacher in biology, of E. G. Conklin, E. B. Wilson, T. H. Morgan, H. H. Donaldson, F. S. Lee, J. Playfair McMurrich, under whom I had the good fortune to study for two years, or such a teacher as Johannes Müller. The number of great masters who were educated by Müller constitute one of the miracles of medical and scientific history. The principal ones are Helmholtz, Schwann, Henle, Reichert, Peters, Remak, E. Du Bois-Reymond, Virchow, Brücke, Ludwig, Traube, J. Meyer, Lieberküln, Hallmann, A. V. Graefe, W. Busch, Max Schultze, and A. Schneider.

In establishing the criteria of a great discovery, it is important that the clinical observer or the scientific experimenter should be familiar with all possible similarities in the course of natural processes. In fact, the discovery of such similarities is the first step in the approach to a new law of nature and the recognition of such similarities requires considerable experience in natural science and thorough testing as to their genuineness before they are utilized for further research. Great achievements can only be accomplished by great work, and one of the criteria of a great piece of new work is that it permits of a deep insight into the connection of some important totality of natural complexes. There is too much of pseudo-investigation at the present time. If all of the researches published in all the scientific journals of America and Europe were correct and presented the complete proof concerning the problem, they presume to solve, there would be no more problems left for future investigation. As a matter of fact, much of the present day so-called scientific investigation is undertaken at universities and other institutions by persons who are mentally not fully equipped with the intellectual prerequisites for these problems or they are left too much to their own initiative when they should be under the direct influence of experienced masters. When richly endowed universities bring out such an abundance of pseudo-scientific investigation, there must inevitably be a few that hit the right mark. It would actually be an art to guess wrong every time. The literature of untested and unconfirmed speculation has no value for the progress of science. On the contrary, I fear that if a few sound thoughts are concealed in this mass of publication, they are hidden by the weeds of ill-digested thoughts, premature conclusions and a false "Fragestellung." Two thousand years ago Socrates expressed himself as follows: "They believe to know what they don't know. I have the advantage at least that I don't pretend to know what I don't know."

To make a discovery, then, requires that the so-called discoverer should control the evidence of much previous thought work on the same subject. If the discovery is to be in physics, it would not be very promising to have a medical man undertake the problem. If it is to be in therapeutics, it would not be promising to have a morphologist undertake it. The problem should be handed over to the exercise of men who are by training and experience fitted for the same. If therefore a dentist, who did not discover sulphuric ether nor even was the first to learn of its anæsthetic effects, is announced as the discoverer of surgical anæsthesia by ether, it would be a misapplication of the term to call him the discoverer or originator. Morton was not the real discoverer. He did not discover its anæsthetic properties. He was not a medical man but a dentist. He endeavored to exploit his discovery in a manner that was unethical. All in all, this man cannot be justly selected as a great medical discoverer. The literature on this subject which is recorded in the index catalogue of the Library of the Surgeon-General is replete with articles on the correctness of the objections I urged against William T. G. Morton's claim. Dr. Hugh Young in an article in the *Johns Hopkins Hospital Medical Bulletin*[1] has given the evidence, based on reliable historic sources, that Crawford W. Long of Danielsville, Georgia, was the first to anæsthetize a patient with sulphuric ether for the purpose of producing insensibility to pain. Morton obtained his knowledge of ether from Dr. C. T. Jackson, the chemist. It was not original knowledge, neither of the substance, of ether itself, nor of its effects. I don't wish to disparage the initiative of Morton in effecting a surgical operation and compelling mankind to hear him by the loud proclamation of his deed, but that is advertisement and not scientific discovery. On October 16, 1846, Dr. John Collins Warren, the Surgeon of the Massachusetts General Hospital, Boston, removed a tumor from the neck of a young man who was put under the influence of ether by the dentist Morton. On November 7, 1846, an amputation was made painlessly.* Morton had attempted to disguise the odor of the substance by aromatic oils and gave it the name of *letheon,* even endeavoring to secure letters patent upon his supposed discovery. I will here append a historic tabulation of the events in the development of our knowledge of anæsthesia.

CHRONOLOGIC REFERENCES TO THE HISTORY OF ANÆSTHESIA

64 A. D.—Dioskorides prepares an anæsthetic to be used in eye-diseases. It consisted of an alcoholic extract of the root of Mandragora. Dioskorides is the first to report this form of anæsthesia, but it was used by physicians of the School of Alexandria before the time of this great physician. It is very probable that Erasistratos, 310–300 B. C., was familiar with it. The active principle in mandragora which produces this narcosis is a substance very closely allied to scopalamin. Formerly this substance

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[1] Long, the Discovery of Anæsthesia, Vol. VIII, 1897, p. 174.

* But Crawford W. Long had produced Surgical Anæsthesia by ether and operated on James M. Venable, removing a tumor on March 30, 1842.

was known by the name of hyoscin, and was gained from hyoscyamus niger. For pharmacology see Meyer and Gottlieb, Die Experimentelle Pharmakologie, page 24 and 25.

360 A. D.—St. Hilary of Poitiers gives a clear description of Surgical Anæsthesia in his work *"De Trinitate."* It is best read in Schaff-Wace *"Nicene and Post-Nicene Fathers"* Vol. IX, P. 185. I quote it as follows:

"When through some grave necessity part of the body must be cut away, the soul can be lulled to sleep by drugs which overcome the pain and produce in the mind a death like forgetfulness of its sensory powers. Then the limb can be cut off without pain. The flesh is dead to all feeling and does not heed the deep thrust of the knife—because the soul within it is asleep." (360 A. D.)

1010—Ali ben Isa, employed narcotic mixtures supposedly consisting of extracts of mandragora and opium. Wrote the best work of the Middle Ages on the diseases of the eye.

1150—Nicolaus, preceptor of the School of Salerno, in writing a dispensatory with 150 formulas, is the first to refer to the so-called sleep sponges, which are sponges soaked in narcotic plant juices and dried, and before they were used for inhalation, they were dipped in hot water.

1220—Hugo D. Lucca, and Theodorick de Lucca reported to have produced narcosis by sleep sponges, by Guy de Chauliac.

1799—Humphry Davy, discovers the anæsthetic effect of nitrous oxide gas, with which Horace Wells in 1844 produced the first anæsthesia on himself.

1842—Crawford W. Long of Danielsville, Georgia, had already experimented with inhalations of ether in March, 1842. See History of Medicine by Roswell Park, page 306, also *Johns Hopkins Med. Bull.* Vol. VIII 1897, P. 174. Account of Long's operations published in 1849.

1844—Horace Wells undertakes the first narcosis with nitrous oxide gas on himself.

1846—Charles Thomas Jackson of Plymouth, Massachusetts, discovers that anæsthesia can be produced by the inhalation of ether vapor, and suggests the use of sulphuric ether to the dentist, William I. G. Morton.

1846—John Collins Warren after the discovery of the anæsthetic effect of ether, by the proceeding observers, executes the second series of surgical operations under narcosis with this substance in the Massachusetts General Hospital, October 16, 1846.

1847—James Young Simpson first uses chloroform to produce narcosis. See chapter on Simpson in Victor Robinson's "Pathfinders in Medicine."

1859—Charles Gardner Guthrie introduces amylnitrite as an anæsthetic.

1870—Bernhard von Langenbeck introduces ethylene chloride as anæsthetic.

1905–1906—James Meltzer and J. Auer published the narcotic effect of magnesium salts, especially magnesium sulphate, American Journal of Physiology, and the Journal of Experimental Medicine.

1907—Redard of Geneva demonstrates that narcosis [2] lasting several minutes can be produced when the rays of a blue electric lamp are allowed to act upon the eyes, and all other light rays are excluded.

History of Sulphuric Ether:

Lothar Meyer Geschichte der Chemie.
Kopp, Pages 299–300, Geschichte der Chemie, Vol. IV.

Ether was discovered the first half of the 16th Century. In 1807 Saussure and Guy Lussac in 1815 worked out the composition.

Ether became known through the labours of Frobenius in 1730, Hoffman, Pott, Baume. (Hoffman drops are admixed with spirit of wine C_2H_6O.)

The following men worked with diethyl ether at the various periods indicated:

Kopp, Page. 299, Vol. IV.
Raymond Lull in the 13th Century
Basilius Valentinus in the 15th Century
Valerius Cordus in the 16th Century
August Frobenius in the 17th Century (in 1540 described a method of making it).

Raymond Lull treated spirits of wine with H_2SO_4 by distilling both together.

August Siegmund Frobenius, Philos. Trans. in 1730 speaks of a Spiritus Aethereus—Ether. In a note appended to this paper it appears that both Boyle and Newton had directed their attention to ether.

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[2] Concerning the theory of narcosis, see E. Overton (Studien über Narkose, Jena, 1901), according to whom anæsthetics and narcotics enter the protoplasm of the cells in proportion to their fat solubility as compared to their water solubility. He assumes that the plasma envelope of cells contains lipoids which possess similar affinities for solution as the fats. The tissues of the nervous system and the fat tissues will contain more of a given anesthetic (chloroform) than other tissues and organs.

Frobenius made ether in Hankwitz Laboratory according to the directions of Newton (1727) with H_2SO_4 and C_2H_6O (Sulphuric acid and alcohol in equal parts) by repeated distillation.

An institution for gas inhalation anæsthesia was opened in 1793, known as the Hotwells Hospital, at Clifton, Bristol, England, by Dr. Thomas Beddoes, and Humphrey Davy was appointed as Dr. Beddoes' assistant and to conduct the institution. Every volatile substance was tried and amongst others ether, which Cullen in his lectures had recommended in bronchial irritation. Dr. Pearson, in writing to Dr. Beddoes from Birmingham, February 29th, 1797, stated that he had found the vapours of sulphuric ether, when inhaled, relieved the hacking cough of consumption better than anything else. Experiments in nitrous oxide gas were also carried on at the Hotwells Hospital, and on October 7th, 1794, Dr. James Watts reported that the gas produced a slight but uncommon nausea, attended with some elevation of spirit all evening, but no heat or thirst. One of Humphrey Davy's first duties was the preparation of gas and in a letter addressed to Dr. Gilbert of Penzance dated April 10, 1799, he writes "The gaseous oxide of azote is perfectly expirable. Mr. Davy suffering from the pain of a cutting wisdom tooth a few days later, inhaled nitrous oxide gas and found it had the desired effect. He also wrote "As nitrous oxide, in its extensive operation, appears capable of destroying physical pain, it may be used with advantage during surgical operations in which no great effusion of blood takes place." This great truth remained unnoticed for almost fifty years. Dr. Beddoes was too deeply interested in the treatment of tuberculosis to take up the question of pain-killing powers of new gas and the inhalation of gas had become a pastime. M. Filvee in his "Lettres sur l'Angleterre, 1802" wrote "Nitrous oxide gas has become a fashionable article of luxury and named it in the catalogue of follies to which the English were addicted. (See Janus, Vol. V, Pages 138–9, by George Foy). In 1818 Faraday and Orfila pointed out the anæsthetic effect of diethyl ether.

Why did Dr. C. W. Long lay claim to the discovery so long after Morton, Wells, and Jackson had put forward their claims? Dr. Charles T. Jackson in 1849, had much influence with the United States Senate and at a meeting with Dr. C. W. Long in the presence of his Honor, Judge C. W. Andrews, he tempted Dr. Long to agree to the statement that "He, Jackson, made the scientific discovery of the anæsthetic properties of Ether and that Long was the first to practically apply the discovery." Dr. Long was too honorable a man to entertain the idea of publishing a falsehood to the world.

How can we expect unanimity between different nations on disputes of priority when the claims of Boston, Philadelphia and Athens (Georgia) concerning the discovery of ether anæsthesia are not historically settled, and a university perpetuates the erroneously founded fame of its local favorite by a bronze memorial tablet etc.?

The history of the development of anæsthesia, therefore, is a continuous record from the year 64 A. D. to the present time, and no one particular individual can claim to be the sole discoverer of it. Concerning ether, the anæsthetic effects must have been known to the very first men who ever produced it, because it is so volatile, and because its attractive odor must have induced them to smell it, whereby they could not have failed to notice that a certain mental drowsiness resulted. Therefore, although the chemical substance itself and its pharmacological effects were not discovered by Morton, in line with American habit the glory of a therapeutic advance is given to the man who does most of the urging and pushing to get the substance in use, or as it is commonly said, "the man who puts it over." This, of course, is no scientific achievement. "Putting things over" is simply advertising, and certainly does not deserve to be credited with sufficient merit to immortalize a man by a place in the Hall of Fame.

PROPHECIES IN SCIENTIFIC EVOLUTION

In 1801 the great mathematician, Carl Friedrich Gauss, found a new method to determine the orbits of planets from four approximately close observations. He was a man who should always be remembered, not only on account of his eminence as a mathematician, but because it was he who brought the great physiologists, Wilhelm Edward Weber (1804) and Ernest Heinrich Weber (1790), to the knowledge of the scientific world. Gauss effected the call of Wilhelm Edward Weber to a professorship at the University of Göttingen. The Weber brothers are two of the most famous physiologists that Germany has produced. It was they who discovered the inhibitory action of the vagus nerve upon the heart, and who discovered very many other physiologic functions of nerves. Gauss gave the science of astro-physics a new method which was put into application by Piazzi, in his rediscovery of the Planet Ceres. He had discovered this planet on January 1, 1801, but it had since disappeared because of its proximity to the sun. This method was also of use in similar endeavors, but as we are speaking of exact methods for the purpose of scientific prophecy, I will have to cite Urbain Jean Joseph Leverrier of Paris, who from the irregularities in the movements of Saturn and Uranus concluded that there must be an unknown planet present beyond the orbit of Uranus, and he published the orbit of this planet on the 31st of August, 1846. John Couch Adams of Cambridge had preceded him, but had only communicated his calculation in October, 1845, to the astronomer Airy, and as no further observations were made the priority of the discovery, that of Neptune, was conceded to Leverrier. Prophecies must be accepted as such by the contemporaries of the scientific prophet. The difficulty with historic prophecies is not that they are impossible, but that they are not accepted by contemporaries, but rather rejected, because of the unwillingness of a large proportion of humanity to accept, believe, and act according to a new idea that works contrary to innate conventionality, and contrary to beliefs and practices to which the masses have become habituated.

PROPHECIES IN THE PERIODIZATION OF CHEMICAL ELEMENTS

In 1817, Dobereiner demonstrated that very definite regularity existed between the atomic weights of certain similar elements. For instance, he showed that three similar elements exhibited one among their number, the atomic weight of which was the arithmetic medium of the other two. Such triads are first Lithium, Sodium, Potassium; secondly, Calcium, Strontium, Barium; thirdly, Sulphur, Selenium, Tellurium; fourthly, Chlorine, Bromine, Iodine. He believed that on relations of the atomic weights one could erect a system of chemical elements. This idea was later on further developed by Pettenkofer, 1851; Dumas, 1859; John Chancourtios, 1862, but complete success was achieved in 1869 by the great Russian chemist, Mendeléeff, and the German chemist Lothar Meyer. Mendeléeff's wonderful work has been translated into English under the title "The Principles of Chemistry." Both chemists arrived at a natural system of the elements by arranging them according to their decreasing atomic weight, and in this it was demonstrated that similar elements, those which belonged to one family, followed each other in regular periods. Many properties of the elements appeared, therefor, as periodic functions of their atomic weights. These systems showed numerous gaps; formerly the gaps were more numerous than at the present time. For example, Germanium, Scandium, Gallium were wanting when these systems were established, but before these elements were discovered (and this is a very brilliant corroboration of the exactness of the mathematico-chemical prophecy), Mendeléeff described their properties on the basis of his periodic system and the lawfulness of the systems of Lothar Meyer and Mendeléeff are a guarantee for the fact that the atomic weights deducted by other methods were correct. That the system is not perfect is conceded, but that it is a wonderful confirmation of the methods of the human mind to construct thought bridges over great vacancies in science is also conceded.

VII

STATISTICS AS AN ADJUVANT METHOD IN HISTORIOGRAPHY[1]

IN the chapter on Laws in General and Medical History it has been made clear that in general as well as in medical history, laws in the sense in which they are defined by the Duke of Argyll or by Ernst Bernheim are not traceable in the history of the past or present. No parallelism can be drawn between the operation of laws in natural science and in history, and furthermore, what has been erroneously mistaken as the operation of law in history is almost exclusively traceable to the effects of psychic causality. To the present day psychology has not been expressed by exact quantitative, scientific methods, and therefore, psychic causality which the human mind must accept as a resting point for historic circumspection, is itself not measureable in all of its important domains by the standards of mathematics and physics. The next effort to succeed in arranging of categories and classifications easily comprehensible was to probe for motives and purposes in historic activities. Now if every interest in history and the work bestowed upon the realization of interests could be isolated and analyzed in all its constituent elements, and if the determinable and constituent factors of human interest could be homogeneously distributed upon all human beings, then we could make possible an ideal of scientific comprehension and establishment of laws in history. Such laws would then control the purposes of men. It is thinkable that any special transaction, event, state of evolution or progress could then be fitted under some particular law, as a case categorically fitting that law, and it is conceivable that we could solve and exhaust all the conditions, causative to that special case, and the theoretical applications of its effects for future happenings under identical conditions. In other words, we could exhaust a historic problem just like we exhaust some scientific problems, in a causal-analytic manner. The only human endeavor that aims at a foundation for such systematic procedure is found in Statistics. So-called medical statistics have a purpose and cover a domain entirely different from statistics as they are generally and experimentally made use of in historiography. In historiography, as we have seen, we must have an approximate knowledge of the purposes and motives of humanity if we are to approach the solution of historic problems. There is no realm of human thought, aspiration or activity so devoid of systematic uniformity as that of human purposes, and the most complete knowledge of the history of the past reveals no intertwining of human purposes that would correspond to the world situation of to-day. That being the case, it seems highly improbable that our present historic situation could offer statistical data whereby we could prescribe to posterity how they are to arrange and conduct their lives.

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[1] For the history of Statistics as a scientific Method, see Article on Methodology.

It has been said in explanation of the historic significance of statistics that regardless of the impulses and psychologic specificity of the individuals in their activities, the actions of the great masses of society, even those that appear as the most voluntary actions, show such a constant regularity as we are accustomed to experience only with natural phenomena, and that statistics in order to yield correct deductions and prognostications need only to be expanded among as great a number of activities and domains of human events as possible and then we shall be enabled to recognize lawfulness everywhere, and also to predict the activities of the most diverse groups of society. That striking figures regarding the relations between human purposes and human action have occasionally been revealed by such methods is indubitable, but they have led to the hasty conclusion that so-called laws for all human action could be found provided the observations and problems could be expanded on a sufficiently large number of historic processes. Such observers have even expressed the hope that a method of procedure based on such statistics would reveal the activity of a so-called purpose-consciousness. See L. A. J. Quetelet, "Sur l'homme et le développment de ses facultés, ou Essai de physique sociale" (1835) and "Du système social et des lois qui le régissent" (1848). Having found the statistical method as a sufficient explanation for the action of human beings living at the present time it was thought possible that the same mathematic procedure could be used regressively for the study of historic transactions of the past, and that thereby history could be elevated to the rank of an exact science. This is the standpoint that was taken by Henry Thomas Buckle in his "History of Civilization in England," London, 1857. Buckle, however, makes use of points of inquiry in testing his inferences which are not traceable to the purpose-consciousness. For instance, the number of letters devoid of addresses that are taken out of the letter boxes of all England in a given year is certainly a matter of forgetfulness and this is not a purposive action, nor is the number of suicides in time of severe historic crises a logical test of the statistical method, for the thought that suicides must be induced by conditions that make life harder to live must have preceded the statistical investigation of this subject, but statistics cannot instruct us as regards the causal relation between pecuniary, social, economic, and war misfortunes and the number of suicides in a given year. It has been emphasized by A. D. Xénopol that a quiet contemplation of such statistical relations will lead us to the conclusion that it really would be astonishing if a certain regularity of the total conduct of very large groups of human beings were *not* observable as long as the disposition and the conditions of life continued the same.

Raymond Pearl in his "Introduction to Medical Biometry and Statistics" says: "It is doubtless too much to hope that this chapter will make Newtons out of all its readers, but it seems desirable to give the medical man some little introduction to the methods which the followers of the sciences at the moment more exact than medicine, use in fitting together mathematical expressions and observational data. It should be made clear at the start that there is, unfortunately, no method known to mathematics which will tell anyone in advance of the trial what is either the correct or even the best mathematical function with which to graduate a particular set of data. The choice of the proper mathematical function is essentially, at its very best, only a combination of good judgment and good luck. In this realm, as in every other, good judgment depends in the main only upon extensive experience. What we call good luck in this sort of connection has also about the same basis. The experienced person in this branch of applied mathematics knows at a glance what general class of mathematical expression will take a course, when plotted, on the whole like that followed by the observations. He furthermore knows that by putting as many constants into his equation as there are observations in the data, he can make his curve hit all the observed points exactly, but in so doing will have defeated the very purpose with which he started, which was to emphasize the law (if any) and minimize the fluctuations; whereas actually, if he does what has been described, he emphasizes the fluctuations and loses completely any chance of discovering a law.

"Of mathematical functions involving a small number of constants there are but relatively few. If one takes account of that group of curves, which in his youth he studied under the name of 'conic sections,' adds to it the curves which are derived from the trigometrical functions, and fills out the equipment with the logarithmic-exponential family, he will not have exhausted the possibilities of curves with few constants, but he will have included the great bulk of the mathematical functions which have so far been found to be of wide utility in expressing the laws of nature. In short, we live in a world which appears to be organized in accordance with relatively few and relatively simple mathematical functions. Which of these one will choose in starting off to fit empirically a group of observations depends fundamentally, as I have said, only on his judgment and experience. There is no higher guide."

Ernst Bernheim in his "Lehrbuch der Historischen Methode" points out with great clearness that the social conditions and motives that actuate human beings generally and which one would presume could best be approached by the statistical methods are the very factors which present the greatest difficulties to this method. He quotes, for instance, the relation of the cost of living to the number of marriages in a given time. One would presume that the most decided factor would be proven to be the cost of the necessities of life. But an investigation of Bernheim shows that this is not the case but the greater or lesser determination of the individuals in their decision to be married is the deciding factor. Now this determination may be influenced, it is true, by the cost of living, but it is influenced by many other motives, among which are: first, the intensity of the sentiment and loyalty between the two parties concerned; secondly, the consideration and regard for the social status and needs of a future family. These motives are within a certain social group and in a certain time determined by the educational standard of the persons to be married, by their social aspects, and by the character of the people among whom they are living. It is also made evident that within the domain of social human life there is no complex of conditions which necessarily would be sufficient to compel a definite human action with certainty. Close scrutiny brings us to the insight that the great number of so-called mass actions and mass proceedings are after all, in their final analysis, due to the incentives of individuals. J. von Kries in his principles of the calculation of probabilities criticizes the so-called tacit premise of constants for general conditions. It is, therefore, incorrect to assume that deductions made from great numbers by the statistical method are infallibly followed by an absolutely correct conclusion. For the so-called statistical Law is in reality nothing but the expression in figures of the fact that any human action may be due to a great variety of motives. It does not explain these motives. The statistical deduction does not say how they come about. In the example of Ernst Bernheim, for instance, the statistical expression states that if in a given society the cost of living changes in a given direction, then the frequency of the marriages also changes in a definite way. But the statistical method cannot explain why a special society reacts to such external conditions in a given manner, and why another social group reacts in a different manner. By this method it is not possible to assert that every change in the cost of living will result with the same effect on the number of marriages because statistics cannot draw into its calculations all the motives and basic factors of human action. Most of the so-called statistical rules assume that the psychic and social dispositions and the *Anlage* of a given society are known once and for all as unchangeable uniform quantities, or as constants, whereas, in fact, they are very complicated, variable and ofttimes unknown quantities. Raymond Pearl's instructions (l. c.) for medical men are mainly for the purpose of introducing exact methods for the observation of medical factors, for the study of inheritance factors, for the evaluation of clinical records, etc., but he states (l. c. page 333, as already quoted) "The choice of the proper mathematical function is essentially at its very best only a combination of good judgment and good luck." If such an experienced teacher of vital statistics and biometry is so careful in his limitations of his own methods, how much greater must the care and how much more extensive must the experience be when such methods are used for the verification and confirmation of historical deductions. When it comes to the effort to determine the effectiveness of a historical personality quantitatively, statistics desert us and even when we are dealing with what the collectivists enthusiastically term "calculable mass procedures" the method is doubtful also. I refer for this whole discussion to Bernheim's "Lehrbuch" from page 100 to 130. An extreme view concerning the problematic value of statistics in history is expressed by A. D. White in his "Studies in General History and the History of Civilization" (New York, 1885), page 18, where he states: "Meeting our ethical necessity for historical knowledge with statistics and tabulated sociology entirely or mainly, is like meeting our want of food by the perpetual administration of concentrated essence of beef." Nevertheless, there is a very important field for the experimental application of the modern statistical methods to history and that concerns mainly those periods of history in which exact numerical and statistical records have been kept. With all due respect to so great a historiographer and master of methods as Prof. Ernst Bernheim, I am diffident about his statement (on page 123, l. c.) concerning the application of statistics to contemporaneous times and to the future. He inquires, how far must the collection of statistical data be extended to furnish the necessary material on all domains of science and knowledge and historic activities? "WITHOUT DOUBT," he continues, "ONE HALF OF HUMANITY WOULD HAVE TO BE OCCUPIED TO CONTROL BOTH ITSELF AND THE OTHER HALF IN ALL REQUISITE STATISTICAL RELATIONS."

The largest part of the historic transactions of masses belongs to the past and as there are no numerical records of any consequence handed down to us from this great past it is impossible to study this record statistically and if scientific historiography depended exclusively on statistics then it could only begin with modern times. When I stated that there are no numerical records of any consequence concerning the past, I am fully aware of the numbers given in the histories of various races and nations and the dates they have handed down to us for their great historic events and experience; thus we find in the biblical records numbers pertaining to the human beings that emigrated from Egypt under Moses. We find (600,000 fighting men) with families the aged and the young and in other parts of the Bible we have references informing us of the number of those who died in epidemics and of those who were killed in battle, even the number of those recorded in a census. E. Kautzsch and his collaborators in their excellent and thorough study of the Bible entitled "Die Heilige Schrift des Alten Testaments" attribute importance to these numbers. We find similar numbers given to us when Ezra conducted the captive Jewish tribes from Babylonia back to Jerusalem. Most of the other nations whose history runs parallel with that of the Jews have left us no such numerical records and these only can be found, when great catastrophic disaster, like war, pestilence, or volcanic destruction, overtook them. Concerning the nobler transactions of history like the gradual evolution of culture and civilization there are practically no numerical records. In short the past has left us very few dependable figures for statistical studies in history, moreover there was the regrettable tendency of the victor to exaggerate the numbers of the vanquished; also the tendency of the Oriental races to fantastic play of the imagination, but more especially, hardly anything was recorded numerically except that which concerned large numbers of human beings or large numbers in anything. If there were such a thing as the *Law of Great Numbers* as Buckle expresses it, meaning events and processes that concern great numbers of the human beings which could be determined quantitatively, it would be applicable only in comparative Demography or Demology in problems concerning the social and political affairs of the people, but as numbers have no meaning unless they can be associated with motives, statistics desert us when we attempt to ascertain the intensity of the feelings, convictions, and dispositions that prevail among the people and times that we are studying. Statistics also present no means of understanding the degree of Altruism or Egoism in the great historic personalities, nor can any factor that actuated either the masses or the great individuals be ascertained by this method.

In medical history our main object is to seek an explanation of the historic events and proceedings by tracing them to their causes with the help of the methods of regressive analysis, which in the chapter on methodology I have attributed to Thucydides. In the chapter on laws in history it was explained that it is impossible to trace historic events to laws. The so-called lawful explanation of history meets with many practical difficulties, especially in medical history; even after we have succeeded in tracing events, transactions, and proceedings to the factors which gave rise to them, we find an abundance of causes, complex influences, and causes of causes that condition and lead up to the effect, which we are studying. We are fortunate to possess a medical writer, who has studied some of these conditions and who has declared that the data arrived at in such regressive analysis for historic causes are impossible for practical application because of their complexity, inadequacy, and some times unsubstantiated character. This author is the celebrated physiologist Emil Du Bois-Reymond ("Über die Grenzen des Naturerkennens," 1873, p. 11). Emil Du Bois-Reymond occupies the unique position of being an extreme collectivist, a term which has been explained in the chapter on ideas as historic factors, Chapter IV. He asserts that nothing else is worth knowing in history but the general average results of the achievements of culture and the general forms of social processes and institutions; how all these states came about is not worth knowing. Du Bois-Reymond calls this the "Archimedean" standpoint. The reason for this curious designation is not very clear to me, certainly none of the principles of mechanics or hydrostatics could be applied to such an exclusive perspective on history. What the designation refers to appears to be the remark of Archimedes that if he were given a firm position in space outside of the earth he could invent devices for mechanically moving it. If Du Bois-Reymond had this statement in mind, it should be added that it is historically not well founded. It appears to be one of those highly imaginative flights of assertion met with in the narratives concerning all great men. Very probably the brilliant Berlin physiologist meant an aspect of history sublimely elevated above the affairs of this earth, or a remote viewpoint away from the earth whereby the observer could not discern the individual details of historic processes, but only the vast general movements. The same idea is conceived by E. Mach (Erkenntnis und Irrtum, 1905, p. 25) who characterizes the view that would be offered to a historic observer of humanity from a position on the moon, from which standpoint one could observe only human beings, who would multiply, grow, feed themselves and contest with each other with great regularity. Such an observer would know nothing of motives, feelings and ideas of the masses, but historians could not operate with such a type of observation. For them distance does not lend enchantment. The great aim of the historian is to gain insight into the events, personalities and proceedings from as close a proximity as possible. A remote standpoint, either spacially or chronologically, is to be avoided.

Bernheimm ("Lehrbuch's," s. 125) refers to these speculative indulgences of collectivistic historians with delicate irony, stating "the Archimedean standpoint is not the one for historiography, for this science is rooted on the earth and for the earth."

Medical statistics as a means for clarifying medical research and clinical studies are indispensable, but as a means for elucidating historical events and processes it is only an accessory method, which brings general deductions of relations and conditions of the past. Occasionally the statistical method may reveal a concealed cause or call attention to oversights in the inward connection of events. If precise formulation of mass phenomenon is desired, the statistical method is the only one to be employed, provided the conditions for such method are present in any given case.

Henry Thomas Buckle wrote a history of civilization of England which appeared in two volumes from 1857 to 1861. The greater part of this work is concerned with the conditions of the development of history. He argues that it should be raised to the exactness of a real natural science. In order to discover supposed laws of history, Buckle analyzes the factors which appear to condition history and he finds two of them: first, nature and secondly, the intellect of the human being. He ignores any contemporaneous elements of culture and asserts that man has emancipated himself from nature and became her master in the temperate zone and accordingly he argues that all historic evolution in the temperate zone is determined by the intellect of man. All progress in culture and civilization are accordingly attributed to human reason and intellect. He asserts that the moral incentives in man remain stationary; if they are changed at all it is effected by increased understanding, not by a higher noble-mindedness. It is for this reason that he could say: "The progress of civilization is marked by the triumph of intellectual over the moral law." It is a statement in which he places himself in opposition to the greatest thinkers that have ever taught the human race, but the most curious feature in the chain of Buckle's argumentation is his effort to find the laws of the human intellect by the statistical method, the limitations of which have already been dwelt upon.

VIII

TENDENCIES IN MEDICINE IN THE TWENTIETH CENTURY[1]

CLINICAL and scientific investigations are not calculated to develop the gift of rhetoric nor the esthetics of expression. Boerhaave, one of the older Holland clinicians, who was born in 1668, is the author of the motto *Simplex sigillum veri,* and scientific men, conscious of the power of truth, are gradually coming into the manner of stating it in the simplest possible manner. And modesty is a characteristic of their delivery; oratory and science do not go hand in hand. The brain, grave with facts, is in its expressions simple, modest, and sweet, except in rare instances where great emphasis requires force of statement. Gaze upon the ears of growing wheat—those that contain no fruit raise their heads proudly above the field, but lowly and modest do those bow to the earth that are heavily laden with seed.

Tyndall once compared this to certain crystals of fluorspar, which, having lain in the depths of the earth darkly hidden for ages, reflect no light, are not fluorescent, but nevertheless have a potency of light locked up within them. The light in the crystals is held by a peculiar molecular structure, a detent, but when these crystals are heated the detent is lifted and an outflow of light at once begins.

Some similar check or inhibition is lifted from me whenever I address physicians. The crystal when warmed by social congeniality begins to show light, though encumbered by a molecular detent imposed by habit and the doctrine of Boerhaave, *Simplex sigillum veri.*

This evening, as I followed the massive and subtle "motives" of the Tannhäuser overture, as I had followed them many times before, the idea came to me that to a musically-trained mind it should not have been difficult to foretell at the time when Wagner first brought out his unique and original compositions that in them there was contained the evidence of a genius that must serve in future as a beacon light for other composers; and indeed at the very time of the first production of "Tannhäuser," certain German musicians, gifted with acuteness of vision, applied to it the designation of "Music of the Future." How true this prediction of Liszt has come to be, may be well known to you from the fact that to-day it is considered the highest ideal of dramatic music. But to reach this ideal Wagner did not confine himself strictly to the influence of music pure and simple, but he called to his aid other arts, and in his work we find a classical combination of poetry, the drama, painting, and music.

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[1] Read before the Medical and Chirurgical Faculty of Maryland, Tuesday, April 24, 1900. *Maryland Medical Journal,* August, 1900.

Not only was it possible for minds, musically trained, to foretell that this talented composer represented a type to be imitated by future ages, but by the highly artistic combination and elaboration of what were called musical "motives," it was in many instances possible to foretell in the beginning or first act of one of his operas the manner in which the various themes would be utilized in the climax.

In medicine, as in music, it is sometimes possible to foretell coming events, not by any extraordinary acuteness of vision or prescience, but by following the tracks of facts already established and observing their direction out into the unknown domain, the hazy territory of the times that are coming. As Juvenal said, *Nequeo monstrare, et sentio tantum* ("I cannot embody it in words, but can feel it in my soul"). The statements which may be made concerning the future development of our science are not based on experimental evidence entirely, but are advanced from the psychic conviction that certain things, facts and conditions, which are not now, certainly must eventually be. To begin with the better beaten road of established facts would compel me to refer, though briefly, to that which has been accomplished in this present century, not only in medicine, but in other natural sciences, for the coming progress of medicine will be associated with the progress that is made in other natural sciences, especially physics, chemistry, and physiology. Evidently it will be impossible to even outline the tremendous amount of work that has been accomplished during these past 100 years in these domains of knowledge. Under chemistry I should have to refer to the life and work of John Dalton, Johan Jakob Berzelius, Joseph Louis Gay-Lussac, Justus von Liebig, Gustav Robert Kirchhoff, Robert Wilhelm Bunsen, Louis Jacques Mandé Daguerre, and John W. Draper. Under physics, to Thomas Young, Michael Faraday, Hans Christian Oersted, Arago, Fresnel Julius Robert Meyer, James Prescott Joules, Sir William Thompson (Lord Kelvin), John Tyndall, James Clerk Maxwell and Roland Helmholtz. In anatomy and physiology I should have to mention the epoch-making work of Karl Ernst von Baer, William Hyde Wollaston, Matthias Jakob Schleiden, Marie François Xavier Bichat, Jean Baptiste Dumas, Claude Bernard, William Benjamin Carpenter, Hugo von Mohl, Johannes Müller, and Max Schultze. To give the century's progress in biology it would be our duty to speak of Erasmus Darwin, Jean Baptiste de Lamarck, Etienne Geoffrey Saint-Hilaire, Charles Robert Darwin, Alfred Russel Wallace, Thomas Henry Huxley, Asa Gray, and Ernst Heinrich Haeckel.

A glance into the future should reasonably be preceded by a glance into the past.

For the historical references in the revision of this essay I am indebted to the articles in Sudhoff's Archiv and the essays by H. S. Williams, *Harper's Mag.*, Hist. of Science, vol. IV, 1897, from which the following account of the discovery of auscultation, percussion, and anesthesia have been taken.

Two French monarchs have been instruments, unwillingly perhaps, for the advance of medicine. One was Charles IX, who, in proclaiming that all Protestants in France should be put to death on St Bartholomew's day, made an exception, that of Ambroise Paré, the father of French surgery. It is probable that this was done from purely selfish motives, for in Paré he preserved the healer of his army's wounds.

Although Napoleon Bonaparte, First Consul, was not lacking in self-appreciation, he probably did not realize that in selecting a physician for his own needs he was markedly influencing the progress of medical science as a whole. Yet so strangely are cause and effect adjusted in human affairs that this simple act of the First Consul had that very unexpected effect, for the man chosen by the envoy of a new method in medical practice, and the fame which came to him through being physician to the First Consul, and subsequently to the Emperor, enabled him to promulgate the practice in a way otherwise impracticable. Hence the indirect but telling value to medical science of Napoleon's selection.

The physician in question was Jean Nicolas de Corvisart. His novel method was nothing more startling than the now familiar procedure of tapping the chest of a patient to elicit sounds indicative of diseased tissues within. At the beginning of the century, Corvisart, and perhaps some of his pupils, were probably the only physicians in the world who resorted to this simple and useful procedure. Hence Napoleon's surprise when, on calling in Corvisart, after becoming somewhat dissatisfied with his other physicians, Pinel and Portal, his physicial condition was interrogated in this strange manner. With characteristic shrewdness, Bonaparte saw the utility of the method, and the physician who thus attempted to substitute scientific method for guesswork in the diagnosis of disease at once found favor in his eyes, and was installed as his regular medical adviser. (Henry Smith Williams, *Harper's Monthly Magazine*, 1897.)

For fifteen years before this Corvisart had practiced percussion, as the chest-tapping method is called, without succeeding in convincing the profession of its value. The method itself, it should be added, had not originated with Corvisart, nor did the French physician for a moment claim it as his own. The true originator of the practice was the German physician Auenbrugger, who published a book about it as early as 1761. See James J. Walsh, Makers of Modern Med., Auenbrugger p. 53 and Laennec p. 133. This book had even been translated into French, then the language of international communication everywhere, by Rozière de la Chassagne of Montpelier in 1770, but no one other than Corvisart appears to have paid any attention to either original or translation. It was far otherwise, however, when Corvisart translated Auenbrugger's work anew, with important additions of his own, in 1808. By this time a reaction had set in against metaphysical methods in medicine that had previously been so alluring. The scientific spirit of the time was making itself felt in medical practice, and this, combined with Corvisart's fame, brought the method of percussion into immediate and well-deserved popularity. Thus was laid the foundation of so-called physical diagnosis, which is one of the cornerstones of modern medicine.

The method of physical diagnosis as practiced in our day was by no means completed, however, with the work of Corvisart. Percussion alone tells much less than half the story that may be elicited from the organs of the chest by proper interrogation. The remainder of the story can only be learned by applying the ear itself to the chest, directly or indirectly. Simple as this seems, no one thought of practicing it for some years after Corvisart had shown the value of percussion. Then, in 1815, another Paris physician, Réne Théophile Hyacinthe Laennec, discovered almost by accident that the sound of the heart-beat could be heard surprisingly through a cylinder of paper held to the ear and against the patient's chest. Acting on the hint thus received, Laennec substituted a hollow cylinder of wood for the paper, and found himself provided with an instrument through which not merely heart-sounds, but murmurs of the lungs in respiration could be heard with almost startling distinctness. (Victor Robinson's Laennec, in *Pathfinders in Medicine,* 1912).

The possibility of associating the varying chests sounds with diseased conditions of the organs within appealed to the fertile mind of Laennec as opening new vistas in therapeutics, which he determined to enter to the fullest extent practicable. His connection with the hospitals of Paris gave him full opportunity in this direction, and his labors of the next few years served not merely to establish the value of the new method as an aid to diagnosis, but laid the foundations also for the science of morbid anatomy. In 1819 Laennec published the results of his labors in a work called *Traité d'Auscultation Médiate,* a work which forms one of the landmarks of scientific medicine. By mediate auscultation is meant, of course, the interrogation of the chest with the aid of the little instrument already referred to, an instrument which its originator thought hardly worth naming until various barbarous appellations were applied to it by others, after which Laennec decided to call it the stethoscope, a name which it has ever since retained.

In subsequent years the form of the stethoscope, as usually employed, was modified, and its value augmented by a binauricular attachment, and in very recent years a further improvement has been made through application of the principle of the telephone; but the essentials of auscultation with the stethoscope were established in much detail by Laennec, and the honor must always be his of thus taking one of the longest single steps by which practical medicine has in the 19th century acquired the right to be considered a rational science.

Laennec's efforts cost him his life, for he died in 1826 of a lung disease acquired in the course of his hospital practice, but even before this his fame was universal, and the value of his method had been recognized all over the world. Not long after, in 1828, yet another French physician, Piorry, perfected the method of percussion by introducing the custom of tapping, not the chest directly, but the finger or a small metal or rubber plate held against the chest—mediate percussion, in short. This perfected the methods of physical diagnosis of diseases of the chest in all essentials, and from that day till this percussion and auscultation have held an unquestioned place in the regular armamentarium of the physician.

Meantime, in quite another field of medicine, events were developing which led presently to a revelation of greater immediate importance than any other discovery that had come in the century perhaps in any field of science whatever. This was the discovery of the pain-dispelling power of the vapor of sulphuric ether, inhaled by a patient undergoing a surgical operation. This discovery came solely out of America, and it stands curiously isolated.[2] Davy, in England, had indeed originated the method of medication by inhalation, and carried out some most interesting experiments fifty years earlier, and it was doubtless his experiments with nitrous oxide gas that gave the clew to one of the American investigators. Since the beginning of the century, when Davy turned his attention to other matters, no one had made the slightest advance along the same line until Crawford Long and Morton in America renewed the investigation.

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[2] The entire History of Anaesthesia is reviewed and discussed in Chap. VI.

Moreover, there had been nothing in Davy's experiments to lead anyone to suspect the possibility that a surgical operation might be rendered painless in this way, and, indeed, the surgeons of Europe had acknowledged that all hopes of finding a practical means to secure this most desirable end must be abandoned—that the surgeon's knife must ever remain a synonym for slow and indescribable torture. By an odd coincidence, it chanced that Sir Benjamin Brodie, an acknowledged leader of English surgeons, had publicly expressed this as his deliberate though regretted opinion at a time when the quest which he considered futile had already led to the most brilliant success in America, and while the announcement of the discovery, which then had no transatlantic cable to convey it, was actually on its way to the Old World.

The American dentist just referred to, who was, with one exception to be noted presently, the first man in the world to conceive that the administration of a definite substance might render a surgical operation painless, and to give the belief application, was Dr. Horace Wells of Hartford, Conn. The gas with which he operated was nitrous oxide; the operation which he rendered painless was no more important than the extraction of a tooth, yet it sufficed to mark a principle. The year of the experiment was 1844. Prior to Wells, Crawford W. Long had already operated under ether narcosis in 1842 (see page 147).

The experiments of Dr. Wells, however, though important, were not sufficiently demonstrative to bring the matter prominently to the attention of the medical world. The gas with which he experimented proved not always reliable, and he himself seems ultimately to have given the matter up, or at least to have relaxed his efforts. The relations of Wm. T. G. Morton and C. T. Jackson to the history of anæsthesia by Sulphuric Ether have been set forth on p. 146 where the important facts of the gradual evolution of the idea of anæsthesia are arranged in chronologic order.

The Historic evidence that Crawford Long of Jefferson, Ga. operated under Sulphuric Ether Anæsthesia in March, 1842, has been investigated by Hugh H. Young and one can but agree that the sources and evidence brought out by Dr. Young are correct.

Swiftly as steam could carry it—slowly enough we should think to-day—the news was heralded to all the world. It was received in Europe with incredulity, which vanished before repeated experiments. Surgeons were loath to believe that ether, a drug that had long held a subordinate place in the armamentarium of the physician, could accomplish such a miracle. But skepticism vanished before the tests which any surgeon might make, and which surgeons all over the world did make within the next few weeks. Then there came a lingering outcry from a few surgeons, notably some of the Parisians, that the shock of pain was beneficial to the patient; hence that anesthesia—as Dr. Oliver Wendell Holmes had named the new method—was a procedure not to be advised. Then, too, there came a hue and cry from many a pulpit that pain was God-given, and hence, on moral grounds, to be clung to rather than renounced. But the outcry of the antediluvians of both hospital and pulpit quickly received its quietus, for soon it was clear that the patient, who did not suffer the shock of pain during an operation, rallied better than the one who did so suffer, while all humanity outside the pulpit cried shame to the spirit that would doom mankind to suffer needless agony. And so within a few months after those initial operations at Danielsville, Ga., by C. W. Long in 1842 and at the Boston Hospital in 1846 ether had made good its conquest through the civilized world.

Imagine the state of mind for our medical brethren at the beginning of this century, in the year 1800. Imagine the question put to them, "What will be the tendencies of medicine in the nineteenth century?" Ignorant as they were as to the nature and cause of infectious diseases, could they possibly have foretold the coming era of revelation and evolution that medical science would, in the century at whose threshold they stood, explain the causes of diphtheria, typhoid fever, tuberculosis, hydrophobia, tetanus, cholera, syphilis, variola and even discover the means of curing many of them? Would they have believed it possible that, by means of a new manifestation of energy, the interior of opaque bodies could be made visible to the human eye? One had only to look into a tube containing a screen of certain composition, and direct it toward a peculiar electrical apparatus, to acquire clairvoyant vision, more wonderful than the discredited second-sight of a medium, that the bones of the human frame would stand revealed in their weird simplicity, as if the living, palpitating, flesh about them were but the shadowy substance of a ghost. And yet the standard of the medical mind at the beginning of the century was high, and looked forward into the coming century with high expectations.

Speaking of our latter-day preventive inoculation, remember that the anno 1800 physician had seen Jenner vaccinate successfully for smallpox in 1796. And speaking of our latter-day *x*-ray photography, had not Thomas Wedgwood and Humphry Davy in 1801 proven that it was possible to secure the imprint of a translucent body upon a chemically-prepared plate by exposure to sunlight? In this way translucent pictures were copied and skeletal imprints were secured of living insects and plants which bore a striking resemblance to the shadowgraphs of more opaque objects which we secure by means of the new photography of to-day.

This makes it conceivable that the imprints of Humphry Davy and Thomas Wedgewood were in reality modified skiagraphs or shadowgraphs. Already the suggestion has been thrown out that rays in every particular analogous to the Roentgen or *x*-rays are contained in sunlight, together with an abundance of rays of other qualities.

In the *Philadelphia Medical Journal,* Vol. IV, p. 655, Valdemar Bie of Copenhagen brings together the facts attributing to the sun's rays certain indubitable bactericidal properties. The successful treatment of lupus vulgaris is there illustrated under the influence of sun rays. What will be the development of the *x*-ray method for diagnostic purposes in the coming century? As yet we can use it with exactness concerning conditions of bones. The states of the heart and kidneys can occasionally be ascertained with limited exactness. Renal calculi are in some cases capable of localization, but the condition of the other tissues, particularly muscles, blood vessels, nerves, cannot be recognized by this method. One of the first additions to our knowledge which the future will bring to us is that we will be able to recognize conditions of the blood vessels, or at least their location, by means of the *x*-rays. In his original report, Roentgen stated that the new rays named after him were not only cut off by metals, but by the salts of metals, not only by iron and zinc, but by chloride of iron and sulphate of zinc. As the blood vessels contain normally a compound of iron, it is reasonable to presume that they should cast a shadow upon the fluoroscope. The reasons why these shadows are not visible to our eye are to be sought either in the power of the *x*-ray apparatus, or in the amount of iron in the blood-vessel itself, which may be insufficient to cut off the rays. The next step in this investigation will be either to increase the penetrating power of the ray, or to increase the amount of iron in the blood vessels, at first perhaps only in localized areas, those concerning which we desire information. A recent examination of photographic plates gained by the new photography after very long exposure and careful and special development has led me to suspect that the large blood vessels are already photographed on most of our plates, but are invisible to us from lack of proper development. I mean in this case the chemical development of the plate by means of reagents. So the next advance which we may look forward to is the recognition of the condition of the blood vessels in the plates gained from *x*-ray photography.[3] Many of the blood vessels are invisible in photographs of this character, because they are concealed by overlying bones. I do not attempt to cast the horoscope for the new photography. My object at present is reminiscent, not prophetic. It is of interest to recall what knowledge of the sciences men had in the days when Wedgwood and Davy, a hundred years ago, photographed with the sun's rays through organic bodies. In order to inquire what is the mental horizon of a person standing at the threshold of our own century, we must be reminded of the chief scientific legacies of that century to its successor, in order to become more accurately aware of the tendencies of our own mind. What will the future bring in the line of treatment and prophylaxis? There will be no doubt State care of congenital invalids of many kinds, state control of the marriage of individuals affected with diseases of which it can be established that they are propagated by inheritance.

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[3] The author's prediction that we would be able to recognize blood vessels in *x*-ray plates came true ten years afterwards.—Ed.

New diseases, hitherto unknown, will inevitably originate from the new industries using tremendous potential energies in electricity, magnetism, light, heat, and water-power. New diseases will originate from new industries utilizing chemicals that exert a detrimental influence upon the tissues of the body. New neuroses will originate from the widespread use of electricity, and the almost unavoidable increase of mental pressure, work, responsibility, and from the accompanying neglect of hygienic and dietetic rules. Medical men must raise their voices against the crowding together of human beings in gigantic cities, which must lead to self-destruction. Man is a pastoral animal. The normal human life, like that of the patriarchs, is existence in the country under agricultural occupations; we expect State or government laboratories to study the physiological and pathological effects of food, not only with regard to heat-producing, force-producing and tissue-building effect, but with regard to the influence of psychic functions on the production of possible pathological conditions.

The architecture of private dwellings will provide a special sick-room in each house, similar to the aseptic rooms in first-class hospitals; subdivision of labor among practitioners of medicine; necessity for a more detailed practice; fewer cases, but much higher fees—these will be unavoidable because of the time-consuming analytical, and microtechnical work that medical cases require, and which can only be executed in clinical, chemical laboratories.

The enormous and alarming increase of malignant diseases, brings forward the only treatment which has hitherto been of any practical value for such cases, that is, the treatment by surgical operation. I did not intend to consider the prospects of operative surgery in my present paper, but as I have had considerable experience with the results of operative treatment of gastric malignant tumors, I may state my views briefly concerning operative treatment of such diseases of the stomach. In my opinion, operative surgery of gastric diseases has almost reached the limit of its development. It stands arrested in front of the natural borders of internal medicine. These are the fundamental pillars of pathology. I am impressed with what appears to be only symptomatic improvement of gastric surgery, for 99 per cent. of all cases of gastric carcinoma that come to operation, die of a return of the disease if they survive the immediate results of the operation. The truth must impress itself forcibly upon medical men that surgery cannot be the treatment of the future for carcinoma. In all countries where there are universities and hospitals, medical philosophers, histologists, bacteriologists, and chemists are working toward the cause and nature of malignant neoplasms, and the first half of the present century will see the nature of these growths explained, just as tubercule, tetanus, and diphtheria have been explained to us, and then will come a logical and effective treatment, based upon a knowledge of the truth concerning this dreadful disease—a treatment under which the malignant growth will melt away, let us hope, like the pseudo-membrane melts away under the influence of the diphtheria antitoxin.

Will we ever be able to understand and successfully treat all diseases that human flesh is subject to? It is doubtful, for in the very time that we are engaged in solving the problem of the most prominent diseases of one period our new habits of life, new industries, the crowding together of human beings, exhaustion of certain food supplies are originating other diseases, and for all that can be told, the end of the next century will confront the medical man of that day with abnormalities baffling to him and beyond our present comprehension. Thus again, renewing his missionary work to heal and to cure, his motto will be *Nec aspera terrent.*

IX

THEODOR BILLROTH, MUSICAL AND SURGICAL PHILOSOPHER

A BIOGRAPHY AND A REVIEW OF HIS WORK ON PSYCHO-ANATOMIC-PHYSIOLOGICAL APHORISMS ON MUSIC [1]

IN the following paper I have attempted a sketch of the life and work of a man who represents one of the most scholarly, versatile, and inspiring characters in the history of German medicine; an investigator who accomplished and perfected achievements of enduring excellence in two arts, widely divergent—Music and Surgery; or, to state it in broader terms, in Biology and Medicine as well as in the Psychology of Music. For facts here narrated I have drawn largely from the various obituary notices and necrologies in German journals, and, for the second portion, from the introductory chapters to Billroth's book entitled "Wer ist musikalisch?" or "Psycho-Physiologische Aphorismen über die Musik."

BIOGRAPHY

Christian Albert Theodor Billroth was born on the 26th day of April, 1829, in Bergen, on the beautiful island of Rügen. Here, in the midst of a happy family, and subjected to various intellectual influences, he spent his youth. After leaving the gymnasium he studied at the universities of Greifswald, Göttingen, and Berlin, and in 1852 received the degree of Doctor of Medicine at the last-named institution. His graduating thesis on this occasion was entitled "De natura et causa pulmonum affectionis, quæ nervo utroque vago dissecto exoritur." (Concerning the nature and cause of a pulmonary affection which arises from section of the vagus). It was thought to possess unusual merit at that time.

Billroth had become so conversant with all branches of medicine, during his university studies under such men as Rudolf Wagner, Johannes Müller, Lotze, Schönheim, and von Graefe, that even after he had been persuaded by von Baum and B. Langenbeck to devote himself to surgery, he did excellent work not only within the limits of his own specialty, but also in normal and pathological histology, physiology, and general pathology.

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[1] Read before the Johns Hopkins Hospital Historical Club. *The Johns Hopkins Hospital Bulletin,* December, 1900.

To broaden his views he journeyed to Vienna and Paris, and spent some time at the universities of both places. In 1853 he received an appointment as assistant to B. Langenbeck, at the Royal Clinic of the Berlin University.

Under this great master, he very soon acquired the art of operating. His extraordinary skill in this direction, and ability to rapidly comprehend the surrounding conditions of a case, together with his decision and steadiness of character, proved to be of the greatest importance to him.

In 1856 he was already doing good work as Privat-Docent at the Berlin University, and a few years later he had come to be regarded as the most prominent of the younger surgeons of Germany, not only with respect to his operative activity, but also on account of his work as an investigator.

In 1860 he received a call as chief professor and director of the surgical clinic in the University of Zürich. This position at once afforded him that independence of action that was befitting him. Whereas in Berlin he had to attend to the diagnosis and treatment of disorders following surgical operations, and could only satisfy his innate yearning for original investigations by a historical and histological study of the rich material afforded by the clinic, he now, through a continuous study of the sick, could open up paths of his own. His most important investigations at that time dealt with septic fever and accidental wound infections. These studies which he afterwards continued in conjunction with his friend Weber, and later on at Vienna, effected a complete revolution of thought with respect to the causes and nature of these conditions.

His "General Surgical Pathology and Therapeutics," the first edition of which appeared in 1863, showed him to be an accomplished teacher, who had illuminated with new ideas the entire field of surgical pathology and therapeutics. The work defined sharply the limits of knowledge on the subject and incited many others to independent critical study. Written in a most attractive and fluent style, it soon displaced all contemporaneous works and appeared, at short intervals, in many editions; even to the present day its attractiveness is undiminished. The publication of the ninth and succeeding editions was entrusted by Billroth to his pupil, A. von Winiwarter.

The best evidence of the great success of this work is the fact that it has been translated into the French, English, Italian, Spanish, Hungarian, Polish, Russian, Servian, Croatian, and Japanese languages.

Billroth contributed, moreover, a number of important articles relating to various branches of surgery, to the Archives of Clinical Surgery, the editorship of which he had shared with Gurlt ever since its foundation in 1860 by B. Langenbeck. He also stimulated his Zürich pupils to many investigations of importance.

When at the death of Schuh, the second chair for surgery became vacant, Billroth received a call to Vienna from the Medical Faculty, upon the determining vote of Rokitansky, Brücke, Skoda, Oppolzer, and Hebra.

At the very height of his activity, handsome in appearance, and in the fullness of his vigor, he accepted the chair, August 20, 1867. Nor was it without difficulty that he finally succeeded in organizing his clinic somewhat according to his own notions, and the most necessary buildings and hygienic arrangements, as well as other indispensable improvements, were accorded to him only step by step. So great indeed was the opposition, outspoken and covert, with which he had again and again to contend, that even to the last he did not succeed in obtaining, as he had long hoped he would, either an up-to-date clinic with modern equipments, or a remodeling of the entire hall, as planned by himself.

In one respect he could claim a quick and decisive victory. The enthusiasm and love of those who attended his lectures, and the reverence and admiration of his more intimate pupils were his from the beginning to the end.

A fascinating delivery, coupled with great fluency of speech, and freedom from all doctrinal tenets, attracted everyone to him. Ideas and views quite new to his pupils were developed, and pathological phenomena, as they appeared at the sick bed, were explained in the light of the latest physiological, pathological-anatomical, and experimental-pathological discoveries. The soul of science breathed life into his teachings; the love of truth, with which Billroth was completely saturated, made them real. Here in his new sphere of duty, he inaugurated that restless activity which made him a captive for his last twenty-seven years at the Vienna institution.

"Per aspera ad astra," and "Durch Klarheit zur Wahrheit," these were the standards under which he unfalteringly moved on from victory to victory.

Perhaps his greatest contribution belongs to the domain of practical surgery. Hardly had he arrived in Vienna, when he began to compile his report on the Surgical Clinic in Zürich, from 1860–1867, which was followed by one on the Vienna Surgical Clinic from 1869–1870, and finally a report on the latter clinic from 1871–1876. The value of these volumes, which could only have been collated at the expenditure of the greatest industry and through an apparently infinite capacity for work, is due not merely to the important contributions to practical surgery contained in them, but much more to the fact that Billroth for the first time presented to the world a complete account of his entire experience.

Collections of statistics on various subjects belonging to medicine and even surgery had existed ere this, but no one before Billroth had had the courage to appear before the tribunal of scientific criticism with his experience in its entirety, including, therefore, that of an unsuccessful, as well as that of a successful character. In this way he stimulated others, and caused them to do likewise. The generally accepted opinion of to-day is that this manner of utilizing the statistical method in surgery is the correct one.

The stirring events of the year 1870 carried Billroth along with them. He felt it his duty to proffer his assistance, and he did so generously, both at Weissenburg and Mannheim. His scientific contributions to the surgery of the Franco-Prussian War are to be found in the magnificent letters received from him while at the hospitals for the wounded, which form the worthiest monument of Billroth's activity as a military surgeon.

No sooner had he returned from the field hospitals than he resumed his extended investigations concerning the vegetative forms of septic cocci.

At the close of the year 1874 he was in a position to present to the wonderment of the scientific profession the results of his experience in the form of a monograph.

The wealth of observational data stored up in it, and the manifold points of view from which he attempted to master numerous problems which had presented themselves during the course of his investigations, together with those magnificent ideas which acted as the precursors of demonstrable facts, bear witness to that stupendous and ingenious spirit of investigation which impelled Billroth onward with unmerciful force, and caused him to make good use of well-nigh the last minute of his busy existence. Many and many a night he would remain till late at the microscope and study his specimens. During this period of time, as we learn from Gussenbauer (Necrolog über Billroth), Billroth worked for months at a time from 16 to 18 hours a day.

Hardly had he finished this stupendous task, when he undertook an investigation in a direction apparently remote from his immediate profession. He gathered information concerning the arrangement and character of medical studies prevalent at the universities of all countries. He took an interest in this subject as no one had done before him. The outcome of his study was a book, "How to teach and study the medical sciences." It created a sensation at the time being, particularly in Austria. It aroused much opposition on account of the plain truths which Billroth unhesitatingly and without heed of possible consequences had expressed in it.

Although many of his opinions may be now considered as too rigid, his book is still, and will remain for some time to come, a veritable treasure-house of the grandest ideas. His views have been accepted as being the most practical of those that deal with the organization of medical instruction.

Over and above this activity—so extensive as to furnish by itself ample employment to even an exceptional mind during a lifetime—he wrote a long series of essays and papers in which he clearly defines his position with respect to everything of an important nature evolved within the department of surgery and pathology during the course of many years.

His monographs on wounds and their treatment, on scrofula and tuberculosis, and on diseases of the mammary gland, contained in the joint treatise on surgery by von Pitha and Billroth, and in *Die Deutsche Chirurgie,* are graphic models of lasting value, and everywhere mark the expert clinician and surgeon.

His historical and critical studies on matters relating to the transportation of the sick and wounded, on nursing as practiced at home and in the hospital, show the lively interest he took in everything which was intimately connected with the cure of his patients.

Even during the last years of his life, when illness began to annoy him, with constantly increasing intensity, he managed to keep himself thoroughly informed on all new discoveries in the spheres of surgery and pathology, and enlarged and supplemented them out of the rich fund of his own experience.

Many an idea which he has embodied in his treatise on the "Reciprocal Influence of Living Plant and Animal Cells," published in 1890, may be expected after thorough investigation to develop into a fact of lasting utility.

At the inauguration of the Building of the Royal and Imperial Society of Physicians, Billroth made use of the occasion to summarize his entire experience on the treatment of aneurisms, and discuss the relative values of the various methods suggested.

Over and above all his very numerous publications, which show Billroth to have been one of the most productive and influential medical authors of the last century, he took upon himself an immense amount of the work involved in the editing of the *Treatise on Surgery* by Pitha-Billroth, of *Die Deutsche Chirurgie* by Billroth-Lücke, of the *Archives of Clinical Surgery* by von Langenbeck.

From Gussenbauer [2] we learn of still another treasure which Billroth had stored up in the course of decades.

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[2] Gussenbauer, loc. cit.

I have reference to his collection of illustrations of diseases of rare occurrence, executed under his direction by celebrated artists. His long-conceived plan had been to incorporate this collection in a comprehensive work on special surgical pathology. It was not his good fortune to carry out this magnificent idea. His bodily powers did not suffice towards the end to keep pace with the onward movement of his indomitable intellect.

And what did not Billroth accomplish as an operator! Unequaled on the technical side of his art, and urged on by the noblest sympathy with the sick to help where others had failed to do so, he strove to extend the boundaries of practical surgery, and succeeded as no one before him had done.

Experimental studies on animals were first carried out, and not until the results of these, supported by all existing knowledge of the human body made the final outcome of the well-conceived operation promise more than a probability of success, did he proceed to the rescue of the otherwise doomed. His resection of the esophagus was followed by his extirpation of the larynx; these led to resections of the stomach and intestines and finally to extended innovations in the field of intestinal surgery, the limits of which have not yet been reached. To Billroth belongs the honor of having performed the first successful extirpation of the larynx, as well as the first successful resection of the stomach.

As a teacher, Billroth stands preëminent, for while training his numerous pupils in the ways of thinking, which his experience had taught him to be the best, he constantly incited them to undertakings of their own, and was ever ready to place at their disposal all the wealth of his own far-reaching knowledge. The force of his own brilliant example, and a certain subtle ability which he possessed of discerning the individual traits of each one, rendered his influence over his pupils all-powerful. Anyone who had within him a genuine striving after progress was welcomed by Billroth.

In addition he seemed to have the rare power of forming a just estimate of differences of character. So unprejudiced and loving was his heart, so comprehensive his intellect, that he was capable of attracting to himself even those of unlike aims and wishes. Billroth's enthusiasm and energy of mind live again and flourish in his pupils.

And finally, what was he not as a man! Although coming from a family in which sickness had found an abode, nature nevertheless had amply endowed him with comeliness and strength of body and at the same time had equipped him so richly with sterling qualities of mind and heart, such as are not likely to be soon found united again in one individual. By means of strict, even unrelenting, education of self, he developed into that ideal model as he appeared to his pupils even to the end of his days.

He was a homogeneous whole, not one-sided, as is wont to be the case with so many professional men. His excellent musical capabilities were used to advantage and developed to such an extent that indeed, up to the very end, he obtained solace from this source whenever the grave and onerous duties of the surgeon threatened to oppress him. In comprehension and critical estimation of the masterpieces of the art, he could hold his own with the most famous artists and professional critics.

Wherever Billroth happened to be, he kept himself in close touch with men of prominence in their various callings; in this way he preserved himself from a narrow view of life and its phenomena, and ever maintained an active interest in everything that concerns the beautiful, true, and good.

Nor was Billroth averse to the ordinary pleasures of life, considering them a necessary means of diversion for the mind. The decorations of his home, that magnificent Villa Billroth on one of the most beautiful spots on the Wolfgang Lake, were prompted by his own taste for the beautiful, and a desire to make his own, as well as the lives of those around him, as agreeable as possible.

Billroth may be certainly said to have been singularly rich in true happiness and honors of all kinds. Happily married in 1858 to Christel Michaelis, his domestic life was not without its trials and tribulations. His son died in Zürich, his daughter in Vienna. To his three remaining daughters he transferred all his fatherly love and care.

Among his personal friends and intimates were numbered members of many prominent families both in and outside of Vienna. To his numerous pupils whom he constantly assisted with his fatherly advice, and to whom, when in need, he ofttimes lent a helping hand, he proffered the hospitality of his house. And here, within the circle of his family, Billroth disclosed the real man, possessed of all the truly magnificent qualities of mind and heart.

These very qualities, combined with a refined artistic instinct, chained him to Vienna, the city of the musical muse. Here he very soon felt at home; he became an Austrian and a warm patriot. His Austrian patriotism compelled him, a man from the North, twice to decline honorable calls to prominent universities of United Germany. In the year 1872 the University of Strassburg, which, during its reorganization, was richly endowed, offered him a professorship. After the departure of Jünkens from Berlin, he received a call to the Charité Hospital, but again declined. And even when Langenbeck himself, on his retirement from the most important surgical chair of the German Empire, declared Billroth to be his most worthy successor, the latter could not decide to return to his fatherland. He had found his home in Austria; he remained at the Vienna University. His only immediate reward was the grateful cheering of the Vienna students.

Proofs of his patriotism still remained in his creations of a benevolent character in Vienna. The Rudolfiner Association, with its grand hospital in Oberdöbling, and the building of the Royal and Imperial Society of Physicians in Vienna, are his works.

He was the recipient of a large number of decorations and distinctions from his own and foreign governments, as well as from many learned societies. Among the former he possessed the Prussian Iron Cross—conferred for display of extraordinary bravery while in action with the enemy. As regards American Societies, he was a member of the Academia Chirurgia, of Philadelphia, and the Pathological Society, of Saint Louis.

Such was Billroth! This man won the hearts of all who knew him. He was honored and revered in numerous foreign countries, wherever the fame of his deeds had penetrated. Nor is he forgotten after death. The imperial capital assigned a grave of honor to him, and countless was the human throng which followed his hearse in deep sorrow for the man and for the scientist. His works and his fame among men still live and will live after him.

The circumstances under which Billroth's book "Wer ist Musikalisch" came to be written are stated in the following introduction written by his musical friend Ed. Hanslick:

Billroth at an early age gave evidence of love and talent for music, which, as he himself always insisted, was hereditary. His grandmother, Frau Wilckens, née Willich, filled an engagement at the Berlin Opera House as soprano, alongside of the then famous tenor Eunicke—the first "Florestan" of Berlin—who was also the grandfather of Billroth's wife. "In this way," so wrote Billroth to Hanslick, "I am truly a child of music and the stage." When a boy he expressed the earnest wish to devote himself entirely to music, but his intention was thwarted by the determined opposition of his mother, for which, however, later on he was deeply grateful. Not that this vigorously minded and cultured woman desired to suppress a love for music in her children; quite the contrary. There is still a letter written by her from Bergen, in which she reminds Theodor to give punctually regular piano instruction to his younger brother; both brothers were then studying in Berlin. Music remained Billroth's true and beloved companion until the end of his days. During the years of his professorship in Zürich, a string quartet met at regular intervals at his house, and on these occasions he himself played either the first violin or second violin parts. He also wrote musical criticisms for the *Züriche Zeitung,* and exerted an inspiring influence on the musical life, then at a somewhat low ebb, in that city. Being not only a good performer on the violin and piano, but also a refined connoisseur and earnest thinker in musical matters, he felt during the last years of his life an ardent desire to arrange, clearly outline, and write down his views concerning music. In this matter Hanslick gladly urged him on, inasmuch as he, through his two-fold position of expert musician and trained physiologist, appeared to be singularly fitted to throw light on that line of demarcation at which musical effects and the experience of our nervous system meet.

Billroth's work progressed bit by bit, and was often interrupted. He only succeeded in doing work on it at long intervals, during his vacations in St. Gilgen, or Abbazia. During the summer of 1888, he seems for the first time to have busied himself more constantly and coherently with his musical views. At that time he wrote from St. Gilgen:

> Who is musical? This would be a heading for you for an essay. How complicated this idea is! One man is possessed of especial susceptibility for rhythm (the most elementary rhythmical principle of the human body is the heart-beat); another is endowed with a decided talent for melody (melody cannot be separated from rhythm; the articulation of the human body and its twofold arrangement, with respect to a horizontal and a vertical axis, are a part of its fundamental formation); a third man appears to be musical owing to an outspoken technical and mechanical ability (elementary principle; the pleasure experienced in overcoming difficulties as the chief result of an intensified feeling of self-consciousness); in a fourth, music seems to be the expression of an intense exhibition of emotion displayed in dramatic expression (elementary principle: a desire to appear to be of as much importance as possible, as seen, for example, in the pheasant when he strikes a wheel before the female); in another, musical talent seems to be the result of a well-developed faculty for the retention of tone-combinations and rhythms; again, in another, music means a predilection for purely sensuous effects on the auditory sense, etc. To me it is all chaos. It pleases me to deceive myself (that is no idle dream of mine), that sometime I may be able to write something sensible concerning matters outside of my profession; and that it is only the short extent of my vacations that prevents me from developing within me some mature thoughts. I like to think this, I say, although I am well aware that it is a delusion. Similarly, in my chosen profession, I have only given the incentive, have acted only as pioneer and blaster; as soon, however, as the ground was cleared, the road made, the blasting done, I gladly let others do the sowing and harvest the results.

In a letter of September, 1890, Billroth again refers to his undertaking:

> Your kind encouragement has finally begun to bring about a realization of ideas which have occupied my mind for years. I am now writing an essay: *Anatomical-physiological aphorisms about music.* Who is musical? It is to

form one of a number of essays (which I think of publishing together under the title "Reflections of an Abirsee-promenader") providing that they turn out to my satisfaction. At present matters are beginning to swell like Faust's poodle behind the stove, and I am afraid that the book will represent only the lucubrations of a vagrant pedant. It supplies me, however, with a fund of pleasure; whether it will do this for others is, of course, a question of another kind.

One year later, in September, 1891, Billroth wrote from St. Gilgen:

About a year ago, here in Abbazia, I wrote a fairly large manuscript—"Aphorisms on the anatomy and psycho-physiology of musical matters." It remained untouched for a whole year; I have now taken it up again. The first chapter, "Concerning rhythm as one of the most important elementary principles of music, and one most intimately connected with the human organism," was fairly satisfactory to my critical judgment, so that I made a fair copy of it. The beginning of the second chapter also, "Concerning the relation of pitch to the human organism," seemed good. But after that consideration other kinds arose—speech, song, vowels, harmonics. I began to question the accuracy of some of my statements. This led me to read Helmholtz, which I did at first only cursorily, but after taking up Landois' book on "Voices of Animals," I came back again to the former. I used the fourth edition, which contains much new matter as compared with the first which I had studied years ago. A consideration of vowels, harmonics, speech, etc., brought me into the sphere of psychology—concerning the evolution of speech and song in the child—and in this way to Preyer's highly important, somewhat voluminous, but nevertheless interesting treatise. I really do not intend to write anything of a learned nature, but wish to clear away false notions about such matters from the minds of amateurs. Is the process of composing intimately related to the phenomena of hallucinations or illusions? This forced me to resort to a book on hallucinations, sleep, dreams, etc.; indeed, it led me to a study of mental diseases. I do not care to write a line which might convey the least uncertainty about such matters, nor to use a single word in an erroneous or equivocal manner. . . . Now I have certainly read so much of real excellence, that, for the present at least, I am too timid to look at my manuscript. You can see that I am not destined to become a popular writer. I readily pass over inaccuracies on the part of others so long as the authors interest me; but every statement of my own I weigh with intense seriousness. Thus my contemplated essay can hardly expect to ever become a realization. No matter! If such a work is ever needed, it will be written by some one else. What is occupying the mind of one person at any given time is receiving the attention of hundreds of others.

At last, November 21, 1891, Billroth sent Hanslick a part of his essay together with the following letter:

Although you have often encouraged me to commit to writing my various views on music, expressed in conversation at one time or another, it is with some feeling of anxiety that I send the enclosed manuscript. If you will read it at your convenience, you will please me very much. I have written down these ideas partly because I wished to be better able to form an opinion of their real meaning, and partly from the mere pleasure of producing something. But I do not intend, at least for a long time to come, to hand them over to publication. While writing I have kept before my mind the needs of a fairly educated public, but have earnestly endeavored to avoid medical or philosophical cant. In the first two chapters sent you, I think that everything has been gone over which might be designated as "Musical Physiology," or "The Physiology of Things Musical." The remainder is based on conventions. But inasmuch as convention depends on the psychology of the human being with respect to his position as a social or gregarious animal (ζωον πολιτικον), and on psycho-physiology—because of the inseparabilty of the soul and body—the following portions of this work may be considered appropriate so far as regards the main title.

It is interesting to observe in a man of Billroth's reputation that shy modesty, in relation to his work, displaying—in various letters to Hanslick—such a diffidence as regards his knowledge and style. It must have been fear lest he should write too learnedly or too pedantically that induced him at first to prefix to a number of chapters musically humorous headings, viz.: I. Marcia (like a march); II. Allegro serioso, ma non troppo (at a not too serious gait); III. Grave (with gravity); IV. Thema con variazioni (theme and variations); V. Serenata (serenade); VI. Intermezzo (interlude); VII. Finale tempo giusto (conclusion at a good gait). These headings, present in his first sketch, he afterwards dropped. The title "Who is musical?" was originally prefixed only to the last chapter. It was finally, but perhaps less appropriately on account of its limited meaning, selected as a title for the entire work.

Shortly after Billroth's death at Abbazia, on February 6, 1894, his son-in-law, Dr. Otto Gottlieb brought to Hanslick the somewhat voluminous manuscript on the cover of which was written in bold script the following:

"This manuscript is to be handed to my dear friend Ed. Hanslick, to be disposed of as he deems fit."

Abbazia, February 3, 1894.
TH. BILLROTH."

The first three chapters appeared in "Die Deutsche Rundschau"; the complete work was not published until the fall of 1895. In a prefatory note Hanslick says: "In his manuscript Billroth designated the first two chapters as finished. The later ones demanded a more careful revision on the part of the editor because of the many illegible corrections, supplementary remarks, and inserted additions. I have tried scrupulously to avoid interfering with Billroth's mode of thought, and style, and have therefore confined myself to making only insignificant changes in some expressions and in removing many needless repetitions.

"The last chapters, although not so polished as the first ones, characterized by Billroth as 'finished,' are not less important and rich in content; in fact, they possess for my emotional sense a somewhat unique, and indeed a greater charm, owing to their informal freshness, which is suggestive of a personal discourse."

Themes from "Wer ist Musikalisch?" or Psycho-physiological Aphorisms on Music

Billroth's work is an attempt to fill a gap that exists in the literature of acoustics, and of esthetics. Helmholtz, in his "Sensations of Tone," attempted to connect the boundaries of two sciences—physical and physiological acoustics on the one side and esthetics on the other—which, although drawn toward each other by many natural affinities, had before his time remained practically distinct. Works on physical and physiological acoustics are by no means rare, and of those dealing with esthetics, pure and simple, there is an abundance. Ed. Hanslick, has ably covered the ground of musical esthetics in his book "On the Beautiful in Music" (*Ueber das musikalisch Schöne*). Billroth makes a modest and largely successful effort to construct a road through the almost unknown territory of the purely psychological in music. The book, as edited by Ed. Hanslick, is called "Wer ist musikalisch?", the title being that given by Billroth to the last chapter in the book, and designated by him as a sketch. The selection does not seem to me a very happy one, inasmuch as the title by no means represents the matters treated of in the volume.

The book contains some 250 pages, and is divided into seven chapters. The first treats of rhythm, an essential element in music, which is intimately associated with our organism. Nearly all human beings and many animals are agreeably moved by recurring rhythmical beats. Human beings feel induced to execute similar rhythmic movements with their body, particularly with the head and hands. This may be explained on the ground that the movement felt as rhythm acts upon certain properties of our body, which are known to us from experience and to which we are accustomed by practice and exercise, so that the perception of a rhythm may be transformed without reflection into a rhythmical movement.

Rhythmic movements are among the most important properties of our body, and are necessary to life. Thus for instance, we have rhythmic movements of respiration, rhythmic movements of the heart, and the rhythm which we are capable of imparting to our voluntary muscular movements. It is probable that all muscular movements of the body, conscious or unconscious, are brought about by a summation of numerous infinitesimal and imperceptible rhythms.

The psychic contagiousness or power to cause imitation which rhythm exercises upon human beings is well known. The psycho-physiological law of sympathetic movements and sympathetic sensations (*Mitbewegung und Mitempfindung*) has such a wide distribution in the animal world, and is of such eminent significance for our entire social and ethical culture that it gives the impression of a fundamental natural law applying to all organized matter. The influence exercised by human beings upon one another is based upon it. When Billroth speaks of sympathetic movements and sympathetic sensations as the results of a transference of psychic and physical rhythm, he has mainly in view the transference of a movement or sensation, through observation by the senses, from one individual to another. Speaking purely physiologically, sympathetic movement is often used to describe the transference of excitation from one motor nerve to another, and sympathetic sensation would mean the transference from one sensory nerve to another. This latter conception has little bearing upon Billroth's consideration of rhythm.

It would lead me too far to enter upon a consideration of the powerful social consequences of sympathetic movement and sympathetic sensation, in which we must, according to Billroth, recognize one of the strongest foundations of ethics. He next considers the tendency to appreciate rhythmic movements as it is continuously impressed upon us by observation of our fellow creatures, men and animals. In an entertaining manner the rhythms of the movements of a horse are described. The walk is compared to the Andante (2/4), the trot to the Allegro (2/4), the gallop to the Allegro con brio (3/8), with a 1/16 up-beat (*Auftakt*), the canter (Presto 2/4). The description of the rhythms in the walk of hens is almost comical, while the recognition of those hidden in the call of the cuckoo, of the quail, and of the cock, shows an analytic and careful observer.

Billroth asserts that a fundamental condition of music, viz.: the more or less conscious ability to receive and appreciate rhythmic movements, must be innate in man and many animals. This question is intimately associated with the problem whether the aspects of time and space are also innate with us, *a priori,* in the sense of Kant. Billroth's opinion is that these capacities are born in us, but they can only become conceptions by experience. The perception of rhythmic movements occurs with varying intensity in different individuals, and it may or may not lead to sympathetic movements. The event will depend upon the power of observation and experience on the one hand, and, on the other, upon the greater or lesser irritability of the nervous organs, and also upon the degree to which excitation can be transferred from the tracts of special sense to the motor paths. The quicker or slower propagation of nerve excitation, is a quality that may be innate, or may be developed by custom. Everyone has noticed the difference between vivacious and phlegmatic temperaments. Most European nations, particularly the Romance, South-Germanic, and South-Slavic tribes, are in general of a quick, vivacious disposition, and love this temperament in other men; whereas the inhabitants of the North, as well as the Oriental nations, have a quiet temperament, and prefer this also in other people. Of course there are individual exceptions. Religion and custom in certain great tribes have in a few instances idolized the acquisition of the greatest possible indifference in point of temperament, including not only the suppression of every individual emotion, but the encouragement of the greatest possible independence from surrounding stimulations. Similar principles, according to Billroth, constitute the highest ideal of human perfection in the education of the English nation. To the latter the freest and the most perfect individual is the one who can appear independent of all external influences, like a rock in a stormy sea; to be moved or to show emotion under the influence of environment means to become ridiculous. Thus does Billroth conceive this to be one of the forces of English custom. Inquisitiveness and the pleasure caused by changes in the external environments, are not totally excluded, only there must be no expression of this sensation. This is an Oriental trait. Whilst the powerful significance of such innate and acquired peculiarities of character are not to be underestimated for practical life; nevertheless, so far as the appreciation of music is concerned, such men are wanting in the most fundamental requisite. Individuals who have no perception of joy or pleasure resulting from sympathetic sensations or sympathetic movements, and in whom the expression of any possible joy has been gradually deadened by education, must of necessity become relatively unmusical.

It is a commonly accepted view among the laity that the feeling for rhythm is innate in every man. Billroth disproves this idea by a number of statistics from Austrian and Hungarian regiments, indicating that in some regiments there are recruits, amounting to 2 per cent. of the total number, who never learn to march rhythmically. These men are not permitted to appear in the parades, or are transferred to cavalry regiments. Besides these there are always about 20 to 30 per cent. of recruits coming from Roumania and Bosnia, the mountainous countries, who remain awkward; sometimes they imagine they march well, and are surprised when they are criticized. In a Polish regiment there were to be found soldiers who had served 10 or 12 years, but who nevertheless could not march rhythmically. Some of them would march passably well when they were in the ranks, where they could watch the feet of other soldiers and be guided by their eye, but they could not march by themselves. It is therefore correct to assume, that the rhythmical feeling is not innate in all human beings, and that there are some who cannot even be taught to appreciate rhythm. They must be absolutely unmusical, for the ability to apprehend the rhythmic organization of tones into a melody is the fundamental and first condition for the comprehension of music. The popularity as well as the longevity of certain music, depends far more upon the rhythmic, than upon the melodic property. All the songs of the people are constructed on very simple rhythms. The rhythm of a melody, in order to be popular, must not only be short and simple, but must frequently be repeated in the same manner.

From popular songs the author turns to a consideration of popular dances; and in an interesting paragraph gives suggestive historical facts, tracing back the dances among the mountain inhabitants of Austria to those of ancient Greece. This section is of unusual interest, but can only be touched in a cursory manner. The dance is one of the oldest arts spoken of in history. Simonides calls it mute poetry. It would be interesting to know how David danced before the Ark of the Covenant; what was the step of Louis XIV in the minuet; what was the dance that Aspasia taught Socrates, and that of the guests at the banquet of Xenophon.

But to go back again, rhythm is asserted to be more important for the longevity of a composition than melody, because it is the more elementary of the two, and is more intimately associated with certain fundamental properties of our body. Melody is always more or less dependent upon conventionality, upon habit, and fashion. As illustrations we may take the compositions of Händel, Marcello, Bach, and Scarlatti. The melodies of these masters may appear strange to us, and their harmonies at times uninteresting, even ugly and bizarre. What has perpetuated these compositions, according to Billroth, are the incisive energy and the abundance of wonderful rhythms. Many great masters in music have lived after them, but they could not create anything more powerful and impressive than the rhythms of these composers. It seems that the creation of magnificent rhythms in music has been exhausted—a condition analogous to that existing in drawing and painting, and in the disposition of space in architecture. Both seem to be exhausted, since the productions of the masters of the Renaissance period. Billroth does not consider that R. Wagner's compositions have become extensively appreciated, and what popularity this master has obtained is due partly to the exquisite rhythm in the beautifully-formed Wagnerian Motives. The endurance of Meyerbeer's four great operas is attributed principally to the rhythmic contents of his music. When the sensation for rhythm has entered consciousness as a pleasant perception, and when it has caused sympathetic sensations and motions, it becomes incorporated in memory, and we begin to combine and play with these inner musical conceptions. It is an interesting, fascinating, and esthetic playing of our fantasy. By playing Billroth does not mean toying or trifling, but rather the association and combination of musical memories and conceptions. We could not say, if we are conscious of such melodic and rhythmic cerebration, that we are playing, but we would have to say as the Germans

"Es spielt in uns" (It plays in us).

This playing may be a partly unconscious and a partly conscious and active process with us. Thus a philologist or a poet plays with language conceptions; a mathematician plays with quantities or geometric forms; a painter plays with visual forms and colors, and so forth. Why an individual plays more particularly with one form of conceptions, rather than with others, and why in one person one group of conceptions are retained by the memory, and another group lost, depends in the first place upon his organization, and the inherited peculiarities of his character. In every man will be fixed those perceptions of the senses, for which he has inherited the greatest receptiveness and ability for comprehension. This play of conceptions leads to various results and actions which will vary according to the talent and character of the individual, and according to the characteristics of the human beings into whose society he is born at a certain time.

The acuteness and distinctness of musical perception depends upon three properties of the mind, which may be partly inherited and partly acquired. These are the following:

I. *Memory for Rhythmic Forms.*—An individual who does not possess this ability, or who cannot acquire it; who cannot recognize the repeating individual rhythmic structures in a continuous composition; who, for instance, has forgotten the beginning of a musical sentence at the end of the same, is not capable of the perception of genuine pleasure at the hearing of complicated rhythmic combinations.

II. *Pleasure at the Change of Conceptions.*—This capacity, which is peculiar to man, and in animals, possibly occurs only in the highly intelligent ones which are in constant association with man, is inactive in indolent human beings.

III. *Pleasure at the Increase of Conceptions, and the Augmentation of Memory. Pleasure at Overcoming Difficulties.*—There can be no doubt about the elementary significance of rhythms and the arrangement of modern music into measures, when one reflects that the attractive and charming coöperation of several instruments or several voices is inconceivable without rhythm and measured arrangement. The effect of various rhythms in causing certain sensations and conceptions, is of interest. A rhythm that we hear, or a rhythmic motion which we see, may cause a direct sympathetic movement in us. For example, we may beat the time with the hand when we hear music, or we may feel induced to imitate a rhythmic movement which we see others execute; but the same sources of excitation may cause other conceptions by association, such for instance, as may have been deposited in our memory on former occasions by or simultaneously with that particular rhythm. For instance, dance or funereal music may remind us of a joyful dance or of a funeral, respectively. There may even be recalled to memory conceptions of certain persons, whom we met at the dance or at the funeral. All at once a definite landscape, a hall, a room, or the inside of a church with all its detail, may appear before our mind's eye, and we may be moved by the identical sensation that filled us when we heard similar music and the same rhythm for the first time.

Rhythm may effect an accelerating and inhibiting influence upon the psychic movement, the frame of mind in which we are momentarily. This leads the author to the question whether rhythmic movements alone—simply as such—are capable of expressing or communicating anything. This of course is associated with the question whether music without words can express ideas. That this is possible to a high degree is evident, and an analogy is found in the so-called language by gestures. Rhythms may be perceived simultaneously by three senses. They may be heard, seen, and felt in our muscles. Now, as the influence on consciousness may be exerted from three senses at the same time, it is evident that the major part of our nervous system is occupied in the process—a fact which readily explains the marked effect exerted upon the entire organism. The relation of rhythm to several senses simultaneously has been described by Aristides.[3]

Billroth next compares rhythm in motion with rhythm at rest—*symmetry*. Moving rhythm is an arrangement or disposition of pulses at intervals of time, while symmetry is an arrangement and organization in space. This close connection, with thoughts on rhythm and harmony, as expressed in painting, sculpture, and architecture, explains the author's inquiry as to whether the perceptions of rhythm and those of time and space are innate (in the sense of Kant).

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[3] Westphal, "Griechische Rhythmik," page 47.

In the third chapter, an abstract of which follows, Billroth deals with the development of the musical art with the science of music.

The term Physiology (Natural Philosophy, Physiology of Man, the science of dealing with the functions of the human organism), in recent times, has often been employed in popular works in connection with matters that refer far more to culture than to nature;[4] whence has arisen much confusion concerning the character of the "original" or "natural," and its relation to the acquired or artificial. On this account I consider it necessary to explain here that we, with what has been previously said (in the first and second chapters), are at the end of our purely physiological considerations, in so far as they are related to music, indeed, that we have already crossed its boundaries here and there and touched upon the domain that has been acquired by purely psychological work.

The soul (Psyche), according to the views of the present time, is not to be separated from the body, and the phenomena in its domain, therefore, form a very essential part of physiology. Nevertheless these phenomena appear to most people as distinct, separated from the body. This I find very natural! The entire development of the civilization of mankind, its power, though limited, over many forces of nature, the comparative comfort of the ruling classes in the social divisions created by themselves, have not developed from the purely physiological, physical struggle for existence, but are victories which, especially in historic times, have been won principally with psychic weapons that are continually being forged in the smithy of the human brain. We cannot, therefore, be astonished that the psychic appears to the man who has not received an education in natural science, as something distinct, higher, separated from and controlling the brute force of the body.

And yet the soul is more dependent upon the body than the body upon the soul. What we call perception, thought, conception, consciousness, can not come into existence in the absence of a brain. To be sure, there exist, not only in the lower animals but also in man, unconscious bodily sensations and bodily motions (reflex motions) which take place without the so-called psychic phenomena, as in a well-constructed machine.

It seems not only inadvisable, but, in the present condition of society even absolutely harmful that in popular works so much stress is laid upon the dependence of the soul, because the constraint of the will is naturally connected with it. To such a degree does the entire structure of human society rest on the social dogma of the freedom of the will and on the consequent responsibility of the individual for his actions, that the beautiful edifice would be shaken to its foundations if everybody were to maintain that all his thoughts and actions were nothing but the result of phenomena in his body, conditioned by his individual bodily constitution, his inborn psycho-physical character, for which he could be made responsible as little as for his existence. Indeed, as one of its great aims, modern investigation has set itself the task of explaining the development of all ethics, as well as the rise of religions, as purely psycho-physic necessities, and as the natural consequences of the structure and composition of the human organism. But even if we were beyond the very first beginnings on this path, I should consider it dangerous, as yet, to practical ethics, to raise the veil from these mysteries of our temple of sciences before the populace.

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[4] Physiology of the Social Body (Schäffle), of Law (Stricker), of Love, of Hate, of Marriage, etc. (Balzac, Stendhal, Mantegazza, Bourget).

Without going further into these and similar considerations, let it suffice to say here that the term "psycho-physical" or "psycho-physiological," as used at present, is applied not only to the connection of the qualities of body and soul but also to the treatment of psychology by means of physiological methods. If, therefore, that which I have still to say about the "Musical" does not belong to the domain of the purely physiological, it nevertheless is included under that of the psycho-physiological. I shall go still further and put the results, due to social forces, of certain concessions and compromises in the domain of the musical—the "conventional"—also in the domain of the psycho-physiological.

"Conventional" and "natural" are in a certain sense antitheses. But to simply identify conventional with unnatural is in itself wrong. No creature can do anything that is not made possible for him by his natural organization; the unnatural cannot at any time be acted or even thought by man; for man cannot think anything else but what the nature and organization of his brain allow him to think. The conventional is the natural result of human striving toward conditions that bring advantages and give pleasure; it is one of the results of and at the same time one of the conditions underlying the existence and advancement of human society; it is not imposed upon society by tyrants, heroes, or gods, but imposes itself, just as law and custom, and like these, it varies in its practical consequences according to place and time. The formal development of the conventional requires, to be sure, one, sometimes more, personages; these are the typical expressions, to a certain degree the embodiment of a determination or will of society (Gesellschaftswillen) or of the people (Volkswillen) in a certain time—a determination which has not yet come to a definite expression. They become leaders, lawgivers, heroes, and to us, if the mental tendencies of a long past form of society are not well enough known to us, they may appear as arbitrary tyrants, although in reality they were the slaves of the spirit and the will of their times. The same holds true for the rise of fashions; the need of society for change arises from a similar need in the individual; attempts are made by this man or that man to take the lead; whoever finally strikes the taste of the majority becomes leader. "Fashionable" and "Conventional" are terms for the same psycho-physiological processes in the organism of human society. *In this sense the conventional also belongs to the nature of man and of human society.*

After these explanations of terms to be used later on, let us return to the "Musical."

It is justifiable to assume that the feeling for rhythm and the power of perceiving differences in tone-pitch, tone-sound, and tone-strength, as well as the capacity of differentiating these qualities in rapid change and combination are the physiological prerequisites for what we now call musical. Nevertheless, that everybody who has these qualities, is *ipso facto,* according to the customary use of language, to be called "musical" must be unconditionally denied. Does every man, unless he has been born absolutely deaf, possess the aforesaid psycho-physic qualities? This question cannot at once be answered in the affirmative. We cannot observe the inner phenomena of another man directly, but only deduce them indirectly from his physical utterances, especially from his reflex motions or his motions of accompaniment or imitation.

If we take as a standard for the rhythmic sensibility of a man his ability to correctly accompany seen or heard rhythmic movements, or to imitate them, we should occasionally, although seldom amongst civilized peoples, find persons apparently incapable (refractory) in this respect. As was pointed out above there are men whom it is impossible, or at least very difficult, to get to march or to dance in rhythm. This may be based on awkwardness. One of the most remarkable examples was Beethoven: eminently successful as he was as pianist, he is said never to have succeeded in dancing in time. It is also said of the singer Malibran that it was impossible to dance with her, because she did not keep time; and yet she was very fond of dancing. In both cases the defect could hardly have been entirely due to the lack of the feeling of rhythm; it could certainly have been obviated with some practice, unless it arose from a kind of shyness to exhibit one's self before others. This, indeed, is very difficult to remove.

Much more frequently we meet with people who find it impossible to repeat accurately a tone that has just been sung, and who claim that they do not perceive intervals of quarter tones or even half tones, when sounded together. A false note in an opera, since but few concentrate their attention entirely or largely on the music, is probably noticed by hardly a fourth of the audience; the tremolo of the singers by a still much smaller part of the hearers. A not quite accurate reproduction of a tone, so frequent, especially among beginners in vocal music, is not always a proof that the person singing incorrectly is unmusical; often it is only the result of inattentive listening and of an awkwardness in the motions of the laryngeal muscles, and can be removed as soon as the ability to perceive the impurity of the tone exists at all or can be awakened from its slumber. Singing incorrectly on the part of trained musical singers, especially on the stage, is principally due to psychic causes—excitement, fright, and also overexertion. Most people probably can distinguish different intensities in tones and whether they are produced by an oboe, a violin, or a human voice. But there are individuals, who seem to have no conscious sensation even for greater tone intervals, indeed even when one tone is relatively much higher or lower; they think they are imitating a song correctly when they reproduce its rhythm alone, and with it arbitrarily use first one tone, then another, or always the same. These persons, to be sure, are hopeless cases so far as regards a musical education; and yet they may be passionately fond of music with a kind of childish delight in rhythm and combinations of tones as such, just as other people take delight in brilliant colors without being in the least endowed with the artistic sense.

It is only since the appearance of the epoch-making book of Helmholtz that musical physiologists, psychologists, and specialists in ear diseases (Preyer, Stumpf, Politzer, Urbantschitsch, and others) in the last decade have busied themselves in determining with greater accuracy the physiological and psychological foundations of tone sensations and tone combinations and their closer relation to the music of our era. As a result of their studies very remarkable facts have been brought to light, showing on the one hand how widely the power of distinguishing tone intervals varies in different persons, and on the other hand how much may be obtained by systematic practice, even in the case of deaf-mutes (Urbantschitsch). It would lead us too far into details to treat this more fully. Suffice it to say, that the cause is to be found more rarely in corporeal defects (consequently defective or partially destroyed organs in the auditory apparatus) than in defective afferent nerve impulses and faulty attention to complicated sound impressions. Indeed, there exists a total psychic indifference toward all sound perception, especially toward all combinations of sounds, which might be termed a harmonic nihilism, a harmonic deafness. A friend of mine, who enjoys songs, and occasionally goes with his wife to a concert, is without any perception of the agreeable or disagreeable in the combination of tones. He forms the same impression on hearing a chord of three sounds that he does when he hears five neighboring tones struck at the same time. Thus, when I played for him the air "Wir winden dir den Jungfernkranz" in F sharp major on the piano, and accompanied it in F major with the left hand; he said, "That is from *Der Freischütz.*" I repeat the F accompaniment with the left hand and played the air in G major. I asked "Do you notice any difference?" He reflected a while and said: "I believe it pleased me better the first time."

To appreciate the fact that in a large concert hall there are a hundred or more auditors who stand on this same plane, and that there are many more to whom all variations up to a third, sound as one and the same tone, so that for them there are at the most only four well-defined tones in an octave, and that for these there does not exist any incorrect playing or singing at all, must certainly cause a somewhat gruesome feeling in the mind of the artists—"Love's Labor Lost." But during the thundering applause, the musically deaf probably clap more violently than the really musical. In my friend there was really a musical force present, that is to say, the memory for the rhythmic: he recognized the air as taken from the *Freischütz.* But even this memory may be wanting and yet the individual may play the piano. A young girl who had taken lessons on the piano for two years, was practicing a piece of Mozart's, and was ready to play it for her teacher. She came somewhat late and found him sitting at the piano and playing. In order to make her presence known she asked: "What are you playing there?" The teacher turned around astounded and said: "Why, that is the piece you are to play for me to-day." "Indeed!" She then played the piece faultlessly and the instruction was continued. This lady married a very musical man. Of their three sons, all very intelligent, two are absolutely unmusical, the second one is exceedingly musical. To give another example of an apparently unmusical person: A very musical couple had a son who seemed, when a small child, not to take any interest in music. When he was about eight years old, his mother would sing airs for him, which he, however, could not repeat; nor could he sing a note struck for him on the piano. His mother consequently considered him totally unmusical. When he was about twelve years old, he came now and then when his mother played the piano and said: "That is pretty, mama." As time went on he could sing and play correctly melodies which he had heard, and later became anxious to learn to play the violin. In this case there was a small degree of attention to sound-impressions during earliest childhood, together with awkwardness in reproducing with the laryngeal muscles the sound heard, which in turn gave rise to a sort of shyness, from fear of making himself ridiculous by unsuccessful attempts.

These three examples from my latest experience (I formerly gave the matter no particular attention) show how manifold the coefficients are which determine whether a man is musical or not musical.

I should prefer to call them, along with the aforesaid rhythm defects, tone-interval defects. It is to be hoped that both are the same, for otherwise the number of the non-musical would be frightfully large, especially if the only slightly musical be also added to it, those, I mean, who are born with an appreciation of rhythm and a perception of the smallest intervals but who have no memory for music (for two successive bars) and for whom music as such has so little interest that they find absolutely no enjoyment in directing their attention to the succession of tones. Nevertheless, these persons occasionally remember airs and sing them tolerably well.

"Who then is musical?" The answer to this, apparently so simple a question, is very difficult and extremely involved, because the term music applies just as well to that produced by the beating of a tambourine as for the most complicated polyphonic vocal music. Anyone who tries to answer the question must of course consider himself musical, otherwise he certainly would not make the attempt. But here there can exist self-deception, for not infrequently we hear people talking of things concerning which they know nothing and yet think they know much. I must leave it to the reader whether he holds me to be musical and to what degree he will admit my right to give a verdict.

The concept "Music" has, as said before, become very complicated. There are various degrees (I might almost say kinds) of being musical, because the science of tones is composed of different elements, of the rhythmic, the melodious, and the harmonious, and in each of these elements there is again a purely technical and a purely esthetic element. A person may have more talent, interest, and responsiveness for one than for another of these elements, just as those with talents for the pictorial arts may have more interest and talent for drawing, or for artistic compositions (lines), for coloring, or for the purely technical. The longer one ponders over the matter, the more complicated becomes that which we now call "Music," and I can only venture the bold attempt, to answer this question, after I have tried to make it clear *how that which we now call music or the science of tones arose, and how this science affects us.* Herein I am only following a scientific tendency of our age, in which we try to understand all that is about us and in us, from its origin, and which only bows down before the "inherited" or "inborn," or in other words the "marvelous" when we can get no further on in our attempts to comprehend with our sensations, observations, and thoughts, the phenomena in Nature and round about us in human society. This limit is, to be sure, reached soon enough. Nevertheless, these attempts are the sole sources of that which can create new knowledge for us, and we ought not to be discouraged at the slight progress of our work, which when compared with the whole, it must be confessed, is very small. "There seems to me to be no warrant that our understanding must necessarily conquer *everything* which may exist on earth." [5]

Every creature devotes its attention primarily to those sense perceptions and performs those movements which afford it an advantage in the struggle for existence, or cause a pleasant sensation (a feeling of pleasure).

The first cry of a new-born child is a purely physiological, a so-called reflex action, which reaches consciousness just as little as the first perception of the senses and of motion. It is only with the gradually developing consciousness, with the rise of the personal feeling (ego) that perception begins; the differentiation of sensations is made consciously; the attention is directed to single ones of these sensations; these are lifted up into the inner aspect, and finally there results apperception. The child, through its muscular sensations (that is through the sensations of tension, of contraction and of laxity of its muscles), gradually reaches a consciousness that certain processes within itself are connected with certain muscular motions; it then tries to call forth these muscular motions by means of images from its memory. When this is done successfully the first conception of causality has developed in the little cosmopolitan, the steps being in the following order: Sensation, perception, differentiation, conception, will and motion—all arising out of experience. The child begins to carry out conscious movements; for instance, to cry intentionally. This has for it a quickly apparent result: as a consequence of its crying it is nursed, and now experiences the highest pleasurable sensation which it has as yet learned to know. At the same time its conception of causality has greatly expanded; the association of its concepts and its movements has led it to this conclusion: "If I cry, I am suckled." Its logic is about to develop.

Meanwhile the child hears not only the sounds which it produces itself, but soon also distinguishes different kinds produced by others. It soon learns to know the different pitches in which people speak, and soon recognizes the different tone-colors of voices. If its attention has once been directed toward the differences of these sensations, it tries to produce these variations for itself, which can only be done by means of different muscular movements, which are experienced by it. It sees and imitates many such movements (mouth positions), others it discovers by means of its own attempts. It will call forth and reproduce, more especially, those movements which are pleasurable or useful to it.

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[5] Helmholtz had two classes of critics of his Sensations of Tone: (1) Psychology objected to reduction of esthetics and emotion to physical cases. (2) physicists and mathematicians claimed he had not gone far enough in explanation of physical cases.

If we leave the nursery and try to imagine grown-up men in the primeval condition of intercourse, we may well take it for granted that they soon discovered how much sounds, that were produced consciously and intentionally in different pitches and timbre, assisted communication by means of visible gestures of the entire body or parts thereof. The more man became conscious of the extraordinary power of modulation of his voice by means of the play of manifold, empirically discovered movements of the mouth, tongue, and gums (naturally without having the least idea of the physiological and physical processes involved) and the greater the practical benefit which he learned to draw from it as a social creature ("political animal," Aristotle), the more he developed these "sound-gestures," that is, spoken language, and in the course of time used it even predominantly for communication, and neglected the further development of speech by means of visible gestures of the other parts of the body. The visible gestures of the entire body, which even now form the principal means of intercommunication of animals, are used by civilized man only as an adjunct to speech, although among some nations (for instance, the Italians), they are still very largely used. They fall into entire disuse with the introduction of the written language as a means of communication, which, owing to the acquired necessity of connecting certain sound images with certain definite symbols, can call forth within us any desired circle of representations (Vorstellungskreis), in the same manner as the objective perception of gesture and sounds. Next to speech itself, writing is probably the greatest invention of the brain of the "political animal," because it is practically that which is most full of meaning.

Without the ability to perceive and produce different sounds in different pitches, the development of human speech would have been impossible.

Would not, however, the ability to distinguish and produce only different pitches have sufficed to form a pure tone-language? Would this not have been possible with even one tone alone? This cannot be doubted. Such a language might have been formed somewhat like the telegraph code, which consists only of different combinations of dots and dashes (shorter and longer tones); and if we imagine these dots and dashes to be variously colored (different pitches) then we would have at our disposal a richer supply of expressions. One could use in this way acute and grave tones, rhythmically combined, for the development of a tone-language, without any participation of the mouth, the tongue or the gums, that is, without the simultaneous use of different tone-colors. But such a proposition appears to me just like asking whether a man, if possessed of only one leg and two arms, or of two legs and one arm, could have attained the same completeness of social development as with his present shape. The fact simply is, that he has four extremities, of which the lower are much stronger and longer than the upper; therefore, he walks most comfortably upright and develops his arms and hands for all sorts of useful and pleasurable purposes. Thus the man has an oral cavity which allows very great changes in form. This capacity is lacking in the majority of the higher animals, since their oral orifices reach nearly back to the rear teeth, and on this account the formation of the various vowels as well as most of the consonants is impossible for them. Man has an exceedingly flexible tongue; he has the ability to cut off the connection between the mouth and the nasal passages by means of the velum, and by means of experiments and experience he has learned not only to produce with this complex apparatus the manifold modulations of the tones of his larynx, but also to render the distinctions clear. It was more convenient for him to use this apparatus for coloring tones, employing few changes of the tones of his voice, than to develop the latter into a pure tone-language. We speak more easily than we sing, because speaking interferes much less with the breathing process than singing, and also because the laryngeal muscles tire more easily than the muscles of the oral cavity, etc. A singing in quick, short rhythmic movements (dots and dashes) such as would be requisite for a pure tone-language, if the latter were to have a completeness even approaching our present tone-color language (Klangfarbensprache), would bring us so often in collision with our breathing-rhythm that the effort would become a torture for us.[6] Man could, if he were compelled, by means of continuous practice, even in this direction perform wonders; indeed he could get so accustomed to the system that he would no longer experience its discomforts. If such a style of speech were to become the fashion, who knows what would happen!

[6] The birds have predominantly a tone-language. The parrot, on account of his thick, very flexible tongue and the shape of his relatively short oral cavity, is qualified, physiologically, for human tone-color speech, and intelligent animals of this kind can, it is well known, be trained to repeat words of the human language, as well as to imitate human whistling in various pitches.

It is still a long way from speech to song and thence to music, and yet, according to my conviction, song (even if not all music), developed out of speech. I imagine the process to have been somewhat as follows:

To the original "sound-gestures" belong especially the words of address, exclamations and interjections. Tones, lasting for a longer or a shorter period, are uttered strongly and repeatedly, as the mimical expression in sound (klangmimischer Ausdruck) of a condition of sensation. This probably was, in the beginning, a purely reflex process, analogous to the cry of the new-born child, but soon became a consciously used, useful means of expression. In very loud speaking, in the public loud praying of priests, it was found especially effective with the audience, to at one time raise the tone of the voice, at another to lower it; possibly this was not at first intentional and occurred as a natural result of the exertion and fatigue of the laryngeal muscles. Most people end a sentence with a lower tone than that with which they started—a tone-falling or cadence. To emphasize single, especially important, words, the voice was raised to a higher plane; by this means the speaker succeeded better in retaining the attention of the hearers than by talking in a monotone pure and simple. The more strongly breathed word becomes somewhat higher, just as the gas jet becomes brighter on account of the stronger pressure in the service-pipes. Stronger intonation is at the same time unintentional tone-elevation; but the speaker also often goes to a higher level; the orator intentionally uses different tone-elevations; his speech is, by the side of sound-gesture, also tone-language. In ordinary conversation we keep probably within a fifth; in excited discourse we probably use an octave. These aids to expression were probably used more especially by priests, seers, prophets, orators, and narrators; they simply proved to be useful for the attainment of the desired effects. From such a pathetic way of speaking to half-singing reciting is an easy step, and finally a scarcely noticeable transition. Soon the priests prayed almost altogether in a singing tone. The custom was derived from the Greeks and the Jews into the Christian Church, and was there developed in several directions. In all these cases the singing tone serves only as a practically useful strengthening of the expression. The tones of speaking still rule over the tones of singing.

Tones and words, when they are both connected in similar rhythmic members, approach more nearly what we at present call vocal music. Herein also one of the most important factors of music is developed—*a division of tones into a definite succession of intervals.* In ordinary speech one can let the tones of the voice run into one another without becoming actually indistinct; the thorough comprehension of a verse rhythm requires more sharply differentiated tone-intervals. Finally, the verse brings with it the most important element for the form of music—the repetition of the same or similar, larger, or smaller rhythmically formed parts. The single verse foot resembles a musical bar, the verse line (stichos) the melody-air, or "motive"; a group of verse lines resemble a "melody," or a "Melos" in the Greek sense of the word. The verses, when sung, give rise to regularity and order in the world of tones. *From the linking together of a succession of tone intervals, and of rhythm, in the narrower and the wider sense of the word, the science of tones—that which we now call "Music"—was born. With its release from these bonds it dies as a science.*[7]

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[7] Aristoxenus long ago made very apt observations about spoken and song rhythm. He says, according to Westphal: "In declaimed poetry, we, to be sure, experience strongly the difference between the rise and fall (thesis and arsis) of the verse foot; but it is very difficult here, perhaps impossible, to assign a definite space of time to these two periods; we are satisfied if the period allows us to note a certain number of accents (ictus); how long or how briefly one dwells on the enunciation thereof does not concern us; we take the privilege, and see in it just the advantage of a well-expressed style of declaiming, of resting longer on such syllables, which are especially important for the logical sense, irrespective of the fact whether it is done by a longer-continuing enunciation or by pauses; and it is also a matter of indifference at what these pauses occur; whether at the end of the colon (verse foot) or within the colon and the verse foot." (One sees from this that already in the delivery of Greek poems that were not sung, particular attention was not always devoted to the length and shortness of syllables, upon which Greek metrics are founded. Note of Hanslick.)

"In music, however, the duration of the arses and theses are subject to a definite measure of time, and likewise the pauses occurring here are integral parts of the rhythm. By the symbols —◡ we understand that the second syllable is half as long as the first, accordingly —◡= ♩♪ or ♩𝅘𝅥𝅯 They have no absolute value. (Except when they are determined by the metronome.) The values of the rhythm, assumed at the beginning of a piece of music, remain the same; in declamation, on the other hand, they may be changed at pleasure. In the rendition of songs, the change in time of the smallest parts of a bar (of the Chronos protos) has become almost the rule; it is just through this that this kind of vocal music approaches speech; but the musical rhythm must never be neglected to such an extent that it becomes incomprehensible to the listener. Herein much depends upon the art of the singer. The free instrumental 'performance of the melody of a song' rests upon the fact that it takes liberties, just like the singer, as if it desired to play words, and thereby often scores a great effect, and makes an impression very much as does a song. Exaggeration in a performance in this direction is in bad taste; it can easily become unmusical. The so-called phrasing must, in spite of all freedom, always be simple, clear and musical, and never leave the listener in doubt as to the kind of time. Liszt, Rubinstein and Joachim were and are masters in this kind of playing. Modern music, like that of Chopin, Schumann and others, permit of a performance that gives very much more the effect of words. In the music of Bach and Händel, as well as in that of Mozart, Brahms, and R. Wagner this kind of playing has an absolutely bad effect. These composers treat the composed verses metrically and musically-rhythmically with as great a degree of correctness as modern taste permits, although by this I do not mean to say that they

Since one can find in every history of music a description of the so-called old musical modes or tone-scales, from the Greeks down to the Middle Ages, and their physical explanations in the classic work of Helmholtz, I shall not go further into this subject and will only add that, in my opinion, these tone-scales were only determined after they had long been in common use. The musician composed (granting indeed that this word can be employed for the old recitative music), not according to fixed tone-scales, but constructed the tone-scales from the existing compositions, which persisted principally through tradition. But doubtless the development of the language, especially the pronunciation of the *vowels,* had a great influence upon these. We all know that with the same laryngeal tone (Kehlkopfton) we can pronounce the various vowels, that is, we can change the tone-color of the laryngeal tone by means of modifications of the oral cavity to such a degree, that the different tones of which the laryngeal tone is composed, now this, now that higher overtone or undertone, are heard to resound predominantly. If this were not so, or even if it were possible only to a certain degree, one could only speak and sing the deep-sounding vowels (U, O, Au) with deep laryngeal tones, the high vowels (Ae, E, Ue, I) only with high laryngeal tones. In fact it is difficult to produce the deep vowels on high tones and the high vowels on deep tones, but this can be done with practice.

According as certain pure or impure vowels predominate in a language or dialect, so must certain cadences become easier for the hearers or speakers. If one determines them according to their sound, by means of notes, and brings together the tones occurring at the various intervals according to their succession of pitch (Höhenfolge) within an octave, then

ought to be sung according to the metronome. Every composer ought to be treated according to his style. A sensible actor does not play and speak Shakespeare in the same way as he would Dumas. Similarly, a sensible singer sings Händel differently from Massenet. He who makes the mistake, while rendering the simplest song, of laying pointed emphasis on every word and verse of the poem—most effective for the bulk of a concert audience, that is for the unmusical or the slightly musical—often produces a musical caricature. He who performs purely musically, without much reference to the text, can give much enjoyment to the musical connoisseurs, in his singing of an aria of Händel or Mozart, but the vast majority of the audience will be entirely unresponsive, the more so, if he emphasizes or tries to emphasize also in singing songs only that which is the purely musical. Maintaining the true mean depends on the judgment of the singer, which judgment may be somewhat changed, that is, restrained or stimulated by frequent hearing of other singers, and also by a capable music instructor. But one soon learns to recognize that which has been acquired, and finally the temperament of the singer will reassert itself. It is given to but few to quickly sense what is musically correct for the largest portion of the audience, from which the personality of the singer obtains a large share of the success. In a dark concert hall the judgment would be very different; the public wants to see, and especially *see itself.*"

one gets those tones, which are used, as a scale of tones following one another—a tone-scale. It is clear that such a tone-scale must result very differently, according to the language of the people concerned (Lydian, Doric, Ionic), indeed even according to the taste of the individual. The Italians did not cling to the tone-scales in the form in which they had come down to them from the Greeks and Romans. The Jewish Christians effected changes, so that new tone-scales kept arising, which finally were no longer determined. The popes, Gregory the Great, and Sylvester, tried to fix again the old tone-scales for Church music, as well as to determine a few newer tone-scales. Many improvements in the notation of the intervals also arose, the symbols being placed on and between lines, instead of the hooks previously used. But all this did not satisfy the need for mass singing, so-called harmony because in those days it sounded pleasant; and moreover, the learning of the complicated tone-scales was very tedious. It took centuries after the introduction, by Guido d'Arezzo in 1050, of principles of the present notation, before a system was completed by which the present chromatic scale was finally developed from the old tone-scales. From this scale two tone-scales were selected which contained seven tone-intervals within the octave, and which scales differed from each other only by the fact that either the small or the great third (minor or major) was employed. Besides, this scale, which was composed simply of whole and half tones, in definite succession, could start from any desired one of the twelve half-tones lying within the octave; the difference consisting only in the keynote (Ausgangston). All our harmony and melody rest on this system. Who discovered it? It certainly was not invented by one man, and forcibly imposed—the pope himself could not do that! It has developed out of practical need, out of convention (in the previously defined use of the term). For its determination, to be sure, experienced musicians were necessary; a certain fixed definite symbol was needed for each single tone; definite names had to be found for the modes, that is for the keynotes of the scales, definite names for raising and lowering tones, and so forth. History tells of no actual inventor of our present system of music; it has existed in its present form only about 250 years.

I mean that our present system of music, into which we are brought, and which only the aid of historical study can show us to be different from formerly, has developed just as gradually as our language. Luther did not invent the New High German in his Bible translation (it arose as a compromise between the South German and the German of the lowland or Plattdeutsch), nor did he force it upon the German nation; but he fixed it as the written language in the most important and most popular book of his time, and this language, greatly enriched by Goethe and Schiller with South German words and phrases, will presently absorb all the dialects, one after the other, simply because it is fixed; it does not on this account remain rigid, but is always absorbing new words and phrases, both from these dialects and foreign tongues, not through any intentional violence (Vergewaltigung) on the part of single writers, but from a certain ever-existing need for more exact terms to describe old and new qualities and occurrences, as well as from a need for variation in sound. We can as little imagine that our present system of tones will change as we can imagine any essential variation in our language. Music without rhythm, without melody and harmony, without a certain conventional regularity, would no longer be music to our understanding, but a concatenation of crying, howling, raging, weeping, groaning, and rejoicing—in the form of shorter or longer interjections. It would be analogous to a relapse from the present system of order to a savage state, to individualism, to anarchy, to nihilism. Language, like music, is also a work of art; if cannot even be thought of without a conventional arrangement of sentences, without a definite regularity in succession and order of the ideas that are to be expressed.*

I have adduced all this to support my opinion that our modern music with its development of harmony and melody, (which, apparently, could only have reached their present importance in our modern system of tones), did not develop according to anatomical-physiological laws of nature, and cannot be continued in the mathematico-physical formulas of a Laplace-like genius (Du Bois-Reymond), but arose from individual sensations and impressions and the needs of society and civilization, which, to be sure, have their origin in the important psycho-physical qualities of man. The physical representations of the status of our present theory of harmony cannot claim to be more than mathematical attempts to explain the combinations of tones which have already been found instinctively and which affect us pleasantly (consonant) or unpleasantly (dissonant). The indefinite part of this lies in the fact that "pleasant" and "unpleasant" are just as variable qualities of sensations at different times and for different persons as, for instance, are good and bad. As it is, I cannot conceive that the sound of a major third, and still less that of a minor third, should ever have been considered an unpleasant interval (a dissonance), although many centuries ago it may possibly have affected people that way, and as regards the number of violations of the upper partials (Schwingungsverhältnisse) expressed in numbers, it

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* Billroth did not live to hear our recent degenerate Atonal and Aformal Music, but these remarks are an appropriate condemnation of it.

BILLROTH

ought to be so. In art, as a rule, "Whatever passes is allowable," particularly to those who take an especial delight in art and make it their own by means of study and practice.

As to the difference of major and minor keys, we at present usually connect with them conventionally the ideas "joyful" and "sad," having in mind, perhaps, the literal meaning of the terms themselves, "hard" (dur) and "soft" (moll). It has always seemed to me that the older dance music and love songs of the French and other civilized nations were generally in a minor key. The minor keys also seem to predominate in the folk-songs of the half-civilized Slavic and Hungarian nations, which songs are also used for dancing; they also occur in the monotonous songs of Oriental nations. This has been interpreted to mean that all nations of moderate culture have a melancholy temperament; that the major key is the "natural" one, partly on account of the rhythmic relations of the fundamental tones, partly on account of the unconsciously heard over-tones, which make the major chord. I think this explanation is incorrect. My opinion is that it requires less exertion of the laryngeal muscles to sing the minor third or sixth than the major; even the trained singer will admit this. Most persons talk in the minor key, and sing or scream in a major key. In all languages (the monosyllabic ones excepted), the last syllable usually drops into a minor key. When a people began to sing, it naturally knew nothing about a scale of tones, or a mode (key). But the succession of tones of the existing song recitations must have led principally to a minor key, when tone-scales began to be fixed. Every person speaks usually in one key; in ordinary conversation I speak in D minor, in singing in D major. If I lie quietly on my back and without exertion sing a scale upwards, the following are most convenient for me as a bass-singer: D, E, F, G, A, B, C (Doric); the same holds good when going down the scale. In singing tones which on account of their height require exertion, a little more exertion does not matter; major and minor are, when high-pitched, alike in this respect. If therefore the development of the modern chromatic scale is based more on social and conventional causes, the development of major and minor, and especially the latter's predominance in the beginnings of music apparently, seems to me to have more purely physiological reasons, namely, the lesser exertion of the laryngeal muscles and the relation to the cadence of words.

The development of arts and sciences, like every development, and every construction, does not proceed by leaps and bounds; every addition, every advance, proceeds from something that has gone before. After the chord of three sounds was once felt by many to be the most agreeable of harmonies, and accepted as such, the rest developed from it in a tolerably logical order. A mathematical proof that only this trial could please, and that it alone could serve as the conclusion of a system of harmony, cannot be adduced. We can only say that man in the course of his cultural evolution has developed a pleasure in certain regular relations. This holds true especially for architecture, which, to be sure, was dependent on purely physical relations, on the empirically found laws of statics. Later that which was originally necessary and useful in the relations of construction became gradually the customary, the agreeable, the beautiful. But there is great difference between visual and auditory perceptions, and it is a bold idea to think that our organ of hearing measures the agreeable in a consonance of sounds according to the physical causes of its intervals, for instance, the intervals of the octave, as the eye measures an edifice with a standard of length. I cannot grant to acoustics such an unconditionally fundamental importance for the development of music as I accord to statics for architecture; for every combination of tones is possible, but not every kind of combination of building material. I could hardly imagine what one could urge physically and psycho-physiologically against anybody who might claim that for him the combination of C, C sharp, D, or C, D, E, or C, C sharp, D sharp, was most agreeable and was harmony, while on the contrary a triad was very disagreeable. It is only a question, how large a public there is which also feels, or claims to feel this, and which hears pieces composed on the basis of these tone-combinations, which to it are agreeable, with increasing and continually spreading enthusiasm: one cannot furnish a proof that such a music is absolutely impossible and not beautiful. The majority of the public carries the day, and when it is tired of this kind of music, will follow others who offer something else. An analogy may be found in the formation of a sect, which results from the impressions and broodings of a single person, but becomes a widespread religion only by the number of those who join it. The impression of the one is, to be sure, a part impression of the whole (Teilempfindung des Ganzen) in a particular form; but why just this form in which it appears should especially please few or many of its contemporaries, cannot be demonstrated either mathematico-physically or psycho-physiologically.

I must finally express my conviction that *the sensation of that which is harmonic is conventional, that it gradually developed in the subject, and is not originally necessary.*[8]

[8] An ear-witness (Wittmann's, in the "Neue freie Presse") gives the following report of Chinese theater music: "The star, dressed like a prima donna, sings an aria. This indeed is almost more than a European ear can stand. We simply cannot imagine how they can call such a maiming of the voice (Stimmverquetschung) singing. It is in this way that a raven croaks, a cat meows, a door creaks

The same holds good for what we now call music. I would not think of maintaining that, before the introduction of the diatonic and chromatic scales there could not have existed any harmony and melody partially agreeable to us. Both of these words are derived from the Greek and already at the time of their first use denoted certain combinations and successions of tones which were agreeable to the ear. It is probably doubtful, however, if we should experience as a pleasant succession of sounds at the present day, all that which the Greeks called melodious (from melos, song). The same holds true of the music of the Middle Ages. Indeed, many of the Protestant chorals, fixed by Luther, intended to be sung by the congregation, and often taken from worldly folk-songs, now hardly give us a melodious impression, nor do the words strike us as poetical. The memory of childhood and of the impressions which the Protestant order of worship made upon the youthful mind, is necessary for a warm appreciation of many of these chorals. As it is, a Catholic would find it hard to accustom himself to the joint singing of the congregation,[1] just as a Protestant might not appreciate the mass, and the music of the masses. Whoever seeks the opportunity of developing himself musically, must live in both these forms of Christian church-music, and will be able to find melodious elements in both; it nevertheless requires a sort of historical self-adjustment. What now does the term "Melody" denote for us? One is accustomed to say: "A rhythmically formed succession of tones, which is agreeable to the ear." In general there is no objection to this, and we will now go into the matter a little more deeply.

I have previously pointed out that music developed into a "science of tones" chiefly because it connected itself with such word-thoughts (Wortgedanken) as were rhythmically formed in verses; by this it was bound to a certain form of rhythmic repetition. If one sang the music without words, or played it on an instrument, then one had a rhythmically ordered

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on its hinges, a dry boot-sole squeaks, a pig squeals, a wheel screeches under the brake; thus the Easter rattle of the street gamin resounds, but no human being ever sings in this way. The orchestra, besides, very seldom stops. The drama is almost continuously accompanied melodramatically. Higher dramatic movements are naturally treated more in detail, especially those of battle and conflict. The cymbals whirl, the tom-tom resounds, the whistles scream, the fiddles moan, and all is raging topsy-turvy, and over all the terrible kettle-drum rattles as if madness were wielding the hammer. Not a trace of harmonic combination. Here the genius of discord conspires with all evil music-hating fairies to call into existence a symphony of horrible noises of which we can simply form no conception." The Chinese are the oldest civilized nation, and take the same delight in their music as we in ours. Who is right? Both; we can here form no compromise. Our reporter adds: "In the living presence it (the theater) gives the impression of a senile art that has become childish." Will this also be the end of our theater, of our art?

succession of tones, a "melody," *which probably still bore the traces of speech cadence, but could exist by itself.* Thus arose the regular, absolute music, music without words. I heard in Grosswardein, if I am not mistaken, several czardas, which pleased me very much, played by a fairly civilized gypsy orchestra; when I found out that they were composed by the first violinist, I asked him if he had had them printed; he answered: "Not yet, I must first look for words for them." Witty persons among the populace not infrequently make new verses for existing well-known dance tunes of a definite rhythm; the improvised quatrains call forth the same enjoyment among the people that improvised couplets do among the audience of a theater. Thus at one time the music comes first, at another the words. It is my opinion that in the beginning, however, the words formed the point of crystallization for the music; that the successions of tones, which were rhythmically joined on account of the words and verses, have separated themselves from the words, and gradually became forms which were sounded independently of the words and were differently connected. This is just as comprehensible as the fact that, later on, new speech-rhythms developed without reference to tones. The forms of the science of tones, when separated from the words, gradually attained a certain degree of independence, and found the *new condition of their development in the new material.* Thus independent instrumental music arose. Its derivation from church music, march music, dance music and work songs, can be plainly recognized in the forms of our modern instrumental music. The suites of Bach are composed of the melodies of dance music: courante, sarabande, passacaglia, gavotte, minuet, gigue. The minuet has maintained its position in sonatas and symphonies down to the present day. Marches, and especially funeral marches, still occur in serious concert music. The rhythms of riding songs, traveling songs, thrashing songs, anvil songs (Schmiede Lieder), sailor songs (when heaving the anchor), spinning songs, harvesting songs, etc., have been derived from the rhythmic movements of the body and are arranged for them. If, indeed, they have not been used for purely instrumental music in exact song form, they have enriched it by means of their various rhythms. Since language is one of the prime factors which affects musical rhythm, it follows that international intercourse, which brought us more frequently into communication with the languages and popular music of other nations, has enriched international music with very many new rhythms; it (music) has taken up the rhythm of the national English, Scandinavian, Italian, Slavic, Hungarian dance-songs and dances.

It will probably be universally admitted that we can conceive of a "melody" only as a rhythmically ordered succession of tones, and that this rhythm arises from different sources. But the above definition of "melody" requires also "a succession of tones *pleasant* to the ear." One at once asks: "Pleasant to what ear?" If one answers somewhat like this: "To a musical ear," the question at once arises: "What are the characteristic signs of a musical ear?" We thus get into a circle of questions, from which I find no escape. We do not inquire about rhythm, whether it is unpleasant or pleasant; the most one can say is that a quick rhythmic motion can be disagreeable to us, if we are in a contemplative, quiet mood; it disturbs us or tires us on account of accompanying it, and the like; but a rhythm as such, independent of our moods, does not easily arouse an impression of the beautiful or the ugly. It is different with successions of tones. The series C′, F′ sharp, B′, F″, and also C, C sharp, F′ sharp, B, give me an unpleasant impression; why? because so pronounced an after-sound (Nachklang) remains in my ear when these tones do not follow one another quickly, that I experience all four sounds as a combination of sounds, and this combination displeases me. Here, again, we may come back to harmony. In the popular, conventional term of modern music, a tone series seems pleasant, if it moves within the harmonies to which we are accustomed and which therefore have become agreeable to us. Thus we conclude that melody and harmony are most intimately connected. If one recalls any melody that has become popular, one finds it moves within easily scanned, not too variable harmonies and successions of harmonies, which have become customary to us (conventional), and that it finally returns to the key from which it started. This, indeed, constitutes what we usually call the melodious. The number of persons who are sufficiently musical that they cannot only sing simple songs together but can also be taught songs in parts, is uncommonly large in the German nation. German-Austrians are especially distinguished in this respect. At St. Gilgem near Salzburg, on the banks of the beautiful Aber lake, where I have made my summer abode, there is a small chorus and a small wind orchestra; the people take the greatest delight in singing and playing together, and practise new pieces every winter. And yet the village has but twelve hundred inhabitants. Naturally one must not criticize these performances too severely; but the pleasure to be derived from music is sufficient to pay for some little trouble to obtain it. The music of the mass, in which such of the children and grown persons as are suited for it are trained by the organist and schoolmaster, gives more scope for church music than the singing in unison of the chorals in the Protestant order of worship. The Catholic service also has a powerful effect on the imagination, inasmuch as it draws to its aid music, painting, and colored sculptures (for the statues of the Madonna, Christ, and the

Saints are always painted). Hence also are derived influences which permeate the everyday life of the people.

The connection of the rhythmic and the harmonic is probably generally recognized; indeed, to some extent, it is always assumed. Similarly the desire and the striving to multiply the succession of harmonies is intimately connected with the same wish in regard to melody, and this holds good just the same for rhythm as for the succession of tones. The music which we have discussed previously is usually termed popular, because it readily spreads through large masses of society. But this does not satisfy those who feel the need of much music, and who from an innate disposition always think of music, and from inner compulsion must always busy themselves with it. Even in church music, not only as regarded the choir, but also the individual singers, there arose the desire for change, for something new. Men became wearied not only by the successions of harmonies, which finally became tiresome from eternal repetition, but also by the melodies, which always proceeded at the same rate and changed but little in rhythm, and which were originally sung by the tenor, then possibly also by the bass, and finally almost exclusively by all in harmony. Attempts were made to have the voices not always commence and finish at the same time, but to enter gradually, and to let some cease occasionally. Again, the rhythms were divided into smaller parts or enlarged, without bringing about any interruption in the harmony. The latter change led to the so-called polyphony. Each single voice went its own way without disturbing the harmony of the whole; in order to preserve the unity of the whole, they came together from time to time, and necessarily, of course, at the end. Thus arose the canon, fugue, and other tone forms; music had gained the mastery over words. The new method became popular and underwent further and further extension. The rhythmic element (rhythmic motive), from which polyphony arose, for a while aroused more interest than the harmonic and melodious, indeed such exclusive attention was devoted to it that the harmonic was somewhat neglected, attention being mainly directed to the individual voices. So heedlessly did the composers proceed in this direction that music became more difficult to execute and more removed from the popular style, so that to a certain degree it represented even its antithesis. This led to a return to the predominantly harmonic and melodious. Naturally, in the excessive development of polyphony, the words and the total expression (Gesammtausdruck) of the word-thoughts suffered; the excessive and continuous polyphone treatment of the voices no longer corresponded to the state of mind which was meant to be called forth by the words. This conflict between music and the words, as well as between polyphony and harmonic melody has already lasted for several centuries in the history of music. Hardly had certain concessions been made on each side when both the artist and the public wearied of them, and diverged again until new compromises were once more reached. Since the connection of word-thoughts and tone-expressions (Tongestaltungen) has no natural physical basis, but is purely conventional, this hesitation, how far we should favor the one or the other perception in their simultaneous action (the hearing of the words and thoughts or the hearing of the tones and tone series) will last as long as man speaks and sings at all. Language has developed its own forms for the expression of thoughts and impressions, for the purpose of poetical expression, according to certain conditions inherent in itself. Tone expressions have separated from the words and from song and have also developed into certain forms, into the science of tones, according to certain conditions which exist in the world of tones (Tonwelt). Nevertheless, the conditions under which the greatest effect of poetry and the greatest effect of music are developed are only partially the same; often they heighten each other, not infrequently they stand in complete contrast.

Turning our attention from the connection between tones and words, we may now glance at the development of instrumental music—the so-called absolute music. I can easily imagine that in times past the instrumentalists found it wearisome to always continue with the same rhythm in the conventional harmonies. Each one, therefore, began to make various changes for his own entertainment. So long as only one took this liberty, the innovation could serve to interrupt the monotony very pleasantly. If, however, every quartetist in every bar allowed himself such changes as suited him, in comparison with the conventional harmonic, the general effect would very often be bad. This tendency toward variation in the performance of quartets and symphonies probably lasted longest among the Italians. Thus Spohr in his autobiography[9] speaks with horror of the variations which even the horn players and clarinettists allowed themselves in the accompaniment to one of his violin concerts at Rome. When not carried too far, however, variation also pleased the greater public, and the composers soon introduced more movement and a regular polyphony into their instrumental works. But the separation of music from words had still another result for the development of instrumental music. The melody was no longer bound to the order of words—it could, therefore, be used also piece-meal, and could be separated into single "motives" (Bewegungsmomente), which could then be utilized more freely; one could put the motives taken from the melodies alongside, over or under each other, and play with them at will harmonically and rhythmically. Indeed, melody in the former coherent form was no longer used; one could work nicely with shorter or longer motives, and then for a change bring in again a finished melody, and so forth. Thus new tone forms arose, based on a purely musical foundation, among which the so-called sonata form soon gained the upper hand. From Scarlatti and Emanuel Bach down to Brahms it has been developed in many ways. Its essence consists in the change of rhythmic and harmonic motives, in partly predominantly polyphone, partly predominantly melodious development of tone forms. The rich diversity developed in this way in the hands of eminent composers has been striking; besides the sonata forms or even within them the harmonic variation of complete melodies—of a so-called theme—has been developed. One can, to be sure, call this playing with motives, and especially as regards the so-called development in the sonata form, a variation of the same, but this is not so strictly bound to a definitely formed theme. This development of purely musical forms, however, had a strange effect on that which we usually call melody. In polyphone music the attention is so much taken up in the pursuit of the rhythmic movement of the single voices, by the motives, which move away from each other, toward each other, over each other, that the discords which arise in passing are not heard, provided only that harmony sets in again at certain rhythmically important points. We can endure a considerable amount of dissonance, even long continued, provided only we feel in advance, by means of our impressions which are accustomed to the usual succession of harmonies, that consonance must soon set it. As a good instance the long trill on the seventh at the end of the Athalie Overture by Mendelssohn may be taken. *The phenomenon that we principally and consciously perceive, that upon which we concentrate our whole attention, leaving all else observed physiologically at the same time unnoticed,* is widely extended in the domain of sense-perceptions. Thus most persons have a number of opaque little points in the vitreous humor of the eye, which they do not notice so long as they look at objects and direct their glances at them, whereas they at once perceive them as so-called "mouches volantes," as soon as they unconsciously look into the distance; similarly the after-images in the complementary colors do not disturb us because we do not notice them. The same, however, takes place in purely psychical phenomena. *It is an essential characteristic of the civilized man,* which must be acquired from childhood, to direct, *to concentrate the attention,* the "inner aspect" upon some few concepts, whether they arise directly or are reproduced.

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[9] Vol. I, p. 330.

We have previously admitted that the "melodious" consists essentially in the fact that the tone series takes place within accustomed harmonies which do not change too frequently. This conception, however, becomes much more enlarged for the musically educated; he gets accustomed not only to pass over transient discords, but even to feel them to be agreeable or at least "interesting," provided that at the same time he can clearly perceive the most important lines along which the tone masses proceed. If we hear only firm, strong bass notes, and retain them to a certain extent as a melody or theme, it does not matter if we hear at the same time rising or falling diatonic or chromatic scales. Indeed, even series of harmonies existing strong and firm can be made more interesting for us by parallel scales; we do not notice the momentary passing of horrible discords. Take, for example, the introduction to Mozart's "Don Juan" overture. If we designate these scales as a melody, probably no objection can be made, although there is a tradition that Mozart inserted them in the score later on. Chromatic tone series occur so often in melodies and motives, from Bach on to Brahms and Wagner, that we have grown completely accustomed to them; we transfer the rhythmic movement of the tone series (that which one calls in painting the lines or outlines) to the inner point of hearing (physical and psychical) and do not notice that which resounds at the same time.

It would not be very difficult for a good composer to make an interesting musical motive out of the tone series which, as I said before, sounds disagreeable to me—the tone series bacb (b-flat, a, c, b) and aecb (a, e, c b-flat) out of which Liszt and Schumann made fugues, do not sound much prettier when heard for the first time—yet after becoming used to them by frequent repetition, one may consider them melodies.[10]

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[10] On the piano, inasmuch as it is difficult to distinguish the various voices of a polyphonic piece on account of the weak resonance (Fortklingen) of tones that are held, and also on account of the same tone timbre of the instrument, harmonic harshnesses are especially emphasized. To play fugues for the piano is very interesting; to simply listen to them affords very doubtful enjoyment; the same holds true of other polyphonic music written for the piano, especially of complicated musical compositions arranged for four hands. Well rehearsed polyphonic orchestral and choral music is much easier to understand, because the single voices stand forth better on account of the difference of the tone colors of the various instruments and voices. The fact that difficult music (schwierig zu bewältigende), especially a string quartet, sounds perfectly horrible when played by otherwise able dilettanti, is principally due to the uncertainty and timidity with which the tones are sounded and the bow is moved. Therefrom arise so many uncertainties, and so much scratching and scraping is heard alongside of the tones, that much good-will and endurance are required to listen to new and complicated music under such conditions. On the piano one plays either correctly or incorrectly; in string music (and also in wind music), there are so many shades of incorrectness which we designate as impurities, that the hearer often does not know what he is hearing,

As long as the appearance of transient discords does not go beyond a certain limit, it entertains and interests us; for that which we conventionally are accustomed to experience as harmonious wearies us, if its effects are continuous and unchanged. But where is this limit of the interesting? How long can we endure the discords and complication of rhythm without having our enjoyment lessened? This cannot be determined, inasmuch as here we have to deal with purely individual, and up to a certain degree, social factors of sensation (Empfindungmomenten, esthetic motives). Much always depends upon how many follow, as disciples of a so-called new style of composition, that new composer who predominantly pursues the interesting. It he has really found the direction toward which the soul of the musical, or indeed of the unmusical people was striving, then he will gradually find more and more disciples; if he has not found this direction, they will let him go by himself and laugh at him.

Helmholtz expresses that which we have developed in this section in our own fashion, in the following manner, in his classical book:[11] "The amount of harshness which a hearer is inclined to endure as a means of musical expression, depends on his taste and his habits; therefore the boundary between harmony and discord has often undergone changes. In the same manner tone-scales, tone-modes and their modulations have been subject to manifold changes, not only among uncultured and rude peoples but even in those periods of universal history and among those nations, in which the most perfect flowers (Blüthen) of human culture were developed (zum Aufbruch kamen)."

From this follows the conclusion, which is not yet sufficiently impressed upon the minds of our musical theorists and historians, that *the system of tone-scales, of tone-modes, and of the harmonious interweaving of the same does not rest upon immutable laws of nature, but is the result of esthetic principles, which in the progressive development of humanity are subject to change, and will continue to be so in the future.*

CONCLUSIONS

In expressing my personal impressions on Billroth's contribution to the psycho-physiology of music, I may say that it is an admirable attempt and largely a successful one, to blend and harmonize the esthetic aspects of music on the one hand, with the physiologic and physical aspect on the

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and how it really ought to be. I must also confess that I have seldom heard string quartets, even those composed of the best artists, which at the beginning of a concert sounded absolutely clear to me. Is it due to the performers or to the fact that the ear in a large room must first accustom itself to the combination of sound from the four stringed instruments, which is most beautiful in itself?

[11] First edition, p. 358.

other. That this is a task of exceptional difficulty is proved by the fact that few successful writers on this subject have emanated from the German nation. Helmholtz in his "Sensations of Tone" has emphasized this difficulty. He holds [12] that it is a mistake to make the theory of consonants the essential foundation of the theory of music and insists that the essential basis of music is melody. This is at the outset a confession which affords great comfort to the esthetic worshipers of music. The life and essence of music, therefore, do not depend upon physical, mathematical, or physiological facts, but upon melody—a psychological entity. Helmholtz in his well-known work, incurred the enmity of two classes of critics, those of an esthetic or psychological bent, and those of a physical or mathematical leaning. The same experience would have fallen to the lot of Billroth had he lived to see the publication of his book; the fact that the volume was not printed until after his death modified the fierceness of the critics. Nevertheless, a few of them have brought forward the same objections that Helmholtz' work aroused. The Latin proverb, *Mortuo leoni et lepores insultant* (Even hares can insult a dead lion), applies to these critics, but perhaps the best answer to them is found in the work of Helmholtz just quoted: "Harmony has become to Western Europeans during the last three centuries an essential, and, to our present taste, indispensable means of strengthening melodic relations, but finely developed music existed for thousands of years and still exists in ultra-European nations, without any harmony at all. And to my metaphysico-esthetical opponents I must reply, that I cannot think I have undervalued the artistic emotions of the human mind in the Theory of Melodic Construction, by endeavoring to establish the physiological facts on which esthetic feeling is based. But to those who think I have not gone far enough in my physical explanations, I answer, that in the first place a natural philosopher is never bound to construct systems about everything he knows and does not know; and secondly, that I should consider a theory, which claimed to have shown that all the laws of modern Thorough-bass [13] were natural necessities, to stand condemned as having proved too much."

It is true that acoustics constantly employs conceptions and names borrowed from the theory of harmony, and speaks of the "scale," "intervals," "consonances," and so forth; similarly, manuals of Thorough Bass generally begin with a physical chapter which speaks of "the numbers of vibration," and fixes their "ratios" for the different intervals. But, up to the present time, this apparent connection of acoustics and music has been wholly external, and may be regarded rather as an expression given to the feeling that such a connection must exist, than to its actual formulation. Physical knowledge may indeed have been useful for musical-instrument makers, but for the development and foundation of the theory of harmony it has hitherto been totally barren. And yet the essential facts, within the field here to be explained and turned to account, have been known from the earliest times. There is good evidence for believing that Pythagoras (B. C. 540–510) knew that when strings of different lengths, but of the same make and subjected to the same tension, were used to give the perfect consonances of the Octave, Fifth, or Fourth, their lengths must be in the rations of 1 to 2, 2 to 3, or 3 to 4 respectively, and if, as is probable, his knowledge was partly derived from the Egyptian priests, it is impossible to conjecture in what remote antiquity this law was first known.

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[12] Translation by Alex. J. Ellis, Preface, page vii.

[13] Thorough-bass is the science that treats of writing correct harmonies to a given melody in the bass.

Concerning the limits of the purely psychical ("rein Psychische") in the perception and interpretation of Music, Helmholtz (l. c.) admits that: "Musical esthetics has made unmistakable advances in those points which depend for their solution rather on psychological feeling than on the action of the senses, by introducing the conception of movement in the examination of musical works of art. E. Hanslick, in his book 'On the Beautiful in Music' triumphantly attacked the false standpoint of exaggerated sentimentality, from which it was fashionable to theorize on music, and referred the critic to the simple elements of melodic movement. The esthetic relations for the structure of musical compositions, and the characteristic differences of individual forms of composition, are explained more fully in Vischer's 'Esthetics' (Aesthetik). In the inorganic world the kind of motion we see reveals the kind of moving force in action, and as the only final method of recognizing and measuring the elementary powers of nature consists in determining the motions they generate, so it is also for the motions of bodies or of voices which take place under the influence of human feelings. Hence the properties of musical movements which possess a graceful, dallying, heavy, forced, dull, powerful, quiet, or excited character, and so on, chiefly depend on psychological action. In the same way questions relating to the equilibrium of the separate parts of a musical composition, to their development from one another and their connection as one clearly intelligent whole, bear a close analogy to similar questions in architecture. But all such investigations, however fertile they may have been, cannot have been otherwise than imperfect and uncertain, so long as they were without their proper origin and foundation, that is, so long as there was no scientific foundation for their elementary rules relating to the construction of scales, chords, keys, and modes, in short, to all that is usually contained in works on 'Thoroughbass.' In this elementary region we have to deal not merely with unfettered artistic inventions, but with the natural power of immediate sensation. Music stands in a much closer connection with pure sensation than any of the other arts. The latter deal rather with what the senses apprehend, that is with the images of outward objects, psychical processes collected from immediate sensation. Poetry aims most distinctly of all at merely exciting the formation of images, by addressing itself especially to imagination and memory, and it is only by subordinate auxiliaries of a more musical kind, such as rhythm, and imitations of sounds, that it appeals to the immediate sensation of hearing. Hence its effects depend mainly on psychical action. The plastic arts, although they make use of the sensation of sight, address the eye almost in the same way as poetry addresses the ear. Their main purpose is to excite in us the image of an external object of determinate form and color. The spectator is essentially intended to interest himself in this image, and enjoy its beauty, not to dwell upon the means by which it was created. It must at least be allowed that the pleasure of a connoisseur or virtuoso in the constructive art shown in a statue or a picture, is not an essential element of artistic enjoyment.

No doubt the ultra physical psychologist would have been more pleased if Billroth had succeeded in localizing a circumscribed area of the cerebral cortex and given experimental and pathological evidence that it was the center for musical apperception, as Broca did for the center of speech.

But when we are guided into the understanding of the true nature of musical apperception the difficulties in the way towards such an anatomical basis for a cortical musical center became evident.

The sensation and consciousness of music after all that has been said is something more than *a psychic process collected from immediate sensation.* The relation of musical apperception to the sense of hearing is something more intimately psychical than the relation of the senses to the functions of their anatomical end organs (e. g., of the sense of sight to the eye or rather to the retina).

To utilize the comparison of Helmholtz in the introduction to his Sensations of Tone:

"It is only in painting that we find color as an element which is directly appreciated by sensation, without any intervening act of the intellect. On the contrary, in music, the sensations of tone are the material of the art. So far as these sensations are excited in music, we do not create out of them any images of external objects or actions. Again, when listening to the orchestra we recognize one tone as produced by a violin and another by a clarinet, our artistic enjoyment does not depend upon our conception of a violin or clarinet, but solely on our hearing of the tones they produce, whereas the artistic enjoyment resulting from looking at a marble statue does not depend on the white light which it reflects into the eye, but upon the mental image of the beautiful human form which it calls up. In this sense it is clear that music has a more immediate connection with pure sensation than any other of the fine arts, and, consequently, that the theory of the sensations of hearing is destined to play a much more important part in musical esthetics, than, for example, the theory of perspective in painting. Those theories are certainly useful to the artist, as means for attaining the most perfect representation of nature, but they have no part in the artistic effect of his work. In music, on the other hand, no such perfect representation of nature is aimed at; tones and the sensation of tone exist for themselves alone, and produce their effects independently of anything behind them."

Fielding H. Garrison describes resemblances of scientists and clinicians to musicians in an article entitled "Medical Men Who Have Loved Music" in *Bulletin of the Society of Medical History of Chicago,* October, 1920, page 158; he states: "If we place in juxtaposition characteristic portraits of Beethoven, Rubinstein, the anatomist Henle and the mathematician Jacobi, this resemblance will at once become apparent. By the same token, Hyrtl, the anatomist, looks like Haydn; Carl Ludwig in profile is like Liszt or Carl Maria von Weber and en face vaguely resembles Chopin; Brahms and Billroth look alike and there are portraits of Sir Richard Owen which resemble certain portraits of Richard Wagner. The theory of Mœbius and the points of resemblance in cranial contour and facial features are of course only matters of empirical observation, but at least as striking as the significance of a high brow, a prominent or receding chin, high cheekbones or deep sunken eyes." Garrison dwells very extensively on the correspondence between Billroth and Brahms which rivals in historic interest the correspondence between Schiller and Goethe, or between Wagner and Liszt. In one of these letters Billroth asserts that Beethoven could not write for the chorus except ineffectually which must surprise any Beethoven expert or anyone who has ever heard the choruses in Beethoven's opera *Fidelio,* or the choral arrangement of the XIX Psalm, "The heavens declare the glory of God and the firmament showeth his handiwork." This exceptional opinion of Billroth's is explainable only on the supposition that he was not in a Beethoven humor when he heard the chorus which he criticized. I have known this to be the case with many of my musical friends. One must be in a specific Brahms mood to appreciate Brahms' music, and in a Chopin mood to appreciate or perform Chopin properly. In a letter dated Vienna, July 27, 1883, Billroth expresses great satisfaction that the house in which he is living at the time was once owned by the famous clinician Johann Peter Frank who is one of the pioneers of public hygiene, and in 1792 published a work entitled "System der Medizin. Polizei." And he also remarks that Beethoven came to this same house in which 100 years later Brahms and Billroth met.

In the year 1774 Goethe made the acquaintance of Lavater who was then editing his "Physiognomic Fragments" for which the great poet-naturalist developed an intense interest. He even collaborated on this work and described a number of heads of famous men. Lavater, in turn, emphasized the importance of the bony foundations of the face for the physiognomy, thus Goethe's early osteological studies began with the doctrine of facial expression and the problem how the character and type of human beings could be read from the physiognomy. It is fortunate that many of the doctrines of Lavater have been forgotten, for instance, the assertion that the eye of a man of genius has emanations and that rays of light are reflected from it in a manner peculiar to itself and there are thus stronger sensations produced in the observer than from the eyes of ordinary men. P. Mantegazza, "Physiognomy and Expression," had similar ideas. He states for example, "There are some expressions which only belong to genius and if we had a photometer which could measure the light issuing from the eye we might perhaps judge of the value of a picture or a poem, or of a book by the vividness of the light that gleams from the pupil of the artist. He calls the attention to the superior tonicity of the facial muscles of highly intelligent men as contrasted with the flaccidity of the countenances of ordinary commonplace persons. This is supposed to be the cause of the strength of the impressions made by great personalities upon their associates, the evident sense being that these great men are in some way set apart by fate as exceptional or predestined. It is said to make itself felt sometimes as a dominating power, as a magnetic emanation, sometimes as a mere charm of irresistible sweetness, subduing and enthralling the will of the observer. Charles J. Whitby (l. c. page 98) speaks of these facial expressions as something perfectly verifiable and real and speaks of them as being unaccountable genealogically.

It has been my good fortune to have been in the presence of a number of men of science whom I could call great: I knew Simon Newcomb, W. K. Brooks, Virchow, and Ehrlich; I heard Pasteur speak; I knew many great composers and musical artists as well as directors of orchestras. Whilst the facial expressions of these men are not of the general type of humanity, and whilst they do not use the muscles of the face in the same way as ordinary mortals do, yet I fail to recognize anything so inexplicably magical as Lavater, Mantegazza, or Whitby make out. I doubt very much whether the incalculable element called genius can be revealed by look or gesture and incline to the belief that the ecstatic, sometimes even autohypnotic descriptions given by some authors as the effect of facial expressions of great men is largely subjective and describes what these writers imagine rather than what exists in reality; nevertheless, there is a faint resemblance between the facial types of certain eminent men of different abilities, in the sciences or arts.

Although no such contribution to localization as that of Broca has been made by Billroth in his psycho-physiological aphorisms on music, nevertheless his work will prove of value because of the general clearing of the entire subject of physical and physiological acoustics and by rendering the work of Helmholtz more accessible and more intelligible to musical artists and connoisseurs so that they may hereafter become conversant with the scientific aspect of their art. Above all, by pointing out the gaps and breaks in the experimental and practical logic he has emphasized the vacant spots that must be filled before we may hope to reach more tangible results concerning the purely psychological aspects of music.

X

HISTORY OF THE CLINICAL RECOGNITION OF GASTRIC ULCER[1]

SUCCINCT histories of the development of clinical conceptions on special diseases are very rare. This is partially explicable by the scarcity of historical data concerning early clinical thinking and by the confusion that has been brought into the accounts of the classical writers of medicine by the facts that the identical disease is designated with different names by different authors, and also that the exact diagnosis was rarely verified by operation or autopsy.

In the following sketch of the history of the clinical recognition of gastric ulcer, conclusions were based only on such cases as were substantiated in the manner just mentioned. In the writing of Hippocrates we find no statement which would justify us in concluding that the "Father of Medicine" was familiar with ulcer of the stomach as we conceive it to-day.

It is stated (Lebert) that Hippocrates treated Pericles for intense abdominal pain (gastralgia) by hellebore and flaxseed poultices.[2] I shall return to the Hippocratic literature on this subject in the latter part of this article.

A careful review of a German translation of Galen reveals no knowledge of peptic ulcer, though undoubtedly both Hippocrates and Galen give accounts of vomiting of blood in the sequence of digestive disturbances and abdominal pain.

It is impossible to personally scrutinize the writings of the historical clinicians for any statement that might be interpreted as a distinct clinical recognition of gastric ulcer. According to Lebert,[3] from whom also Welch[4] has derived most of his information concerning the history of clinical recognition of peptic ulcer, it is not until the sixteenth century that a clinical record available for our purpose can be found. It occurs in the writings of Johann Bauhin, the elder brother of Caspar Bauhin (1550–1624), and may be found in "Boneti Spepulchretum." It refers to a physician's wife, 18 years old, who suddenly showed symptoms of a perforation into the peritoneum, and died four days afterwards. In her abdomen was discovered besides gas, fluids, and food remnants, a perforation in the middle of the stomach near the fundus; there were black contents in the stomach and clotted blood had been evacuated by the bowels.

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[1] Address before the American Gastro-Enterologic Association Atlantic City, June, 1904. *Journal of the American Medical Association,* January 7, 1905.

[2] I have seen this statement quoted also by Talmage in an address to medical graduates, but have not been able to trace the source nor authority for same.

[3] Krankheiten d. Magens, Tübingen, 1877, p. 180.

[4] Pepper's System of Practical Medicine, vol. ii.

From the second half of the sixteenth century dates another observation by Marcellus Donatus, which refers to a man 59 years old, who, after continued vomiting and pains in the stomach, was found at the postmortem examination to have near the pylorus at the lower end of the stomach an ulcer—*tunicam interiorem exesam*—a very graphic descriptive expression.

Courtial describes an oval opening in the anterior part of the stomach, with callous edges in a young woman who had suffered from gastric symptoms since her youth.

Littré found in the case of a man who had suffered from various affections of the stomach, and finally from hemorrhages, a round ulcer near the pylorus.

One of the first ulcers leading to a fistula in the stomach which is recorded in the literature so far known, seems to be associated with a case of Duverney. Similar observations were described in the first half of the previous century by van Swieten, Klein, Wenker, Iodac, Atkinson, Petit, Eckmüller, Cireaud, Helm, and Kade. It is not, however, quite certain to what the trouble in the various cases might rightly have been attributed, whether to the presence of a simple ulcer, or to a circumscribed abscess of the stomach region or a combination of both. The probabilities are, however, that the observation of van Swieten and Wenker were genuine cases of peptic ulcer.

Records of cases of healed ulcer of the stomach we already possess for the sixteenth and seventeenth centuries, by Forestus, and Schenk von Grafenberg (1531 to 1598). In this connection should be mentioned an observation from the close of the preceding century by Reil, in which the scar of a healed gastric ulcer had become adherent to the mesenteric glands. The radiant formation of such a scar at the lesser curvature of the stomach has been aptly described according to a specimen in Meckel's collection. Older observations concerning abnormal conditions of the form of the stomach, with median contraction, by Blase, Mangold, Heister, Sandifort, Sömmering, Morgagni, etc., allude in a measure to such ulcers. In a number of cases symptoms of chronic stomach trouble existed beforehand, and frequently mention is made of callous bands, which would point to scars of earlier existing ulcers. In the last century mention is often made of adhesions of the stomach to adjacent parts (Mead, Murry, Santessen, Westring, etc.) of rupture of the stomach (principally through an ulcer), of death from excessive hemorrhage, also mainly attributable to a chronic peptic ulcer.

A good brief description, with excellent illustrations, was given at the close of the eighteenth century (in 1793) by Matthew Baillie. To Matthew Baillie unquestionably belongs the credit of having first accurately described, in 1793, the anatomic peculiarities of simple gastric ulcer.[5] At a later date he published three good engravings of this disease.[6] Baillie's concise and admirable description of the morbid anatomy of gastric ulcer was accompanied by clinical data, and seems to have had little or no influence in directing increased attention to this disease.

By far the best description of this disease up to the date of its publication is by Voigtel, at the beginning of the eighteenth century. The following description is quoted from Lebert: "Frequently ulcers are found in the membranes of the stomach. Oftener they progress from the interior to the exterior; they are merely superficial cancers or perforations. They have in common a peculiar appearance. Frequently they are surrounded by an inflammatory thickening and hardness, and then again it is not possible to detect near or around them any distinctive obnormal formation of the stomach. They appear as if 'a piece of the stomach had been cut out with a knife,' and the edges so healed as to make a smooth boundary all around the opening; at times the gastric tissue about the opening has condensed." The subsequent remarks point likewise to an admirable knowledge of the whole evolution of the disease on the part of Voigtel and his contemporaries, Meikel, Reil, etc. The words used by Voigtel in describing the appearance of this gastric lesion, namely: "As if a piece of the stomach had been cut out with a knife," remind one of the expression of Rokitansky[7] used much later, and which is much oftener quoted, namely: "As if a piece of the stomach had been cut out with a punch."

Many observations, accompanied by diverse commentaries, are found in the early scientific literature concerning spontaneous perforations of the stomach, the greater part of which may rightly be classed here.

Toward the end of the eighteenth century we encounter Hunter's theory of the origin of gastric perforations, attributing them to the erosion by the gastric juice; although based on the authority of Spallanzani, this hypothesis lost ground very rapidly, notwithstanding the support given it by a number of important authorities. Carswell managed to bring it again into transitory favor. In the year 1800 Joseph Morin stated that perforation of the stomach was a result of a protracted local inflammation of the gastric membranes. He likewise speaks of stagnation in the blood vessels, with a change in the direction of the flow of the blood and auto-digestion, as some of the causes. According to the very pretty historic compilation of Lefevre, the clinicians Lerons and Chaussier, in the year 1808, presented to the Paris Medical Association reports of their observations of perforated gastric ulcers accompanied by adhesions to the spleen.

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[5] The Morbid Anatomy of the Most Important Parts of the Human Body, London, 1893, p. 87.

[6] A Series of Engravings, accompanied with explanations, etc., London, 1799.

[7] Lehrbuch d. Pathol. Anatomie.

Very interesting history also is found in the defense of a person by Chaussier against the charge of poisoning, in 1818, when the latter proved that a perforation of the stomach had taken place after previous prolonged ulceration. Similar cases have come up frequently since that time. Cruveilhier relates that there was a movement on foot to attribute the death of one of his cases of chronic gastric ulcer to poisoning, when he proved that the true cause of the perforation in question had been a previously existing ulcer in the stomach. The dissertation of 1819 by Lainé offers for those times very sound opinions about this perforation, and describes the deleterious action of the digestive fluid as succeeding ulceration. He also emphasizes the importance of studying the antecedents of a case. The expression "perforation by erosion" from now on appears more frequently. Lainé also recommends, for forensic reasons, not only chemical inquiry, but also an examination of those parts of the anatomy through which the alleged poison is supposed to have passed into the stomach.

Rausch, in his classification of gastric ruptures, designates those incident to a real ulcer, often with callous edges, as one of the main groups. Similar results are reached by Ebermaier, who, in addition to his own, cites cases by Henkel, Mole, Baron, Gérard, Desgranges, Trinius, Besker, Spitta, and Thomissen. To all these cases he attributes a chronic, many times unknown course, frequently with easily discernible symptoms, especially cardiac symptoms. The frequency with which the rupture takes place near the pylorus is pointed out by him, but he seems to be unable to determine the cause of this phenomenon. Cancer and ordinary inflammation, however, are not regarded as having causal significance.

In 1824, Abercrombie published a brief, but for his time a very remarkable description of this disease. Some of his observations clearly refer to the ulcus simplex; but one of the cases reported to him by Wood refers to a perforated ulcer of the duodenum. His confusion of cases of gastric cancer and other malignant growths with those of simple ulcer, shows how undefined his views were.

We now have become acquainted with two phases of the history of chronic ulcer—the first being one of the isolated observations beginning with the tenth century, the second beginning with the close of the eighteenth and with the early part of the nineteenth century, and occupying itself with the establishing of a collection of those isolated observations as done by Voigtel, but emphasizing more of the pathologic anatomic point of view; on the clinical side, too, much attention is paid to the perforation itself.

Cruveilhier has the distinction of being the first to subject this disease both clinically as well as anatomically to a thorough and intelligent pathologic examination. The tenth issue of his great *"Pathological Anatomy,"* from the beginning of the third decade of the nineteenth century, 1829–1835, gives evidence of an extensive personal experience of this disease.

Cruveilhier[8] in the first volume of his work on pathological anatomy, for the first time clearly distinguished ulcer of the stomach from cancer of the stomach and from ordinary gastritis. He gave an authoritative and full description of gastric ulcer from the anatomic, the clinical, and the pathologic points of view.

Among his observations are those concerning the Greek professor, Gall, from the end of the year 1828 and beginning of the next; one from December, 1829, about a charcoal burner, who died soon after the perforation of his stomach; one from the beginning of the year 1830, concerning the famous chemist, Darcet; one from the same year concerning a gastric ulcer which had cicatrized—and with respect to this case we learn from his twentieth issue that the person in question died in 1834 from a perforated stomach. Not only are the anatomic aspects—i. e., rapid development, chronic ulcer, closing up, scarring over, perforation, hemorrhage from the coronary artery of the stomach, from the splenic artery, malformation of the stomach, contraction of the pylorus through the agency of scarring—well described, but also a good foundation is laid for clinical and therapeutic work.

We thus emerge into the modern phase, in which a correct and broad interpretation of anatomic and clinical facts of the influence of hygiene, diet, and therapeutics takes place, and thus order and understanding are introduced into this chaos of observations.

In Germany we are indebted to Rokitansky for a classical treatise on the peptic ulcer, based on a large clinical and pathological experience, well analyzed. This excellent work became rapidly known.

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[8] Anatomie pathologique du Corps humain, vol. i, Paris, 1829–35, No. 10 and No. 20; and vol. ii, Paris, 1835–42, No. 30 and No. 31.

Next to Cruveilhier, Rokitansky has had the greatest influence on the modern conception of gastric ulcer. In 1839 this pathologist gave a description of the disease based on an analysis of 79 cases.[9] The anatomic part of his description has served as a model for all subsequent writers on this subject everywhere. A few years later von Jaksch extended the pathologic knowledge of the gastric ulcer (to be sure, more from his clinical experience than from a strict analysis of bedside records), by a series of thorough clinical investigations.

From 1830 to 1840 observations and compilations appear frequently in the medical literature of almost all countries. According to Lebert,[3] rich collections for causal interpretation are found in the *Bulletins de la Société anatomique de Paris,* also in *Archives générales de Médicine,* in the "Annual Reports" of the last 20 years of Canstatt,[10] the Proceedings of the London Pathological Society, etc. Among the older journal articles and dissertations that have appeared is the Thesis of Duval in the year 1852 (Paris), one of the earliest of the better French compilations. Lebert [3] also refers to the publications of one of his pupils, Bisseger, as containing his own views often expressed in the Zürich clinic, and also an analysis of its rich material, together with clinical histories.

After Virchow, in 1855, developed his views on the hemorrhagic necrotic origin of the simple peptic ulcer, a series of good dissertations appeared under his editorial supervision, among which especial mention should be made of those by Steiner, 1868, and Wollman, 1868. Among the English authors, Crisp, Ashborne, Chambers, Habershon, Handfield-Jones, etc., but especially Brinton, have done excellent work. From the latter emanated the largest compilation, with excellent anatomical and clinical data. Habershon has also made valuable clinical additions to our knowledge of this disease.

From the beginning of scientific inquiry, cited in the preceding, to the year 1868, there is, as we have seen, a vast accumulation of clinical data, but out of all this literature, according to Lebert, not 100 are of genuine objective merit.

Cruveilhier's work of 1856, made known in the *Archives Générales de Medicine,* contains for those times and for the present no new matter beyond what was presented in his previous publications, but impresses us with the great anatomic and clinical experience of this wonderful pathologist.

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[9] Oestereich. Med. Jahrb., 1839, vol. xviii (abstract in Schmidt's Jahrb., vol. xxv, p. 40).

[10] Canstatt's Jahresberichte.

Trousseau, in his *Clinique Médicale,* gives a review of the essentials, which acts in a stimulating manner on medical thinkers, but is, of course, incomplete, which is to be expected of clinical lectures, however important they otherwise may be.

Bamberger's description is an excellent sketch of the subject. The work of Ziemssen (in his Encyclopedia, Ziemssen's *Handbuch d. spec. Path. u. Therap.*), on the treatment of chronic gastric ulcer, represents the best knowledge of the Münich clinic at that period.

As early as 1845 Schiff showed that hemorrhagic localized infiltrations of the mucous membrane of the stomach may result from an irritation of certain parts of the brain. After a number of other experiments, he published the results of his investigations in his *"Leçon sur la digestion."* In 1852 Lebert became acquainted for the first time with the hemorrhagic type of growth of chronic gastric ulcer. In 1855 Virchow made known his views on this matter, and brought into general favor the acceptance of a hemorrhagic necrotic origin of the peptic ulcer. Müller has the distinction of having proved, in 1859, that the ligation of the portal vein results in numerous hemorrhagic erosions of the stomach, particularly in the pyloric region. Pavy later on showed the same thing in the case of larger ulcers through ligation of branches of the gastric artery. For the best and most exhaustive experiments on the circumscribed hemorrhagic infiltrates in the mucous membrane of the stomach, we are indebted to Ebstein.

Since the ushering in by Cruveilhier and Rokitansky of the modern era in the history of the gastric ulcer, the era of exact objective investigation, medical literature abounds in articles on this disease. But it can not be said that the importance of these works is at all commensurate with their number or that they have added very materially to the classical descriptions given by Cruveilhier and by Rokitansky. Perhaps most worthy of mention of the works of this later era are the articles by von Jaksch relating to symptomatology and diagnosis, and of Virchow relating to etiology, the statistical analyses by Brinton, and the contribution to the treatment of the disease by Ziemssen and by Leube.[11] In 1860, Ludwig Müller[12] published an extensive monograph on gastric ulcer. The most modern literature on the subject is contained in the works of Riegel.[13] Prof. Wm. H. Welch[14] gives a short résumé of the history of the clinical recognition and pathology of gastric ulcer, taken largely from Lebert. In the new *Handbuch der Geschichte der Medicin,* by Neuburger and Pagel,[15] the history of gastric ulcer is treated rather neglectfully. It seems that the author, George Korn, had no knowledge of the interesting studies of Abercrombrie, Voigtel, Littré, Matthew Baillie, Cruveilhier, and the other clinical and pathologic workers mentioned in the preceding. The article is interesting for the histology and pathology of digestion, the history of the stomach tube, etc., but for the history of the development of clinical ideas on any single gastric disease, little help can be obtained from this otherwise very interesting article. On page 693 of this article, Korn asserts that Hippocrates already described some of the symptoms of gastric ulcer as a special disease which was called *sphacelous disease,* or *morbus niger,* because black masses in the form of clumps were either vomited or passed by the bowels. I have looked up this passage in Hippocrates[16] and, while there is great temptation to credit the "Father of Medical Literature" with the first trustworthy knowledge of gastric ulcer, I regret to report that his account is not even approximately correct for this disease, but would cover that also of gastric carcinoma and even of secondary gastric hemorrhage, due to cirrhosis of the liver. Some have thought the dietetic treatment which Hippocrates recommended for his *morbus niger* gave evidence that he recognized it to be the disease we know as gastric or peptic ulcer. But even this hope is destroyed by a critical investigation of the diet of Hippocrates for this purpose. In 1899 Rudolph Bandel, on the incentive of Prof. R. Fleischer, of Erlangen, investigated the writings of Hippocrates for his knowledge on dietetics. It is published in a small pamphlet entitled *Ansichten u. Ausprüche des Hippokrates über die Ernährung vom Gesunden und Kranken,* etc. (Inaugural dissertation, Erlangen.) In this publication we find nothing to confirm the supposition that the diet of Hippocrates gave evidence of his knowledge of gastric ulcer. The term which Hippocrates gives to the vomited masses in *morbus niger* is *atra bilis* (black bile), which expression maintained itself in medical literature until 1740, when F. R. Hoffmann demonstrated that the seat of *morbus niger* is the stomach. His words are as follows: "Simultaneously with the destruction of the substance of the stomach, the vessels are corroded or broken open, and these open vessels furnish the blood which is expelled by vomiting. We have to assume that the vessels are corroded by sour and acid juices, when a sharp gastric pain preceeds the vomiting, and when the vomited bloody masses are black and acid at the same time, make the teeth stand on edge, and irritate the mouth and gullet."

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[11] Jaksch: Prager Vierteljahrschr., vol. iii, 1844; Virchow: Arch. f. path. Anat., vol. v, p. 362, 1853; and A. Beer: "Aus dem. path. anatom., Curs des Prof. R. Virchow in Berlin, "Das einfache duodenische (corrosive) Magensgeschwür," Wiener med. Woch., Nos. 26, 27, 1857; Brinton: On the Pathology, Symptoms and Treatment of Ulcer of the Stomach, London, 1857; V. Ziemssen: Volkmann's Samml. klin. Vorträge, No. 15, 1871; Leube: Ziemssen's Handb. d. spec. Path. u. Therap., vol. vii, Leipzig, 1878.

[12] Das corrosive Geschwür im Magen und Darmkanal, Erlangen, 1860.

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[13] Erkrankungen des Magens. In the fourth volume of Penzoldt u. Stintzing's Handbuch d. specielle Therapie, p. 316, also pp. 437 and 438 are contained 150 bibliographical historical references on gastric ulcer.

[14] Practical System of Medicine, by American authors (edited by Pepper), vol. ii, p. 480.

[15] Vol. ii, p. 666.

[16] Sydenham Society edition.

Morgagni (1682–1771), a contemporary of the prominent Italian anatomists, a pupil of Valsalva and Albertini, and a friend of Lancissi, in his book on *"The Seat and the Causation of Diseases,"* does not agree with the view of F. R. Hoffmann just expressed, but holds that patients who died of *morbus niger* were suffering from a kind of gangrene of the stomach; that they did not die of loss of blood, but of a kind of poisoning of the blood, which infected the brain. It is interesting to note here that Morgagni described in this work the condition known as "continuous gastric secretion" (gastric succorrhea) occurring in a German nobleman in Bologna, who died later with symptoms of gastric tetany.

The designation *ulcus ventriculi* was first used by the versatile Jena clinician, Johann Peter Frank (1745 to 1821). It is regrettable that the writings of this scholarly asclepiad are being gradually forgotten, for the modern treatment of gastric ulcer dates from him. His insistence on absolute rest in bed, the application of snow or crushed ice to the stomach, the great regard for dieting (milk and bouillon in small portions), all show exceptional clinical experience. The best that is known on gastric ulcer and gastric diseases in general up to 1850 is presented in the work of William Brinton. Since the descriptions of Cruveilhier, Rokitansky, and Virchow, the pathologic conception and the clinical conception of the gastric ulcer have received no notable advance. The further students of this disease, Trousseau, Kussmaul, Ziemssen, Leube, C. A. Ewald, von Mering, Fleiner, Boas, Rosenheim, Riegel, Penzoldt, furnished clinical elaborations purely. But G. Hauser—1883 and 1920 presented an admirable pathologic study on transition of gastric ulcer into cancer.

It might be mentioned before concluding, that the first surgical intervention for gastric ulcer was undertaken in 1881 by Rydygier. For further history of gastric surgery and bibliography of gastric ulcer, see Hemmeter.[17]

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[17] Diseases of the Stomach, third edition, p. 348. For history and literature of gastric ulcer, see same work, pp. 486–526, containing 248 literary references on the subject; also Hemmeter, N. Y. Med Rec. July 3, 1915.

XI

THE HISTORY OF THE CIRCULATION OF THE BLOOD

CONTRIBUTIONS OF THE ITALIAN ANATOMISTS AND PHYSIOLOGISTS—THEIR BEARING UPON THE DISCOVERY BY HARVEY[1]

A VERY curious impression is made upon the objective investigator to see the scientific judgment obscured by ill-guided patriotism. No less than four monuments have been erected in three different countries to perpetuate the memory of four different discoverers of the circulation of the blood. The Spaniards regard Michael Servetus, born in Villanueva in 1511, as the discoverer of the circulation, and have erected a monument in the Museo Anthropologico at Madrid. The Italians have three men to whom the title to this discovery is accredited with more or less historic and scientific correctness, namely: Matheus Realdus Colombus, born at Cremona in 1516; Carlo Ruini, of Bologna;[2] Andreas Cesalpinus, born at Arrago in 1519. Monuments have been erected to Carlo Ruini at Bologna and to Andreas Cesalpinus at Pisa and Rome. William Harvey has been honored by a monument in London erected by the Sydenham Society, a second at Hemstead, and a third at Folkstone. This makes five discoverers in all, but there is one more. The French have claimed the discovery of the circulation of the blood, and their discoverer is that incomparable satyric, François Rabelais, and this assertion is made by no less than Paquelin.[3] But a critical study of the passages from Rabelais, which Paquelin cites, proves that the alleged French discoverer brings nothing new, nothing which was not already stated by Galen, that he nowhere mentions dissections or vivisections as a basis for his claims.

The department of physiology which was most fatally retarded in its progress by a defective knowledge of the ancients is that concerning the circulation of the blood. In the Lane lectures, delivered at Cooper Medical College, San Francisco, in 1900, Sir Michael Foster has given us an admirable review of the history of the circulation of the blood. These lectures constitute a most scholarly and inspiring addition to our knowledge of the subject. The first and second chapters of this book treat of the circulation, especially the second chapter. The first chapter does not treat of the circulation directly, but of Vesalius, his forerunners and followers. In narrating the contributions of the Italian physiologists and anatomists, there is this difference between the tenor and spirit of presentation by Foster, and that by Spanish, and Italian medical historians. Foster leaves us under the impression that the Italian physiologists and anatomists made no contributions or discoveries of enduring excellence or genuine scientific merit, whereas the Italian historians[4] make every effort to convince us that the work of their fellow-countrymen constituted solid building blocks in the architecture of physiology; that their conclusions were based upon precise and accurate observations and experiments, as far as such were possible in those days. In order to judge with what justification the claims of the Italian authors are made, and also to judge of the critical conservatism and broad experience of Sir Michael Foster, a brief and critical review of the main contributions of the more renowned Italian workers in physiology and anatomy, and of Harvey, becomes indispensable.

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[1] *Johns Hopkins Hospital Bulletin,* Vol. XVI, May, 1905.

[2] Book on Anatomy and Diseases of the Horse, published in 1598, in which Ruini gives the unmistakable evidence that he had grasped the action of the cardiac valves.

[3] Revue de Litér médic, 1878, p. 499.

In the *Deutsche Rundschau*, Helmholtz[5] credits poetic genius with the power of reviving long-past historic personages and transactions, and clothing historic characters not only with flesh and bone and garments, but with exactly portraying their psychic individuality and real personality. At the same time there is also an exact probing of the plausibility of the existence of certain characters. We can see this in the Talmud connoisseurs, some of whom, of Jewish creed, have denied that Job and Jonah ever existed, and upheld other characters of the Old Testament as real personages. One has to be a very thorough, patient, persistent, and judicial student of any great historic character in order to successfully think oneself into the life and work and individuality of the person.

This is true in the study of the lives of Michael Servetus, Vesalius, Harvey, and others; it is not sufficient to read their history,—one must repeatedly sink one's psychic self into the life and period of the personage to be studied. Medical history writing, unless the writer and student has sunk his soul into the soul of the character to be studied, unless he has become part and parcel of the life and work, individuality, and even the social, scientifiic, religious, and political condition under which that character developed and produced his results, has little value for an analytical mind.

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[4] See Luigi Luciani, Physiology of Man, translated into German by Baglioni and Winterstein: § G. Ceradini. Ricerche storico-critiche intorno alla scoperta della circolazione del sangue, Milano, Fratelli Rechiedei, editori, 1876. Difesa della mia Memoria intorno alla scoperta della circolazione, contro l'assalto dei signori H. Toll in teologo in Magdeburg, e W. Preyer fisiologio in Jena. Con qualche nuovo appunto circa la storia della scoperta Medesima. Genova, tip del R. Instituto Sordomuti, 1876.

[5] Goethe's Wissenschaftliche Thätigkeit, also Helmholtz Vorträge BI s 34.

In order to present a medical character justly and as correctly as possible, it is necessary to become a connoisseur of that character, and all the conditions influencing and surrounding him. We thus are even now able to recognize specialists on Galen, Vesalius, Servetus, Harvey, just as there are music lovers who are specialists of the compositions of Rameau, Paganini, Bach, Mozart, Beethoven, Chopin, etc. Musical notes as printed are only the skeleton of music. Note-reading and mechanical technique will never make an artistic player. "He is not a correct interpreter of a composer until he puts his soul into it."

This identification of the subjective powers of an artist with the work and musical nature of the composer, resurrects the composer before the mind's ear and eye of the performing artist. He begins to realize the subjective and objective peculiarities and becomes familiar with the train of thought and musical feeling that prompted the composition.

Similarly the mere reading of biographies and personal histories of distinguished anatomists and physiologists, without a calm, self-possessed, and patient analysis of all the collateral sources of information, will not give a picture of life-like distinctness. It is like playing the notes of a composer mechanically in the absence of a wide reach of innate musical gifts. What is needed in medical biography as well as in the reproduction of classical music is the focusing of the entire mental energy of the writer upon the historic or musical problems in hand,—reading the literature in its original sources, and not from abstracts or quotations second-hand, and eventually, if possible, to combine the facts and data from all sources of information into an organic life-like whole, making the character and his times comprehensible to our modern methods of thinking.

The history of the discovery of the circulation of the blood begins with Galen (125–201 A. D.), who by his vivisections exposed the error of the Alexandrine school under Erasistratus (300 B. C.), who held that the left portion of the heart and the arteries were empty and that they, communicating by means of the small bronchi with the arteria aspera (trachea), served to convey the spirit of life (pneuma) to the various parts of the body, to animate them and that the veins alone contained blood with which to nourish the entire body.

Galen showed that one need only to prick any artery or the left portion of the heart of a living mammal, in order to see blood gushing forth, which, in contrast with venous blood, was vaporous, thin, and "genuine," and which therefore consisted of a mixture of blood with the air inhaled in the lungs: "mixtum quid ex ambobus."

According to Galen the left side of the heart is the center of the arteries, which through systole drives the *air-containing* (ærated?) blood (sanguis spirituosus) into all the organs, to animate them. The center of the veins on the other hand is the liver, from which the nourishing blood (sanguis nutritivus) is carried to all parts of the body by a kind of attractive and selective force. The blood from the right side of the heart, from the cava inferior, goes for the most part into the left ventricle, through the pores of the septum (which Galen assumes, although he declares them to be invisible). In the left ventricle it becomes "spirituous" through mixture with pneuma, and through the aorta it is distributed over the entire body. However, a small portion of the blood contained in the right ventricle passes through the vena arteriosa (arteria pulmonaris) and then by way of the arteria venosa (venæ pulmonales) reaches the left ventricle.

Galen therefore had a notion, although imperfect, of the circulation of the blood through the lungs, and he knew that the venous vessels anastomose with the arterial ones, since he also had observed, that an animal might bleed to death from a single artery. One point, however, in Galen's theory might lead some critics astray in the explanation of the text. Galen assumed that the blood of the arteria venosa (venæ pulmonales) flowed back into the lungs at each systole (through a sort of physiological insufficiency of the bicuspid or mitral valve), in order to exhale here the "soot" formed in the blood. Thus he assigned to the arteria venosa a double, and that an opposite, function, that is to say, first of conveying the reanimated blood from the lungs to the heart, and then again of conveying a portion of the same with the "soot"[6] from the heart to the lungs. Similarly Galen assigned to the vena portæ a double function, in that he assumed that during digestion it conveyed the chyle to the liver, and when the intestinal canal is empty, it conveys the blood from the liver to the intestinal canal.

Certainly the two errors of the permeability of the septum and the systolic reflux dim, not a little, the splendor of Galen's theory of the lesser circulation; nevertheless it cannot be denied that Galen was the first to have a conception of the same, a fact which was recognized (long before G. Ceradini again drew the attention of investigators to it) by representative interpreters such as Harvey, Haller, Douglas, Maurocordato, and Senac, who referred especially to a passage in the 10th chapter of Book VI, "De usu partium," in crediting Galen with a conception of the respiratory circulation.

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[6] It is absurd to believe with some interpreters of Galen that he really meant "soot." We are dealing here with one of those orthologic difficulties in that the Greek word which now means "soot" originally meant something entirely different and probably more in harmony with the opinions concerning the nature of venous blood of to-day.

Let us now see who first corrected and completed the theory of Galen, by the rejection of the assumption of the permeability of the cardiac septum, and by the determination of the fact that not only a part, but all the blood driven out from the right ventricle reaches the left ventricle by anastomoses of the pulmonary vessels.

In the year 1553 the Spanish physician and theologian Michele Reves (better known by the anagram of Serveto or Servetus) published his book entitled, "Christianismi restitutio," which at the instance of Calvin resulted in the author's condemnation and death at the stake, in October of the same year, at Geneva. In this theological book (there are only two copies extant, since the greater number were burned, first at Vienne (Dauphiné) together with the effigy of the author, and then in Geneva simultaneously with the author [7]), there is a passage in which Serveto describes the lesser circulation, at the same time denying the communication of the two ventricles through the septum, and assuming that the blood from the right ventricle goes to the lungs where "flavus efficitur, et a vena arteriosa (arteria pulmonaris) in arteriam venosam (venæ pulmonales) transfunditur" (it is made brighter (yellow) and is tranfused from the vena arteriosa (arteria pulmonis) into the arteriam venosam (venæ pulmonales)).

In the year 1559, that is to say six years later, Realdo Colombo, of Cremona, who for fifteen years had been prosector and then successor of Vesalius in the chair of anatomy at Padua, published at Venice his work entitled, "De re anatomica, libri XV," in which, on page 177, there is a description of the lesser circulation and a confirmation of the impermeability of the cardiac septum. The author lays great stress upon this discovery and claims the priority of it: "Nam sanguis per arteriosam venam ad pulmonem fertur, ibique attenuatur; deinde cum ære per arteriam venalem ad sinistrum cordis ventriculum defertur; quod nemo hactenus aut animadvertit, aut scriptum reliquit." (For the blood is carried through the *"arteriosam venam"* to the lungs, and there it is attenuated, thence (mixed) with air, it is carried through the "arteriam venalem" to the left ventricle of the heart; *which no one up to this time has either observed, or left it in writing.*)

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[7] Henri Tollin, l. c., presents convincing reasons for believing that numerous additional copies must have been in existence during Harvey's time of Servetus' "Christianismi restitutio" and that it would be incredible to assume that such an omnivorous reader like Harvey was not familiar with the physiologic contents of this book.

It cannot be denied, that, if we consider solely the dates of the two publications, the priority of the discovery belongs to Servetus, and if one could prove (as Tollin and Preyer in Germany and Willis in England have attempted to do) that Colombo had read the "Christianismi restitutio" of Servetus, then the Cremona anatomist would have to be declared guilty of plagiarism. But this is refuted by a number of incontrovertible facts, which G. Ceradini (1876–77) arranged with great acumen.

Ceradini emphasized that Valverde, a Spanish pupil of Colombo, in a text-book on anatomy published at Rome in the year 1556, *ascribed the theory of the impermeability of the septum to his teacher.* The book is preceded by an introduction dated 1554, in which the author states that he had already caused to be made numerous plates for the embellishment of his book, which must have taken at least a year. Thus *we reach the year 1553,* in which Servetus published the book which cost him his life. The assumption seems justified, therefore, that Colombo had already for some years taught his theory in his lectures before he published it in his textbook.

It has been demonstrated that the passage in the "Christianismi restitutio" treating of physiology was only discovered towards the end of the seventeenth century. Ceradini further calls attention to the fact that in the year 1571 G. Günther, who, as the professor of anatomy at Paris, had been the teacher of Servetus and Vesalius, in describing the lesser circulation in the words of Colombo, *lauded the latter, without mentioning his own pupil Servetus,* a proof that he did not know the "Christianismi restitutio." It is also probable that it was unknown in Italy, since it was not on the "Index librorum prohibitorum" edited by the Council of Trent and published at Rome under Pius IV in the year 1564, whereas this does contain two other heretical works of Servetus, "De trinitatis erroribus."

Finally Ceradini brought forward a splendid proof of the fact that Colombo had not plagiarized Servetus, in the comparison of the two theories.

Colombo denied the permeability of the cardiac septum completely and unconditionally; he emphasized the fact that not only the vena arteriosa, but also the arteria venosa was of considerable size; he further denied—even if incorrectly—the Galenian function of breathing, that is to say the formation of "soot" in the blood and the expulsion of the same by exhalation. Servetus, on the other hand, denied to be sure the existence of openings in the septum, but admitted that through it "aliquid resudare possit" (something could be expelled), and upheld the Galenian theory by the assumption that the blood "in ipsa arteria venosa inspirato ære miscetur, et exspiratione a fuligine expurgatur" (in the arteria venosa itself is mixed with inspired air, and is cleansed by expiration from the "soot").

This question is a family quarrel among the Italian and Spanish anatomists and physiologists themselves, hence the sentiment due to national bias cannot be excluded in the cônclusion of Luciani (professor of physiology at Rome), who says: "We will not go so far as to consider the hypothesis of Ceradini proved, that Servetus took the theory of the lesser circulation from Colombo, and tried to harmonize it with the old theory of Galen; but there can hardly be any doubt that the Cremona anatomist (Colombo) taught his theory for some time before it was published by the Spanish physician and theologian (Servetus)."

It is also of interest to determine to what extent Vesalius of Brussels was concerned in this theory, the great founder of modern anatomy, to whom Flourens (1857) ascribed the priority of the theory of the impermeability of the septum, while Henri Tollin (1884) accused him of plagiarizing Servetus, in which opinion Tigerstedt (1893) concurred.

In a recent publication [8] von Jackschath asserts that Vesalius is guilty of "plagiarism" and to have taken his anatomy—the *Fabrica* (1543) from Leonardo da Vinci.[9]

In the first edition of his great work, "De humani corporis fabrica" (1543), Vesalius says that he is forced to admire the art of the master, who through invisible pores caused the blood to penetrate from the right into the left ventricle. In the second edition of the same work, which appeared in 1555, this expression of admiration for the creator is lacking, and the author declares that he cannot understand how "per septi illius substantiam ex dextro ventriculo in sinistrum vel minimum quid sanguinis assumi possit" (he could assume any substance even any blood to pass through the septum out of the right ventricle into the left).[10]

This more correct point of view, according to Tollin, was obtained by Vesalius from the "Christianismi restitutio" which Servetus had published two years previously, in 1553. It seems likely that Vesalius was using irony or sarcasm in the above expression, for he later on admitted that "*he accommodated his statements to the dogmas of Galen.*"

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[8] Wien. med. Blätter, No. 46, 1902.

[9] See also Münchener med. Wochenschr., Mai 3, 1904, p. 821.

[10] For fuller English translation, see Sir Michael Foster, Lect. Hist. Physiol., p. 14.

But Ceradini proved convincingly through a long series of quotations from the above work of Vesalius and also from some of his smaller publications, that Vesalius had become acquainted with the impermeability of the septum at Padua in the year 1543 through his prosector Colombo, and that he had defended it at Pisa in the year 1543, without however expressly emphasizing the necessary physiological consequence of this, that is to say the theory of the lesser circulation and the necessity, already recognized by Galen, of anastomoses between the arteria venosa and the vena arteriosa; he wished to avoid giving possible glory to Colombo, against whom he always had a grudge, because the latter had apparently sought to incite the students at Padua against him.[11]

In the *Ergebnisse der Physiologie,* Jahrgang II (1903), Biophysik, R. Tigerstedt presents an article on the lesser circulation (p. 533) which begins with a chapter "Zur Geschichte des kleinen Kreislaufs." Tigerstedt here accepts the opinion of M. Roth ("Andreas Vesalius Bruxelliensis, 1514–1564, Berlin") that the lesser circulation was discovered by Colombo. The doubt which is thrown upon the anatomical knowledge of Servetus in this article is by no means substantiated by the contemporary literature, nor by the impressions gained from the writings of Servetus themselves. Tigerstedt, following M. Roth, asserts that Servetus does not present real but speculative anatomy—that his main desire is to bring about an accordance between the Bible and anatomy by compilation from preëxisting books, not from actual dissections. He denies that the recognition of the impermeability of the septum was original with Servetus, but ascribes this to Vesalius, a credit, which, as can be demonstrated from Vesalius' own "Fabrica," etc., stands on very feeble evidence. For it is on this point, "the impermeability of the septum," on which every student of anatomy must regret the uncertain language of Vesalius. Servetus, who had been prosector for Günter (Guinterius), professor of anatomy at Paris (Tollin), cannot be accused of having learnt his anatomy from books alone.

Without therefore trying to belittle the great services of Vesalius in the reformation of anatomy, one can nevertheless regard it as an established fact that he had no direct share in the discovery of the circulation of the blood. Indirectly, however, he aided in this by his refutation of numerous errors of Galen, especially the theory of the formation of blood by the liver. The fact that the lumen of the vena cava in the neighborhood of the heart is greater than at the liver, was for him a sufficient reason to return to Aristotle's theory of the formation of the blood by the heart, and to assume that not only the arteries but also the veins are offshoots from the heart.

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[11] See Michael Foster, l. c., and Henri Tollin, Colombo's Antheil an der Entdeckung d. Blutkreislaufs. Virch. Arch., Bd. 91, 1883.

In 1543 Vesalius was called by Cosimo I of Medici to become a professor at Pisa, where he determined to give a course in "amministrationes anatomicæ" on the errors of Galen. It is probable that among his auditors was numbered Andrea Cesalpino of Arezzo, at that time scarcely nineteen years of age, to whom belongs the fame of being the first to have recognized and demonstrated the general circulation of the blood.

In the year 1571 the physician and philosopher of Arezzo published his "Peripateticarum questionum libri quinque," in which he assumes, that in all parts of the body there is physiologically a constant transition of blood from the arteries to the veins by means of anastomoses, which he defines as "vasa in capillamenta resoluta"; the constant motion of the blood from the veins to the right side of the heart, from this to the lungs, from the lungs to the left side of the heart, and thence into the arteries he defines as "Circulatio." He was the first to recognize the arterial structure of the pulsating vessel which arises at the right ventricle, and which Galen had designated as "vena arteriosa," and the venous structure of the non-pulsating vein which was formerly designated as "arteria venosa." He recognized further that the blood in the arteries is under a much higher pressure than in the veins, and that at its transition from the former to the latter the capillary anastomoses offer a greater or less resistance according to the degree of their contraction or expansion.

Twelve years after the "Quæstiones peripateticæ" his books "De plantis" appeared which alone would suffice to win for him imperishable renown as the forerunner of Linnaeus. In this work also he confirms that "sanguinem per venas duci ad cor, et per arterias in universum corpus distribui" (that the blood is led through the veins to the heart, and is distributed by the arteries to the entire body).

In the year 1563 appeared Cesalpinus' work "Quæstionum medicarum libri II," in which he gave the experimental proofs of his theory. He observed that if in a living animal a vein is exposed and tied, and if soon thereafter an incision into the vein is made in the direction of the capillary, that the blood first appearing has a darker color, and that which flows out subsequently is lighter in appearance. From this observation he, with great acumen, deduced the physiological purpose of the anastomoses found in all organs between arteries and veins, in that he assumed: "venas cum arteriis adeo copulari osculis ut, vena secta, primum exeat sanguis venalis nigrior, deinde succedat arterialis flavior, ut plerumque contingit" (that the veins are so connected with the arterial mouths that

IERONYMVS FABRICIVS AB AQVAPEND. ANAT.
ANNO
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1616

Fabricius ab Aquapendente

the vein being divided, first exudes a darker venous blood, then succeeds the brighter arterial, so for the most part it happens).

A second experimental proof of the circulation he founded upon the fact that in any part of the body the tied veins swell between the ligature and the capillary origin, and not between the heart and the ligature, as ought to be the case according to Galen's conception: "intercepto enim meatu, non ultra datur progressus; tumor igitur venarum citro vinculum debuisset fieri" (for the course being blocked, no longer a flow is permitted; therefore the swelling of the veins ought to be made on this side of the ligature (i. e., between vein and capillaries)).

In spite of these brilliant experimental proofs of the theory of circulation, which Cesalpinus was the first to offer, the view was held by some, and among them the famous Haller, that he had indeed known the circulation of the blood but had only assumed it for the state of sleep, not during the awake state; they based their view upon a quite erroneous construction of a passage in which Cesalpinus assumes a certain reflux of the blood from the arteries towards the heart during the state of being awake. No one has better, and with more cogent logic, reduced *ad absurdum* the assertion of Haller than has Ceradini. It is a pity that Ch. Richet in his "Dictionnaire de Physiologie," repeats the erroneous opinions of Haller concerning Cesalpinus, which had already been refuted through the historical-critical studies of Ceradini; which Ch. Richet certainly cannot have taken into account.

H. Tollin, in a very thorough study of the life and work of Cesalpinus, concedes that this philosopher knew the greater circulation ("hat den grossen Kreislauf gekannt") but did not comprehend it.

A further very convincing proof of the circulation of the blood is found in the presence of the little valves, which are found in great number in the course of the veins, and which are so arranged that they only permit a centripetal flow of the blood and prevent a centrifugal flow. These valves Fabricius ab Aquapendente knew and described, but did not comprehend their object.

This proof Cesalpinus did not take into account, with which fact Sprengel, a historian of medicine, reproaches him. It is a fact, however, that although Cannano of Ferrara as early as 1547 described a few of the valves of the vena azygos, and determined that their concave side was turned toward the heart, and although a few years later Fabricius ab Aquapendente had found analogous valves in the entire venous system, and had demonstrated them to his pupils, he published his discovery *"De venarum ostiolis"* only in 1603, that is ten years after the appearance of Cesalpinus' *"Quæstiones peripateticæ."*

On the other hand it must be stated that Fabricius, the describer of the valves in the entire venous system, did not recognize at all the function of the same, which consists in preventing the reflux of the blood in a centrifugal direction and to aid the centripetal flow during muscular activity; he thought on the contrary, that they were intended to retard the flow of the blood from the heart to the periphery of the veins. Who then was the first to base the theory of the circulation of the blood upon the function of the venous valves?

To Ceradini is due the credit of having brought to light a series of important documents which lead to the conclusion that the first to recognize the function of the venous valves was the famous theologian and canonist of the republic of Venice, Paolo Sarpi, the friend and pupil of Fabricius. It is a fact that some contemporary authors ascribed to Sarpi the discovery of the circulation of the blood. Brother Micanzio, Bartholin, Vesling, Gassendi, and Walaeus name him as the discoverer. Voss (1685) wrote that the discovery made in Italy by Cesalpinus of the circulation of the blood "Paulo Sarpi Veneto in primis placuit." Vesling wrote to Bartholin that he had seen in the possession of Brother Micanzio after the death of Sarpi an autograph of the latter, *in which the circulation of the blood was described.* The famous Dutch physician Walaeus wrote in the year 1640: "Paulus Sarpi Venetus valvularum in venis fabricam observavit accuratius . . . ex valvularum constitutione aliisque experimentis, sanguinis motum deduxit egregioque scripto asseruit" (observed more accurately the device of the valves in the veins . . . from experiments on the construction of the valves he deduced the movement of the blood, and defended it in an illustrious writing (thesis)). Unfortunately, however, the manuscripts of Sarpi which were preserved in the library of the Servitians at Venice were destroyed, together with a large portion of the monastery, by a fire in September, 1769, and there was preserved only a passage cited from a letter, in Griselini's book entitled, "Del genio di fra Paolo Sarpi" (Venice, 1783), in which letter Sarpi makes allusion to that which he "had observed and written down concerning the circulation of the blood in the vessels of the animal body and the structure and function of the venous valves."

What credit then belongs to William Harvey, the British discover of the circulation of the blood, after Servetus and Colombo, after Cesalpinus, and Sarpi? Certainly he was not the first to correct the error of Galen relative to the permeability of the septum and to assume that all the blood passes from the right side of the heart through the pulmonary vessels into the left side: this was the discovery of Colombo and Servetus. It was not he who first recognized the presence of arterio-venous anastomoses, the passage of the blood through the same, and the centripetal direction of the course of the blood in the veins: this was the great discovery of Cesalpinus. It was not he, who first described the venous valves, for already Cannano knew of them and his teacher Fabricius ab Acquapendente described them in detail—it was not he who discovered the physiological importance of the same for the circulation; this was probably the discovery of Paolo Sarpi. And yet great credit is due to Harvey, for the reason that he more clearly defined the theories of his predecessors, and firmly established them by numerous vivisections and ingenious experiments. The modern Italian physiologists (Luciani,[12] l. c.) accuse him of having committed a great wrong by trying to claim the glory of the discovery, not mentioning the names of Cesalpinus and Servetus, and making it appear that he did not know of their works.

After the historical-critical studies of Ceradini and also those of Tollin (which agree on this point) it would be absurd to assume that Harvey was not fully acquainted with the works of Cesalpinus, which appeared at Venice in the year 1593, that is, five years before Harvey took up his abode at Padua, where he remained four years (1598–1602) as the pupil of Fabricius d'Acquapendente. If he remained silent in the face of the charges of his contemporaries Micanzio, Vesling, Walaeus, Riolan, Bartholin, and others, who accused him of plagiarism, it was evidently because he wisely wished to refrain from entering into a discussion in which he had much to lose and nothing to gain.

In spite of all this his pamphlet of 72 pages, which appeared at Frankfurt in the year 1628, "Exercitato anatomica de motu cordis et sanguinis in animalibus," is without doubt the masterpiece of a man of genius.

Even at the present day, after the lapse of more than two and one-half centuries of scientific investigation, this "opusculum aureum," as Haller called it, calls forth admiration of the reader, by the clearness of his ideas, and the logical arrangement of the observations, all of which were based on vivisection. With the exception of a few inaccuracies and errors, everything in the book is well observed and well conceived, and it can still at the present day serve as an introduction into a more detailed study of this interesting subject.

Upon laying bare the cardiac region of living animals, he observed that the heart is alternately in motion and at rest. During systole it rises, and with its apex moves the parietes of the chest; it contracts in its entirety, and especially in its lateral portions; it becomes hard, like the muscles of the arm when they contract, and in the case of the cold-blooded animals it grows pale, because all the blood is sent out from its cavity. Pulse of the arteries is synchronous with systole of the heart. When the heart is at rest, the arteries also cease pulsating. If one open an artery, the blood gushes forth violently at each systole. Accordingly, at the moment of systole the blood is driven into the arteries, and cannot flow back, because the cardiac valves prevent reflux.

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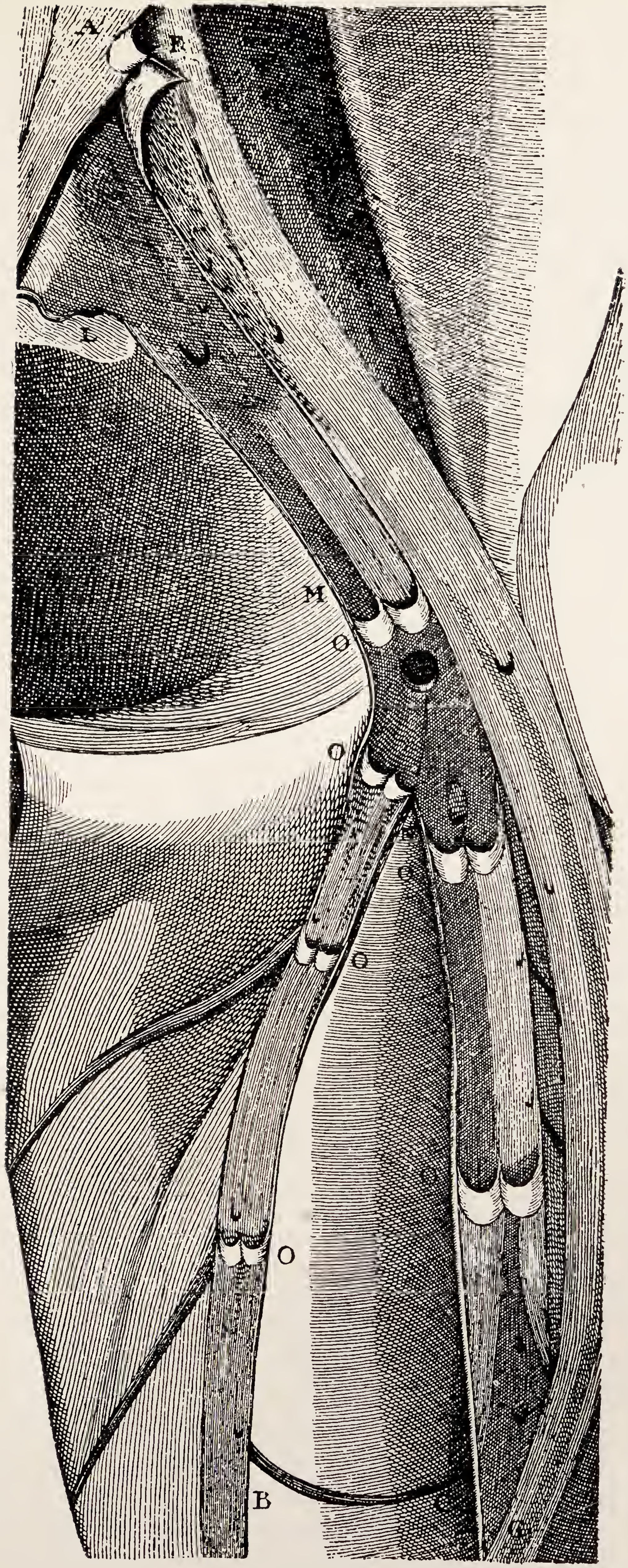
[12] See Luigi Luciani, Physiologia, Vol. I, p. 125, translated into German from the Italian by Silvestro Baglioni and Winterstein, 1904.

Like the ventricles, so also the auricles contract and relax, but earlier than the ventricles. The motion seems to start from the atria, proceeding thence to the ventricles. When the heart dies, the left ventricle is the first to stop, then follows the left auricle, then the right ventricle, and the "ultimum moriens" as already Galen had observed, is the right auricle. If one cut through the apex of the heart at a moment when only the right auricle continues to contract, one sees blood coming forth at each contraction. Therefore the blood is driven into the ventricles by the contraction of the auricles, and not through the suction produced by the expansion of the ventricles.

The function of the motion of the heart is to force the blood coming from the veins into the arteries and thus distribute it over the entire body. Since the interventricular septum is impermeable, all of the blood, as has been recognized by Colombo, must go by way of the vena arteriosa and the arteria venosa through the lungs in order to pass from the right into the left ventricle. In all of this there is nothing essentially new, only the correction of some errors of Galen relative to the motion of the heart.

The conception of the general circulation is clearly expressed by Harvey in the following words: " . . . patet sanguinem in quodcumque membrum per arterias ingredi, et per venas remeare; et arterias vasa esse deferentia sanguinem a corde, et venas vasa et vias esse regrediendi sanguinis ad cor ipsum; et in membris et extremitatibus sanguinem (vel per anostomosin immediatem vel mediate per carnis porositates, vel utroque modo) transire ab arteriis in venas; sicut ante in corde et thorace a venis in arterias: unde in circuitum moveri, illinc huc et hinc illuc, e centro in extrema scilicet, et ab extremis rursus ad centrum, manifestum fit" (it is evident, that the blood enters every portion of the body through the arteries, and returns through the veins; and that the arteries are the vessels carrying the blood *from* the heart and the veins are the vessels and ways of returning the blood TO the heart again; and that in the members and extremities the blood passes from the arteries into the veins (either by *immediate anastomosis,* or mediately through *porosities* of the flesh, or by some other way?); as before was made manifest, in the heart and thorax from the veins into the arteries; thence to move in a



Valves of Veins Depicted by Fabricius

ligature, and the expanded veins appear prominently. Above the ligature on the contrary they become invisible. By the tight ligature the passage of the blood through the arteries is prevented, while the loose ligature prevents the course of the blood in the veins. Therefore the blood passes from the arteries into the veins. Here Harvey, with only slight changes, repeats the final deduction of Cesalpinus: ". . .. apparet qua de causa in phlebotomia . . . supra sectionem legamus, non infra" ("from which cause it is manifest that in phlebotomy we bandage above the cut and not below").

Also the conclusion drawn from this, that the blood flows toward the various organs in much larger quantities, "quam nutritioni sufficiens sit" "than is sufficient for nutrition"), is taken from Cesalpinus, who designated as "alimentum nutritivum" that which the blood carries along for the nourishment of the organs, and as "alimentum auctivum" (growing) the part which returns to the right side of the heart, after passing from the arteries through the capillaries into the veins.

The proof of the third thesis is founded entirely upon the physiological function of the venous valves. Harvey treats this point with great acuteness, since it is best adapted to convince the incredulous, and he adds four illustrations of ligatured arms (one of them being simply a reproduction of *"Figura I, Tabulæ II, brachii vivi ad sanguinis missionem ligati"* (living arms ligated to hinder the flow of the blood) from the treatise of his teacher Fabrizio (Fabricius ab Aquapendente), *"De venarum ostiolis,"* which exhibit the veins swelled varicosely at the place of the valves. The venous valves do not serve the purpose of preventing a superfluity of blood in the lower portions of the body, for they occur also in the venæ jugulares, which go, from above, down, similarly in the venæ emulgentes, mesentericæ, etc. They serve rather to prevent a rushing of the blood from the larger into the smaller veins, so that the latter may not tear and become varicose, they prove that the blood in the veins does not flow from the center to the extremities, but from the latter to the center. Injections through the larger into the smaller veins are often arrested by the resistance of the valves, while in the case of injections through the small into the large veins there is no obstacle.

If on the ligatured arm one press the blood out of a vein with the finger, one sees that when the blood arrives above a nodule (which shows the position of a valve), it cannot flow back again, and the portion of the vein between the nodule and the finger appears obliterated.[13] Acordingly the function of the venous valves is the same as that of the semilunar valves of the aorta and the vena arteriosa (arteria

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[13] See Figs. 1 and 2.

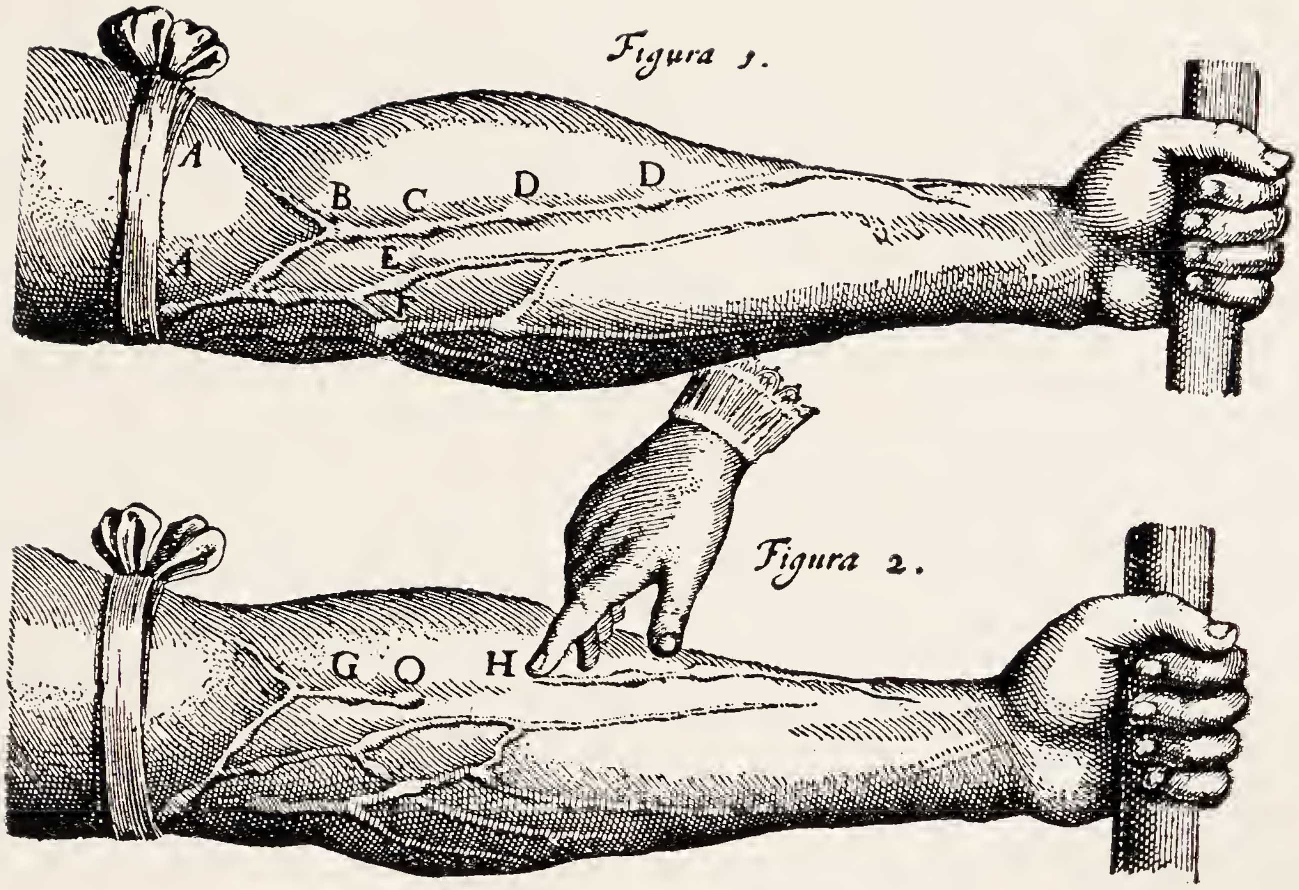

Fig. 1 is a reproduction (Photozinkography) of the two first figures of Harvey's work, edition 1639, ex officizna Joannis Maire. Lugduni Batavorum.

Fig. 1 is an exact imitation of the illustration in Fabrizio's writing "De venarum ostiolis." At *AA* the arm is ligated as is customary in venesection. One sees the swollen veins, which show enlargements at the points *B*, *C*, *D*, *E*, *F*, which proceed from the valves. These are not only found at the bifurcations (*E*, *F*), but also at (*C*, *D*)

Fig. 2 shows the same arm in which the blood has been forced from *O* to *H*, by drawing the finger forcibly along it from *H* to *O*. The portion of vein *H* to *O* appears to be obliterated, because a valve exists at *O*, which prevents the blood from flowing back to *H*, and the finger pressing upon the vein at *H* prevents the blood from flowing in from the peripheral veins.

Figures from Harvey

pulmonaris), which close the orfice and prevent a reflux of the blood. One would think that the theory of the circulation of the blood, which had already been demonstrated by Cesalpinus, would, through its perfection at the hands of Harvey, have immediately been accorded scientific citizenship, and be recognized by all. But opponents were not lacking, and among the most important and at the same time most persistent of them was Jean Riolan (Riolanus), a famous Parisian anatomist, and Kaspar Hoffmann, a leader of German science of that period, who had been, like Harvey, a pupil of Fabricius d'Aquapendente at Padua. They recognized, indeed, that the new doctrine would result in the total collapse of the system of medicine of the period, and every means seemed justified in preventing this, which in their eyes at least seemed a deep disgrace. It is scarcely necessary to say that this opposition (although it disclosed some mistakes and errors of Harvey's work) only aided in propagating and advancing the new theory. And thus the remark of Ceradini seems very apt, that "Harvey owed his success to the opposition of the Parisian anatomist, who, upon Fabricius' death was considered the greatest authority in Europe; and the error of the partisans of the Englishman arises from the parallel which they drew from the impression which his writings made in the scientific world, as compared with those of Cesalpinus. . . . Had Cesalpinus during his life met with a Riolan, to accuse him of plagiarism, absurdity, and of heresy, the very slow development of his ideas concerning the circulation, in lectures covering a period of more than thirty years (first at Pisa then at Rome), would have been impossible without discovering the possible results and applications of the same; no one could have taken from him the great renown of his discovery."[14] Furthermore, Harvey himself was at the beginning so far from having any idea of the far-reaching consequences of the doctrine of the circulation of the blood, which he had learned from Andrea Cesalpinus, that he only thought of printing it after he had for nine years treated of it in his lectures; he was only impelled to do so by the fact that this doctrine had made both friends and enemies for him, and that the latter made a great stir about it. And even after the appearance of his book, in the year 1649, the physiological importance of the theory seemed to him so problematical, that in his answer to Riolan, who declined to accept the theory, because he could see "neque efficientem, neque finalem causam" (neither the influence, nor the final cause) he had no better reply to make than to say: "Prius in confesso esse debet quod sit, antequam propter quid inquirendum. . . . Quot sunt in physiologia pathologia et therapeia recepta, quorum causas non novimus, esse tamen nullus dubitat?" (first it ought to be conceded what may exist, before inquiring what it means or wherefor. . . . How many things are received in physiology, pathology and therapeutics, whose causes we know not, yet no one doubts their existence?)

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[14] Ceradini here means that the complete comprehension of a discovery is often not made by the discoverer himself, but he may be brought to a wider comprehension of his contributions to science by the opposition of men of authority. See Chap.VI.

As long as the theory of Aristotle flourished, which had been rehabilitated by Cesalpinus and Harvey, that the function of the lungs consisted in refreshing the blood, and that this organ, in which the blood was again made spirituous and thin was nourished by the thick blood flowing back from all the organs; as long as especially the place of blood-formation remained unknown, and also the channels through which the products of digestion reached the circulatory system; just so long did the theory of the circulation of the blood remain without its true significance, and it could only be regarded as a physiological curiosity.

To judge from certain passages in Galen, it seems as if Herophilus and Erasistratus, the heads of the Alexandrian school (300 B. C.) had seen the chyliferous vessels in the mesentery of the sheep. Towards the end of the 18th century Portal remarked, as in fact also Fracassatus had done more than a century earlier, that the celebrated Roman anatomist Eustachius,[15] in studing the course of the vena azygos in the horse, had observed the thoracic duct and even some of the valves of the same. But it is certain that every trace of these accidental and isolated observations had vanished to a vague tradition, when Gaspare Aselli of Cremona, professor of anatomy at Pavia, found, in 1622, the chyliferous vessels in the mesentery of the dog, designating them by the name of lactiferous vessels. So happy was he at having found what he had been seeking, that, as he relates, "conversus ad eos qui aderant: εὕρηχὰ, inquam cum Archimede" ("I turned to those who were present: I exclaim with Archimedes, Eureka"). But as yet he had no idea of the true function and physiological importance of these vessels.

In the year 1648 Pecqet, a young physician of Dieppe, who was studying in Montpellier, found that the lactiferous vessels do not send their contents to the liver, as Aselli had believed, but to a large vessel, the thoracic duct (re-discovered by him after Eustachius) which empties into the subclavian vein. Two years later Rudbeck, a Swede, discovered the lymphatic vessels of the liver, and recognized that they also send their contents to the thoracic duct. Finally, in the year 1652, the famous Danish anatomist, Thomas Bartholin, discovered the same vessels in all parts of the body, and found that all of them, together with the chyliferous vessels, flow into the thoracic duct. In order to give further scope to the theory of the circulation of the blood, ascribed by him to Harvey, he published a new edition of his anatomy *"ad sanguinis circulationem reformata,"* being correctly convinced that he had furnished a new argument in favor of it, indirect to be sure, but nevertheless valuable.

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[15] Opuscula anatomica, Venetiis, 1564.

According to Ceradini, "Riolan himself, the upholder of every old doctrine, and the opponent of everything new, in this case withheld the sharp arrows of his criticism, in order not to see them rebound harmlessly from the strength of facts. Harvey, however, DENIED THE EXISTENCE OF THE CHYLIFEROUS and LYMPHATIC VESSELS, and EVEN THE FUNCTION OF THE THORACIC DUCT, *and died without being converted, in the year 1658,* six years after the death of Bartholin. Luciani (professor of physiology in Rome), in his elaborate handbook on human physiology, expresses surprise and regret that Sprengel, the famous historian of medicine, scarcely mentions this *"ugly trait in the character of the Englishman, this contempt for every discovery which was not his own, a blemish which would be inexcusable, even if Cesalpinus had not demonstrated the circulation of the blood before him."*

In order to complete the new theory, and to bring it to a state of absolute certainty, there was now only lacking the last decisive step, the discovery of the capillaries of the blood and the direct observation of the circulation of the blood through these capillaries from the arteries to the veins. "Supererat" (it remained), as Haller said, "ut ipsis oculis circuitus sanguinis subjiceretur" (that the circulation of the blood be subjected to the eyes). (be actually seen)

Galen, who, as already mentioned, was the first to assume direct communication of arterial and venous vessels in the organs, thought of a kind of direct anastomoses or a simple meeting of the two kinds of vessels. This was not in accord with the idea which Cesalpinus had formed; the latter conceived that the junction was made "per vasa non desinentia, ulterius transmeantia" (by vessels not stopping, but passing through), or "per vasa in capillamenta resoluta" (by vessels broken up into hairs (threads)) which term Harvey changed to "per carnis porositates" (through porosities of the flesh), and Cesalpinus thus guessed the existence of a new kind of vessels, which unite the arteries with the veins and which then were called capillaries.

Marcello Malpighi, with the aid of the microscope, was the first to observe the motion of the blood in the capillaries of the lungs of the frog. This was in 1661. With just pride he exclaimed: "Talia mihi videre contigit, ut non immerito illud Homeri usurpare possim ad rem præsentem melius; magnum certum opus oculis video" (It has happened to me to see such things, that not unjustly I could use better in the present instance the saying of Homer: I see with my eyes a sure great work).

After Malpighi, vain attempts were made by Leeuwenhoek, Cooper, and Haller to extend these observations also to the warm-blooded animals. The first to succeed in this was Lazzaro Spallanzani, who hit upon the idea of making use of the hen's egg during the process of development of the chick. The enthusiastic words with which the great physiologist reports his discovery cannot but provoke a smile. "For a long time I had been burning with the desire to discover the circulation of the blood also in the case of the warm-blooded animals, to observe it in the same degree in which I had noticed it in cold-blooded animals, and thus these vessels (the vasa umbilica of the chick) attracted my attention more than anything else, because they belonged to that species of animal. Since the room in which I was, did not have sufficient light and I was determined in some way to satisfy my curiosity, I resolved to examine the egg in the open air, by direct sunlight. After I had fastened it to the small Lyonet apparatus (a small microscope, used by Spallanzani) I immediately focused the lens upon it, and in spite of the flood of light which surrounded me, I could, by partly closing my eyes, distinctly see the blood circulate through the entire complex of the arterial and venous umbilical vessels. Overcome by this unexpected pleasure, I felt at liberty also now to cry out: 'I have found it, I have found it!' I made the discovery in May, 1771, and in the summer vacation of that year I took pains to develop it suitably."

These observations of Malpighi and Spallanzani, separated by a century, constitute two of the chief events in the history of medicine. The glory of the first direct observation of the circulation of the blood belongs undoubtedly to the Italian physicians. Modern scientists, with more perfect microscopes, and more advanced technical skill, have only been able to perfect the description of the circulation of the blood as seen under the microscope.

In speaking of Servetus, Sir Michael Foster does not concede to him any marked influence on the development of anatomic thought of his day, and declines to regard him as a real link in the chain which leads from Galen to Harvey.[16] He would concede to the writings of Servetus only isolated bits of truth, floating along the stream of human thought, by the side of other truths, the outcome of the labors of other men. But though Sir Michael Foster speaks in the diminutive concerning the work of Servetus, he makes use of this very author's work to minimize the

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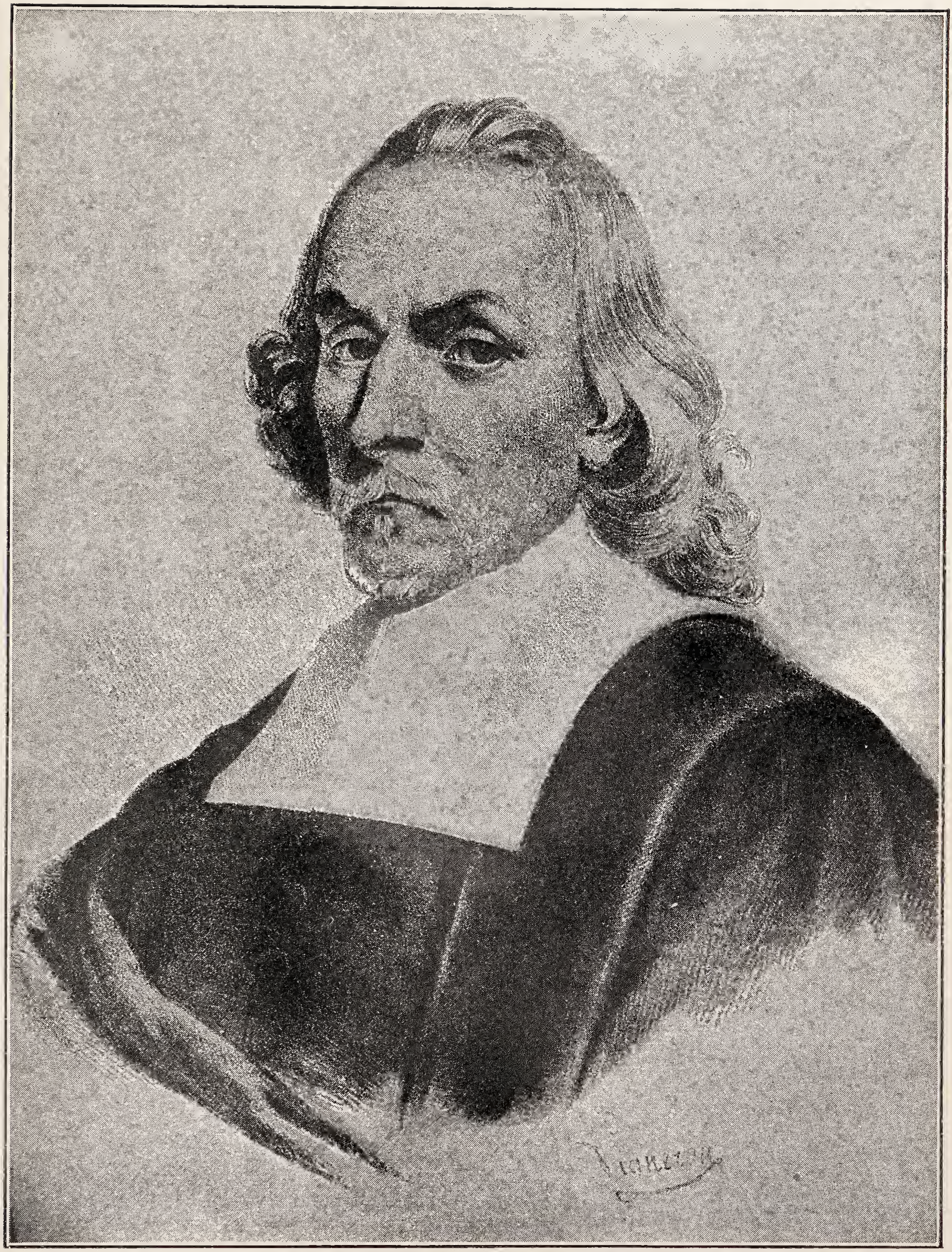
[16] Lectures on the History of Physiology, p. 24.



Harvey

merit of another contributor to the history of the circulation, namely, Matheo Realdo Colombo; for he suggests that Colombo might have taken the idea of pulmonary circulation from Servetus, and his reason is that in 1546 Servetus sent to Curio in Padua a manuscript copy of his "Restitutio." "This Columbo may have seen. Again, when the edition of the published 'Restitutio' was burnt in 1553, some few copies escaped. One of these may have found its way to Rome before Columbo had sent his work to the press." Tollin[17] and Preyer[18] have arranged the quotations from Colombo and Servetus concerning the lesser circulation side by side, suggesting that Columbo learned what he knew from Servetus.

Sir Michael Foster furthermore attempts to support his charge of the plagiarism of Columbo from Servetus by what he calls an "unabashed attempt to assert ownership of the discovery of the third ossicle of the ear, the stapes." According to Fallopius, the stapes was first described by Ingrassius of Palermo, a Sicilian, in 1548. This may be, as Foster states, a theft, but it is not absolutely proven, because at the present day, two physiologists or anatomists, in different countries, have been known to make and claim an identical discovery simultaneously.

The character of Colombo is represented as having been that of a "vain and ungrateful successor of Vesalius,"[19] and his standing, it is indicated, suffered by his fulsome adulation of Pope Pius IV, a pope whose character has met with much criticism.

From the standpoint of critical conservatism we cannot conceive that the charges of plagiarism and theft against Colombo have been satisfactorily proven. At least they would not be accepted as proven by a judicial historian. I have already given the proofs in the preceding on the authority of G. Ceradini,[20] which are incontrovertible facts, that Colombo could not possibly have plagiarized Servetus. His accurate descriptions of the pulmonary circulation may be the results of his own research and subjective analysis. Some doubt is thrown on Ceradini's conclusion by the incomparably precise historic researches by H. Tollin,[21] who does not consider that Colombo is original, but simply has confirmed what Servetus described before him.

Concerning the merits of Andreas Cesalpinus,[22] Foster[23] suggests that "they were not the outcome of patient research, or real study of the phenomena themselves, but may have been flung out in the spirit of controversy, as effective assaults upon the accepted doctrine of the times." In the preceding, however, I have given the evidence that we have no right to question, that the theories as announced by Cesalpinus were not the outcome of real study of the phenomena themselves.

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[17] Virchow's Archiv., Bd. 91, 1883.

[18] Samml. physiolog. Abhdl., 1876.

[19] Puschmann Handb. d. Geschichte d. Medicin, Bd. 11, p. 331.

[20] Loc. cit.

[21] Loc. cit.

[22] See preceding.

[23] Loc. cit., p. 35.

A personal and systematic study of Cesalpinus' works undeniably gives the impression that he was a man of most vacillating character, of insufficient force of conviction even where there was every probability that his views were correct. But even his greatest opponent, Tauriel (of Montbéliard), has much to say in praise of his versatility and the inventive power as displayed in the "Quæstiones peripateticæ," and that these talents when coupled with "love of truth and piety" would justify us in expecting great and useful things of Cesalpinus." ("Qua sua singulari facilitate poterat prodesse plurimum—res magnas et humanæ societati utilissimas præstare possunt.")

It cannot lead to a just opinion of Cesalpinus, to merely glean here and there in his works; isolated patches of his writings must inevitably present a disconnected, illogical character, whereas in truth his argument is as homogeneous and consequential as that of Colombo—whom Harvey honors and quotes to the total exclusion of Cesalpinus.

It is very regrettable that the great Haller made an erroneous construction of a passage from Cesalpinus, in which a reversal of the present idea of the circulation was assumed to take place during the waking state (a certain amount of reflux of blood from the arteries to the heart during the waking state).[24] The forcible logic of Tollin [25] and of Ceradini have set the views of Cesalpinus in the right light, and proven that he was a physiologic experimenter of ability. He had not only grasped clearly the pulmonary circulation, but also the systematic circulation. There is no doubt whatever that he knew that the flow of blood to the tissues took place by the arteries, and that the return of the blood from the tissues took place by the veins and not by the arteries. The published works of Colombo and Servetus to my mind represent independent and individual conceptions. The charges of plagiarism of one from the other, are ingenious speculations.

It cannot possibly deduct from the brilliancy of Harvey's genius to have the work of his predecessors set in the right light. His admirable combination of anatomical features with physiologic function, his conception of experiments, both original and conclusive, testing the validity of his explanations, all combine to complete the picture of a physiologic thinker of colossal ingenuity. If other workers preceding Harvey fell short of his results, it may of course have been due to a lesser intellectual endowment, but it may also have been due to the fact that they were working in countries in which scientific research was under the ban of Church and State. Furthermore, path-hewing is more difficult than path-widening. The older and preceding workers in any department of science do not, as a rule, grasp the entire truth concerning the relation of things. Scientific exploration takes place, as a rule, bit by bit, sometimes by strenuous and painful endeavor. The mental or subjective conceptions preceding the experiment being often more laborious than the actual devising of experiments to test the validity of the conception. All this in the discovery of the circulation of the blood took centuries.

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[24] See preceding.

[25] Loc. cit.

As grateful recipients of the labors of the historic apostles of physiology, it behooves us to maintain a judicial mental attitude and interpret the contributions of each physiologic worker from the aspect of the conditions and influences—political, religious, scientific, and otherwise,—that were predominating at this time, and in that light it is astounding to observe how near to the truth Servetus, Matheo Colombo, and Cesalpinus came with their crude methods of subjective and objective analysis, and we will have to sum up in one abstract sentence with Professor L. Landois that "William Harvey, a pupil of Fabricius (until 1604) finally constructed, between the years 1616 and 1619, partly from his own investigations and partly from the results of former observers already mentioned, the picture of the circulation of the blood, the greatest physiologic ashievement, which was published in 1628 and marks a new epoch in physiology."

A RÉSUMÉ OF WHAT WE HAVE LEARNED FROM THE LITERATURE QUOTED IN THE PRECEDING ABOUT HARVEY AND HIS RELATION TO HIS PREDECESSORS

We gather from Harvey's own writings and the scholarly studies of Henri Tollin,[26] that, seven years before his death, Harvey plainly teaches that before him there was a knowledge of the circulation of the blood. In "De Motu Cordis" and in his writings to Riolan he teaches that Realdo Colombo has shown that the blood does not pass from one ventricle into the other through the middle wall of the heart, but through a long circuit through the lungs. Harvey imparts to his friend Boyle that he got his first suggestion of a circulation of the blood from his Paduan

[26] Loc. cit.

teacher, Hieronymus Fabricius ab Aquapendente, led to the idea by meditation over the function of the valves in veins, and to his friend Ent, that Fra Paolo Sarpi (1623) had left a writing on the uninterrupted return of the blood (to the heart) five years before Harvey published his. Harvey declares it the duty of the scientific anatomist to learn the experiences of his predecessors, to prove and to use them. He abundantly makes use of this right and therefore walks in the footprints of Galen and Hippocrates, of Pliny and Aristotle, of Fernel and Vesalius, of Columbo and Aldobrandino, and of many, many other authorities. Harvey regards the moving blood as inseparable from the spirit, which first gives it movement, warmth, feeling, power of nutrition, even something heavenly, corresponding to "the element of the stars." Harvey makes this probable to us by many relative thoughts, which he has drawn from not only Realdo Colombo, whom he cites several times, but also from Michael Servetus, Sarpi, and Andreas Cesalpinus. But through the proud, independent, indomitable peculiarity of his character, Harvey makes plain to us why, where the renown of a discoverer was at stake, he could not bring himself to say: "I am not the discoverer, but yonder Aristotle, and Aquapendente, there Aselli, Pequet, Rudbeck, and Bartholin, there again Caspar Hoffmann, here finally Sarpi, Cesalpinus, Colombo, and already before them Michael Servetus."

I am well aware that Tollin's [27] conclusion may vary from the current, traditional jubilation over the great Harvey, the demonstrator of the entire circulation of the blood. But Tollin, if abused for this variation, will answer with the words of his teacher, William Harvey: "Shine by your own experiences, not by those of others" (propria experientia nitendum est, non aliena (Opp. 172)).

The completion of the discovery of the circulation of the blood by Malpighi and Leeuwenhoeck, by Eustachius, Aselli, Pequet, Rudbeck, and Bartholin, by injections of Swammerdam, Horne, and Ruysch, the observation on invertebrate animals on the part of Willis and Milne-Edwards, by the discovery of graphic methods, by the observations of the vasomotor nerves on the part of Bernard, Brown-Séquard, Ludwig, lead Chapman [28] to the conclusion that *"the* SPIRITUAL DEVELOPMENT *of humanity is also a growth that is governed by life-laws, and that a discovery, although if it was not made by him to whose name it is wont to be attached, yet at the appointed time would have seen the light of the world."* All circumstances show that towards the end of the sixteenth century intellectual Europe was ripe for the acceptance of the discovery

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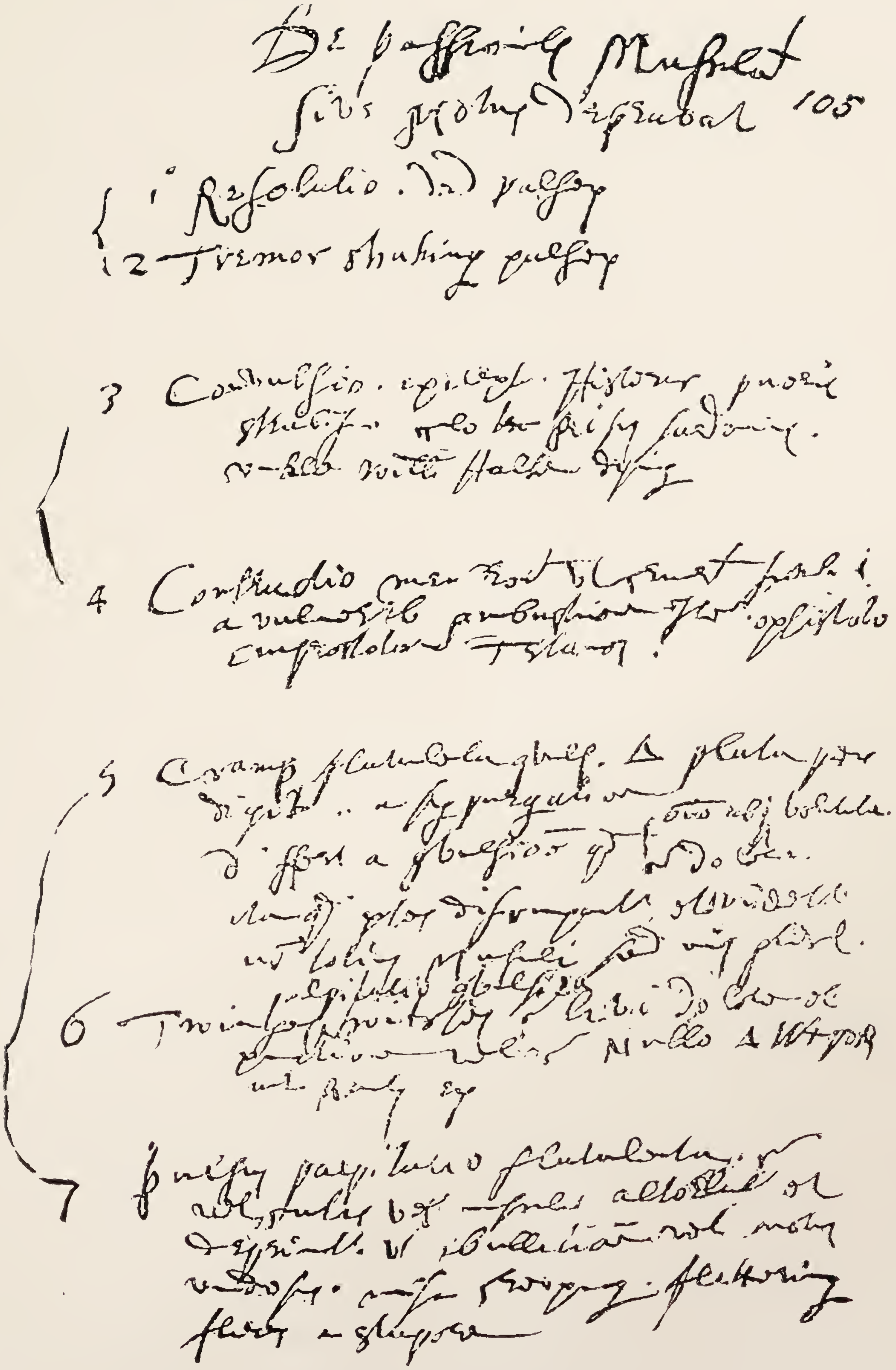
[27] Henri Tollin, Arch. f. Pathol. Anat., Bd. XXXI, 81, Heft I.

[28] Loc. cit.



De passionibus Musculorum sive motus depravati 105

1 Resolutio. dead palsey

2 Tremor shaking palsey

3 Convulsio. epilepsia. hysterica passio [illegible]

4 Contractio [illegible]

5 Cramp [illegible]

6 [illegible]

7 [illegible]

Handwriting of Harvey

of the circulation of the blood. If it had come a few centuries sooner, it would have died at its birth. Italy and particulary Padua were alive with speculations, hypotheses, and theories concerning the course of the blood through the body. One who will compare the Latin original of Harvey's writing upon the circulation with the writings of the great Italian anatomists and physiologists of the sixteenth century, will be astonished at the similarity of thought and expression. (The same mode of thought and expression pervade both).[29] In the method of his investigation, observation, and conclusions, Harvey is essentially Italian. An Englishman by birth, an Italian in thought, Harvey lived and died as a student of Padua. Thus Chapman returns to where he started out. Between Galen and Servetus, the second and third epochs of the discovery, Chapman establishes an interval of 1381 years; between Servetus and Cesalpinus 40 years; between Cesalpinus and Harvey 35 years. When Servetus stood forth, he risked that his discovery might be burned with himself. When Harvey stood forth, all had been prepared. He came, considered, and gained the victory.

Tollin pointed out what an advance in the investigation of the history of the discovery of the circulation of the blood, even in Harvey's fatherland, since 1878 (the 300th anniversary of Harvey's birth), we have to record. Before 1878 it was not only the dogma of the profane crowd of the educated but also of the professionally educated in England, that as Minerva from the head of Jupiter, even so fully and entire, the discovery of the circulation of the blood sprang from the head of Harvey. Now even in England it is said: When Harvey came, everything was fully ripe. If he had not harvested then, at that time, another would have come and gathered in the harvest. The seed sowing was the work of the Spaniards, especially of Michael Servetus; the watering, the care, the pruning, the work of the Italians, especially of Colombo, Cesalpinus, Fabricius d'Aquapendente. To Harvey remained the honorable, remunerative, although always troublesome work of the harvest. But what we possess to-day, we possess through him who harvested it for us. And for this Tollin designates the gifted Harvey, "the great Briton, the pattern of all harvesters and classifiers."

William Harvey, armed only with a magnifying glass, accomplished such great things that one cannot express what this genius would have accomplished had he, like Malpighi, been furnished with a microscope. But the discoverer of the blood circulation is not Harvey, however much, misled by the favor of two kings and the almost idolatrous reverence of his nation, he sought to convince himself that he was such. And he did convince himself and finally did believe in it, because this belief only too soon had become the dogma of his nation. But, in the moments when he is candid, he acknowledges that several persons before him, induced by Galen's authority and that of Colombo and Servetus, beside other reasons, had taught the truth about the blood vessels and had expressed the opinion which he now claimed as his own; yes, that many, long before him, had known of a blood circulation, by means of which continually the blood passed from the arteries into the veins and from the veins returned to the heart; that he himself had gladly and diligently read the books of those who had borne the torch of truth before us, that he willingly paid due respect to those old authorities and only *had not mentioned particularly all of the modern ones in order not to give occasion for disputes;* and that it was he who had erected upon firm and necessary foundations, more distinctly, more orderly, and fully corresponding to the actuality, the blood circulation discovered before him.

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[29] Tollin: Loc. cit.

We once more affirm Harvey, the author of "De motu cordis et sanguinis," 1628, did not discover the lesser circulation. This Servetus discovered in 1546. Harvey did not discover the greater circulation. This Cesalpinus discovered in 1569. Harvey did not discover the venal valves. These Jacob Silvius, Sarpi, and most accurately Aquapendente discovered in 1574. Harvey did not furnish the clear-sighted proofs of the circulation. These were given by Servetus, Colombo, Valverde, Aranzi, Ruini, Rudio, Sarpi, Cesalpinus, and Aquapendente. Harvey never saw the circulation of the blood. Malpighi saw it several years after Harvey's death (1661).

Strictly speaking, Harvey did not even describe the circulation, but a double half circulation. Whether in the lungs and in the extremities the arterial endings were in communication with the commencement of the veins by anastomosis, or by infiltration of blood into the pores of the tissues, he never dared to decide, because the magnifying glass here failed to tell him. And so, since two half circulations suddenly end without a demonstrable continuation, there is no actual, no completed circulation.

Nevertheless, even then, Harvey still remains an incomparable genius. For *"by the accuracy and thoroughness of his deductions, by the skillfulness, by the industry, and the abundance of his experiments, by the carefulness and the delicacy of his observations, by the keenness and shrewdness of the proofs, by the clearness and truth of the conclusions drawn, by the novelty and importance of the interposed reflexions, above all by the harmonious connection of the whole,"* William Harvey, the great regius predemonstrator of the blood circulation, the movements of the heart and of the blood, has lifted these from a hypothesis of the darkest possibility to the clearest probability, from the recesses of single obscure studies to the shield of public opinion, from the individual views of a few favored ones to a dogma everywhere sanctioned. And in this sense one can indeed say with Tollin: "Without the school of Padua, yes without Erasistratus, Aristotle, Galen, Servetus, Vesalius, Colombo, Cesalpinus, Aquapendente, we would have had no Harvey, but without Harvey no discovery of the blood circulation."

Harvey quotes among the authorities known at and previous to his time Aristotle, Galen, Erasistratus, Vesalius, Realdo, Colombo, and Fabricius d'Aquapendente. He does not cite Michael Servetus nor Cesalpinus, and yet he must have been acquainted with their works, for his methods of thinking show many resemblances to that of these writers. It is absurd to assume that such an omniverous reader as Harvey, who studied four years at Padua, where the works of these two men were widely known, was not acquainted with their writings. The omniverous reader, Harvey, who is described as accompanying King Charles I, and taking care of the princes, and during the battle of Edgehill (Oct. 23, 1642), as sitting at the outskirts of the fight under the hedge reading a book (Aubrey).[30] Furthermore, Robert Willis,[31] the greatest Harvey connoisseur in England, actually asserts that Harvey was a free-thinker like Servetus and Cesalpinus, and even an Antitrinitarian, so that he must have been in religious sympathy with these two men, and for this reason again he probably sought and was familiar with their works. And yet we will have to excuse him for not quoting these two early discoverers for the following reason:

Because of his heretic work "De Trinitatis Erroribus," Michael Servetus was burned at the stake at Geneva, in 1553.

Cesalpinus, free-thinker like Servatus, friend of Trismegistus and precursor of Spinoza, could only save his life by retracting.

Matheo Realdo Colombo was a diplomat, clerically inclined, friend of several cardinals and flatterer of the inquisitorially-minded pope Paul IV. His memory was held in high esteem at the English Court, whose queen was a devout Catholic, and whose king was secretly her ally. A hater of the Protestants, an enemy of all Puritans, and still more of the free-thinkers, a systematic adherent of a strict bishopric régime, the royal patron of Harvey was the man who sent the members of Parliament into prison, who made the method of punishment severe to the utmost to

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[30] Foster: Loc. cit., p. 42.
[31] Loc. cit.

Anabaptists and Antitrinitarians, and applauded the massacre caused by Ireland's Catholics among the Protestants. As is well known, the king was beheaded in 1649.[32]

The Protestant England, with its independent spirit of inquiry, to whose free manner the Harvey jubilants attributed Harvey's great discoveries, did not exist at his time, and not for a long time after it. Harvey died the 3d of June, 1658. And still King Charles II, son of the executed Charles the first, forced on Bartholomew's Day, by his Acts of Uniformity, two thousand English clergymen to resign their positions because they refused to subscribe to the thirty-nine articles of the King's faith. Even quiet prayer-meetings in the attics were considered treasonable. The heretics were banished. None of the exiles were allowed ever to come again within five English miles of their villages, or any English cities.

Twelve years after Harvey's death the nonconformist act was rendered still more severe. Indeed twenty years after his death nobody could venture to adhere publicly to Cesalpinus and Michael Servetus. Up to 1828 the Acts of Uniformity of Charles II governed, which excluded all nonconformists from Parliament and civil service, also from the office of a royal court physician, which meant so much to Harvey. Under the son of Harvey's patron, 80,000 Englishmen had to suffer all kinds of persecution because they refused to take oath to the faith of the State. Eight thousand alone had to go to prison for their faith.

James II, successor of Charles II, declared himself openly a Catholic, to destroy the last remains of the free faith by strict government without Parliament.

But his son-in-law, William of Orange, thirty years after Harvey's death, set up the famous Acts of Toleration in 1689. But free-thinkers, like Peter Bayle, were dismissed from their offices (1693) and those who sided with Servetus, called the Socinians, were expressly excluded from every public tolerance.

There are some who lauded Harvey's character up to the skies, just as they called (according to the legend) Harvey's mother the best of all women, probably because one knows nothing of her. But has nobody discovered a predestination as a martyr in a man who always kept his faith as secret as possible, and who deserted his royal benefactor as soon as the luck left the king's banners. No wonder, therefore, that Harvey does not mention Cesalpinus and Michael Servetus, even had he known them by heart.

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[32] A court physician, under such a tyrannical prince, who would have dared to confess himself to Cesalpinus or even to Michael Servetus, would have certainly been executed, like Harvey's contemporary, the Saxon Chancellor Nicholaus Crell, who was beheaded for heresy at Dresden, the 23d of October, 1591, after ten years of imprisonment in a fortress. And Crell was not even a free-thinker, but a Protestant, who had the courage to prefer to be called a Christian, instead of a Lutheran or a Calvinist.

The fruit of an intellectual deed is frequently more evidenced in the incentive which that deed gives to investigation than in the actual contribution to science made therein. An immense industry was developed by the exact proofs of the circulation furnished by Harvey, diseases were conceived of in a new light, efforts at transfusion were made, and of injection of remedies into the circulation.

The discovery of the circulation of the blood was the work of almost a millennium from Aristotle and Galen to Harvey, but the one who first logically drew true consequences out of hundreds of years of preceding work, and upon whose broad intellectual shoulders all subsequent investigations rested, was William Harvey; and to-day, 328 years after his birth, we may side without reservation with the words of Bartholin: "At Harveyo omnes applaudunt circulationis auctori!"

XII

SCIENCE AND ART IN MEDICINE

Their Influence on the Development of Medical Thinking[1]

IS MEDICINE A SCIENCE OR AN ART?

MEDICAL men the world over frequently have to hear the criticism, I might call it the reproach, that medicine is not a pure science, that its methods and its discipline are not sufficiently accurate to merit this term. On the other hand, critics are not wanting among the non-medical public, who argue that medicine is not a perfect art.

Now, what is an art, and what is a science? In a former address President Ira Remsen[2] attempted to define these terms, and on the authorities there quoted by this versatile educator we might profitably start out by borrowing the interpretations of the terms science and art. One writer says: "The distinction between science and art is that science is a body of principles and deductions to explain the nature of some matter, and art is a body of precepts with practical skill for the completion of some work. Science teaches us to know, and art to do. In art, truth is a means to an end, in science it is the only end. Hence, the practical arts are not to be classed among the sciences." Another writer says: "Science and art may be said to be investigations of truth, but one, science, inquires for the sake of knowledge, the other, art, for the sake of production. Hence, science is more concerned with the higher truths, and art with the lower. Science is never engaged, as art is, in productive application."

These definitions are apparently not equally clear with regard to science and art. With regard to science, they are clearer than with regard to art. Science has for its object the accumulation and systematization of knowledge, the discovery of truth. This part is clear, and also that art is a body of precepts with practical skill for the completion of some work. But it is not clear how truth is only a means to an end in art, and in science it is the only end. Surely, if a truth could be expressed as a precept in as concrete a form in any art as it could be in a pure science, it would be, or at any rate should be, the only end, the only object of the art as well as of the science.

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[1] Address on occasion of conferring of the degree of doctor of laws (*honoris causa*), St. Johns College, Maryland. *Journal of the American Medical Association*, January 27, 1906.

[2] "The Age of Science," Science. July 15, 1904.

Then the question might arise, What are higher and what are lower truths? In the decision much depends on the individual standpoint of the judge. What is a higher truth for a scientist may be a lower truth for an artist, and *vice versa*. If it is said that in art truth is a means to an end, we have a right to inquire what is meant by the truths of an art? For instance, are counterpoint, thorough-base, and harmony the truths of music?

I know of but two individuals in the history of the modern scientific world who, by training and experience, could be considered competent to answer this question, both having been artists and scientists at the same time, of acknowledged ability. One was Hermann von Helmholtz, physicist, physiologist, and musician. The other was Theodor von Billroth, anatomist, pathologist, surgeon, and musician. Both of these men have given us the benefit of their thinking on the borderland between science and art. Helmholtz, in his "Sensations of Tone," and Billroth, in his "Psychologische Aphorismen über die Musik" (Wer ist Musikalisch?). (See Chap. IX.) To the student of either of these works seeking information on the questions above propounded, it must soon become evident that an exact and definite distinction between art and science is not always possible; particularly is it not possible in any concrete case. We are, therefore, enabled to meet the critics arguing that medicine is not an exact science nor a perfect art, with the statement that a distinction between these two is by no means possible and that there is much debatable territory between human knowledge and human ability. Medicine did not originate as a science, but by dire force of necessity. For centuries on centuries its treasures were gathered from experience only, and were developed into an art by the genius of its representatives. Professor Remsen was, perhaps, too accurate in attempting to draw a hard and fast line between art and science. Every science is desirous to become an art, and every art tends to become a science. So also medicine. I mean by this that art requires a scientific foundation and that science requires ability to do. In other words, science requires productive application, the power of achievement.

My convictions induce me to differ with my erudite teacher, Professor Remsen. At least, to differ with him in applying the same ideas to medical science as he is disposed to set up for the science of chemistry. In rating the history of the discovery of oxygen and chlorin by Scheele (1774), he lays great stress on what seems to him a fact, that Scheele did not work toward these discoveries with any practical object in view, and, though this work, while it was being done, seemed to be of no utilitarian promise, its value in the light of present-day industry can not be overestimated. It would be very difficult to prove that Scheele worked absolutely oblivious of any practical results that his work might have. It is true that scientific men frequently work from pure love of science, prompted by high ideals and disinterested enthusiasm, but there is often an undercurrent of thought which is sometimes discovered in the private correspondence of chemists, that the work they are doing may yet be of utility to their fellow-men, and perhaps profitable to themselves. In medicine, too, we have the history of a large number of unselfish workers. But if they should work purely in following ideals, without any hope of gaining any utilitarian results, they are not medical men in the true sense of the word. Medical science shall and must be useful. A quotation from Cicero is well applicable here: *Nisi utile est, quod faciamus, stulta est gloria.* An oft-quoted saying of Bismarck is: *Die Politik kann nicht im Laboratorium gemacht werden, denn sie hat überall mit dem Menschen zu thun* (Politics can not be made in the laboratory; they have everywhere and at all times to deal with the human being). In an analogous manner one may say of medicine: "Medical science can not be made exclusively in the laboratory; it has always and everywhere to deal with human beings." I said medical science, but I would, perhaps, have been more exact to have said medical art. For science analyzes, it dismembers and disintegrates in order to penetrate into the depths of things, and to study the final microscopic living element, the cell, in its life properties and processes. This is true, for instance, of physiology and anatomy. But art in medicine keeps the whole together, it observes the individual, the human being, in its entirety. Science seeks general laws; art in medicine seeks the personality. The object of all medical science is to help and to heal, and the object of all medical, artistic, and scientific thinking is the therapeutic thinking. Scientific facts in medicine may have an absolute value, although they may not be directly useful, but, aside from such an absolute value, the whole art and science for the physician must be concentrated on the desire to aid his patient in regaining health by means of his science and art. Prof. I. Peterson, of Copenhagen, was one of the first to assert: "The attempt to erect the medical clinic exclusively on the achievements of natural science has proven itself to be impossible of execution."

HISTORY OF MEDICINE

The evolution of medical thinking can best be studied in the history of medicine. From the earliest rudiments among the Egyptians, Romans, and Greeks, the first beginning of clinical discipline dates from the Arabic apostles of medicine and the religious medical thinking, which was the custom of the healing monks of the Medieval ages, who were the founders of the schools of Salerno and Monte Cassino. A new period begins with Paracelsus, a man who was an excellent chemist for his time, a contemporary of Luther, and who based his therapy on a chemical substratum. Paracelsus created the conception of the *Archous,* the central ruler of the human organism, which holds all the bodily functions together and regulates them. This idea occurred later in the writings of Georg Stahl, under the name of *anima,* and even in our time is frequently met with in a disguised form, sometimes under the designation "vital force." Scientific thinking, that is, thinking based on observations of objective facts, began rather late in the history of medicine. Not until the foundation of universities did it meet with appreciation and respect. This was brought about mainly by the anatomists of Italy, particularly by Andreas Vesalius, a German by birth (Andreas Wesel), but professor in Padua, 1537 to 1544. Then followed Malpighi, Botall, Harvey, the celebrated discoverer of the circulation of the blood, and eventually Morgagni (1682–1771), the founder of pathologic anatomy.

This brings us to a period in the history of German medicine in which this science began its emancipation from philosophic and purely deductive thinking, to the scientific and inductive method of investigation, including severe criticism and testing of facts by exact proofs and experimental research. At the time the battle began in Germany between philosophic and scientific thinking in medicine, the methods of the natural science had already gained victories in England and France, and a new era of logical scientific thinking had begun under Rokitansky and Skoda in Vienna, but the course of the struggle was most interesting in Germany, particularly in Berlin. It was begun by an intellectual giant of such rare genius and unfailing logic that he might be called the father of exact physiology and medicine in Germany. I speak of Johannes Müller. He was the first to compel the abandonment of the philosophic thinking in medicine, insisting on the methods of the natural sciences, on investigations based on exact facts, on tests by experiment, and on the importance of microscopic investigations. The work of this great master was continued and perfected by his two great pupils, Hermann von Helmholtz and Rudolph Virchow. Helmholtz particularly condemned the effects of the volitional thinking of the medical philosophers of his day, accusing them of contempt for exact investigation of the facts. "Natural philosophy in medicine," he says, "aims to explain the phenomena of normal and abnormal life, from the idea of absolutism. It is working toward a false idea of science, in a narrow, incorrect and one-sided appreciation of the deductive methods. It is true it was not only medicine among the sciences which was ensnared in these errors, but in no other science were the evil consequences so evident and did they obstruct progress so effectively as in medicine. The history of medicine, therefore, can claim a very special interest in the history of the development of human thought, for no other history is better adapted to demonstrate that a correct criticism of the sources of our knowledge is practically one of the most important duties of true philosophy." The demands of Helmholtz for exact scientific methods in medicine, when brought into practical execution, succeeded in raising the science to the standard of an exact discipline and brought it into possession of a wealth of solidly grounded facts.

The other great pupil of Johannes Müller, Rudolph Virchow, became the reformer of medicine in another direction. While professor in Würzburg, in 1849–1856, he laid the foundation of his cellular pathology, and later on, in 1856, when he was recalled to Berlin, he continued the work of his teacher in perfecting the science of pathology and pathologic physiology, based on individual investigations, on experiment, and on pathologic anatomy. The conception of a disease became an anatomic conception, through Virchow, but his pathologic anatomy aimed also to fathom the processes of the living body, and to become a beacon light for clinical study, and in that sense was a truly biologic science. For nearly half a century Virchow continued to be the undisputed chief and the greatest authority on medicine as a whole in the civilized world; and it is no exaggeration to say that no other scholar in the history of medicine has exerted a like influence on the thinking of the medical world. As von Leyden says: "He impressed the stamp of his intellect on his time."

This sketch will serve to represent the history of medical thought from the incipiency of medicine as an art to the present day. We are now living in the era of the naturalistic, methodic, and anatomic thinking in medicine. What influence has this had on the artistic side of our profession, the clinic?

MODERN CLINICAL METHODS

It must be emphasized right here that at the time when Helmholtz and Virchow began to be active in Germany, the methods of the natural sciences had already taken root in Paris, Vienna, and in England. During the time that pathologic anatomy found its propounders in Cruveilhier and Rokitansky in Paris and Vienna, respectively, and at the time in which the clinic of Paris was under the inspiration of Laennec and the clinic of Vienna under Auenbrugger and Skoda, the German clinic was still dominated by the philosophic medical thinking. Von Leyden states that the language of the clinicians was Latin and that they followed the volitional deductive methods of Leibnitz, Kant, and Hegel. The first clinicians were Reil, Berend, and Bartels. Reil was a natural philosopher, and Bartels had been a teacher of anatomy in Hildesheim, while the best that could be said of Berend was that he was a "thorough connoisseur of the writings of Hippocrates." Hufeland, a clinician of genius and influence, who followed these men, showed *Wes Geistes Kind* he was by corresponding with the philosopher Kant on his work concerning "Makrobiotik," and expressed himself disparagingly concerning the new diagnostic methods of auscultation and percussion.

But, in 1844, a contemporary of Johannes Müller was called to Berlin, and to this clinician, Lucas Schönlein, is due the credit of having replaced the Latin by the German language in clinical teaching. He brought about the appointment of the very able younger clinicians, H. Simon, Remak, and L. Traube. With these men began the methods of exact physical examination of patients. Traube particularly was a master of diagnostic methods.

But when we look to the development of internal therapeutics at this stage of advanced clinical methods in Germany, it is discovered that the attempt to base therapeutics on pathology and diagnosis had failed. Therapeutic nihilism prevailed at Vienna, and the best that could be said of Berlin was that it worshiped the expectant plans of treatment. It took many years to impress on the clinician what now seems to be acknowledged a self-evident fact, that the only object of medicine is to help and to heal, and after the highly interesting evolution of the purely scientific part of medicine, briefly outlined in the preceding sketch, reformers were needed to emphasize again and again that, in addition to a science, medicine was also an art. Peter Krukenberg, a distinguished clinician of Halle, stated this in the following words: "Medical treatment, that is, therapeutics, is and will always remain an art." This conception of the clinical duties and responsibilities of medicine has been designated by Prof. I. Peterson[3] as "modern Hippocratism." The ultimate object of all medical studying is to help and to heal. The peculiar problem of the physician is not so much the disease, but the diseased patient; and the significance and importance of medicine is to be sought in this object, to protect the highest possessions of human beings, namely, life and health.

[3] Verhandl. d. Congress f. i. Medicin, 1899.

RECENT ADVANCES IN MEDICAL ART

A brief résumé of the advances in the art of applied methods of healing will give some idea of the progress that has been made in our art during the last two decades. I will begin with three methods of discipline, not as a rule classed among the applied therapeutics in the ordinary sense of the term. I mean, first, the physical methods of treatment; secondly, the treatment by diet; and, thirdly, the treatment by the art of nursing and the direct attention to the comfort of the sick.

Concerning the physical methods of treatment, I may say that, while they are by no means new, they have received such admirable and utilitarian additions and developments that modern physical methods of treatment almost constitute a new branch of applied therapeutics. The treatment by baths and water in general, balneology and hydrotherapy, the treatment by exercise and special apparatus for the correction and passive and active motion of diseased muscles, joints and bones, by gymnastics and massage. The wealth of methods by which electricity can be applied, and photo- as well as heliotherapy, the therapeutic applications of different kinds of light, has been endowed with such an abundance of new apparatus and methods that almost each one of them requires a specialist for their perfect application.

The treatment by diet has been placed on the safe and sound foundation of the physiology of nutrition. It has been aptly designated by von Leyden "Ernährungs-Therapie." Not only the selection of the quality and quantity of the food, but the manner of its preparation, the cooking, has received attention of medical men, particularly those whose attention is concentrated on diseases of metabolism and of the digestive organs. There are probably few healing methods by which more permanent good can be accomplished in the diseases just mentioned than by a scientific method of dietetic principles. It is much to be regretted that the art of cooking has not yet been universally admitted as an integral part of the curriculum in schools for girls and young ladies. It is unfortunate that in our well-to-do and even in the middle classes cooking is considered a humiliating occupation, a thing in which the modern housewife takes but a very superficial interest, leaving it, in the great majority of cases, to the cook whose experience and ability are often questionable quantities. The health of our people depends largely on the products of the kitchen, and, incidentally, the work of the kitchen is just as ennobling and honorable as work in any other occupation of life. Cooking should be taught in every nurses' training school; and this brings me to speak of the art of nursing, the direct and immediate service toward the comfort of the sick patient.

Nursing constitutes the ethical factor in the vocation of the physician. It is that part of the applied art of medicine which gives it the character of devotion to suffering humanity, and which invests the physician with the reputation of sympathy and love for his fellow-men and women. Nursing is that branch of applied therapeutics in which our sisters, wives, and mothers can join hands with the physician. It is individual and personal, it is the tie which binds the personality and individuality of the physician with that of the patient. Attention to the comfort of the patient and efforts at the widest application of the art of nursing afford also a common ground for two other much to be desired objects of our art: first, to secure for it the greatest possible support from philanthropic individuals and from the state and nation, and, secondly, to unite the more and more digressive specialties in medicine on the common ground of relief to human suffering. Here we have a tendency in which our claims for philanthropic and state support find a real and enduring sentiment and justification. For we can not claim this support for medicine as a science alone, but we can claim it for medicine as an art. For it is not exclusively the sciences which secure the blessings to the vocation of the physician, but it is the devotion to the relief of suffering, the readiness to help, the sympathy, and the kindness of heart.

Right here a word of caution will not be amiss in reference to the ultra-bacteriologic doctrines on the treatment of the sick. This caution has already been expressed by Prof. O. Rosenbach, of Berlin, viz., we must not let any possible fear of contagion convert our sympathy and love for the patient into fear of the patient. It can not be denied that from time to time a physician or a nurse may contract a disease from the patient. There is no doubt in my mind, however, that for every one of such cases of direct infection there are ten others in which nurses and physicians have actually been protected from the disease by virtue of their very intercourse with the patient. According to the ingenious theory of Ehrlich, very slight infection, sometimes scarcely noticeable by symptoms, may actually immunize an individual against a severe attack of that disease. It has been known to me, for instance, that a family in which there was a typical case of typhoid fever, the blood of the remaining members of the family gave the Widal reaction; yet to all appearances they were in a perfectly healthy condition. The antibodies were being produced in their circulation and tissue cells, preserving them from a severe attack of typhoid fever, not only for the time being, but doubtless for a long time into the future, and all this without even getting sick. Therefore, such experiences have led me to the opinion that intercourse with patients suffering with infectious diseases, especially with light attacks, may be able to protect and immunize the nurse and physician against infection. This does not by any means signify that the ordinary precautions against infection can be thrown to the winds. It is mentioned merely to strengthen the nurse in her devotion to duty and to avoid affecting her efforts by fear of contagion. It would appear then that the immunity thus acquired is due to a mild infection, with the specific organism.

RECENT PHARMACOLOGY

After the physical methods of treatment which I have mentioned, those offered by pharmacology rank next in importance. The products of the pharmacist and physiologic chemist have much enriched the armamentarium of the physician. Many new antipyretics have been brought out, medicines have been manufactured in a purer form, and older medicaments of approved value have been changed to a more pleasant form. New and useful medicaments for the production of sleep and the lessening of pain have been brought out. I greet it as a specially valuable advance that several remedies for the relief of pain, which hitherto could only be taken internally, and though very useful for the disease under treatment, yet frequently disarranged the digestive apparatus, can now be effectively administered externally through the skin. It has thus been found that very effective preparations of salicylic acid can be brought into a form which, when brought on the skin, will be so rapidly absorbed that the reaction for salicyluric acid can be discovered in the urine within half an hour of the external application. The art of general and local anesthesia has experienced wonderful improvements in apparatus, technic, and combination of agents.

Bacteriology has not only brought fertile ideas and facts into the science of pathology, but has opened an inexhaustible field for therapeutics. The discovery of the cure for hydrophobia and anthrax by Louis Pasteur and the discovery of tuberculin by Koch were soon followed by the antitoxic serum for diphtheria and tetanus by von Behring. In connection with these glorious achievements, serum-therapy began to develop as one of the methods of applied therapeutics. It is true that of the many serums thus far prepared only that for diphtheria and that for tetanus have proved generally useful. The fact that the cholera, plague, typhoid, scarlatina, antistreptococcus, and rheumatic serums have so far not proved as effective as those previously mentioned, need not discourage further efforts to perfect them; future research will produce the effective treatment.

The explanation of the difficulties of preparing effective serums is given by Paul Ehrlich in his ingenious theory on immunity which has brought the complicated phenomena of immunity nearer to our understanding. The study of the effect of bacteria on the human organism has led to the discovery of the protective substances of the blood, of the toxins and antitoxins, of agglutinins and precipitins, of the cytolysins. It is impossible here to enter on the consideration of that most wonderful and stimulating of modern medical hypotheses on immunity which has been developed by the genius of Ehrlich.[4]

I desire to call attention to a probable outcome of these studies on immunity, to which I have elsewhere referred,[5] concerning the rôle of intracellular catalytic processes in the pathogenesis of malignant neoplasms. If it be possible so to adapt the blood of an animal to a particular form of cell that its serum shall become specifically destructive of these cells, we have here a possibility for the preparation of serums which will be effective in restricting the growth and perhaps of locally destroying malignant and other tumors. My own studies on the transplantation of malignant tumors lead me to believe that, with a fuller knowledge of cytolysis, the outlook for such serums will be more promising.

Closely associated to serum therapy is the treatment of disease by extract of animal organs, which is gaining more and more in definiteness and exactness of application. Perhaps the most interesting of these substances is the active principle of the adrenal bodies, epinephrin, first isolated by John Abel of Baltimore. Thyroiodin and spermin belong to this class of remedies.

The astounding chemical versatility of the cells of the body, as evidenced in the production of such substances, and the immune bodies to which I have referred before, has forced on us a conception of the cell household as a complicated chemical laboratorium. Prof. Franz Hofmeister, of Strassburg, in his essay, "Die Chemische Organization der Zelle," conceives the cell as executing its work by manifold enzymes, each working alone in a special little cell compartment. Thus we have a biochemical conception of the cell processes. This is analogous to the conception of Ehrlich, on which he has built his side-chain theory of immunity. Benedict, Jacques Loeb, and perhaps Wilhelm Ostwald of Leipzig, have developed the biomechanical conception of the cell's work. Virchow's conception was, in principle, a purely anatomic one. Thus we have many varied conceptions of the manner and means by which the cell performs its regular and irregular processes. Though apparently widely digressing, it is encouraging to observe how many of the more important of the results of the biochemical, biomechanic, and the purely anatomic methods of thinking, concerning the cell, are beginning to overlap and even to agree with each other.

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[4] Aschoff, Ehrlich's Seitenkettentheorie, etc., Zeitschr. f. Allgemeine Physiologie, vol. i, No. 3, 1902; also Wm. H. Welch, the Huxley Lectures on Immunity, Science, Nov. 21 and 28, 1902; and a Résumé of Recent Researches Relating to Immunity, by T. Mitchell Prudden, N. Y. Med. Record, Feb. 14, 1903.

[5] Amer. Jour. of Med. Sci., April, 1903.

SUBJECTIVE AND OBJECTIVE METHODS IN RESEARCH

It might be argued that methods of thinking can not develop a science in themselves. This is true, for thinking should always be linked with observation and experiment. But, on the other hand, neither observation nor experiment alone can make a science. The formation of conceptions, however, from hardened facts of observation, and logical deductions of experiments, can form and develop a science. Unfortunately for our present methods of advancing medical knowledge, experiment alone has usurped the entire machinery of medical progress, and observation, as well as medical thinking, have been pushed to the background. It can not be sufficiently emphasized at the present time to use the words of Socrates, that "the conception is the fundamental condition of apperception and understanding," and Plato says of the sophists, "In the absence of a correct conception, they hold a learned discourse on the ass, when they mean the horse."

Very regrettable delays and even reverses in the progress of medicine have been experienced by the fact that one or another observer or investigator attempted to build his road into the unknown territory exclusively on one of the methods above outlined, namely, medical thinking, or observation, or experiment. One has criticized the value of thinking, the purely subjective method, another finds fault with the method of observation, a third applies his strictures to the method of experiment, the purely objective method. This has resulted in an accumulation of a vast amount of scientific building blocks, but there is no coherent, complete architecture in the modern science of medicine, and this is at least in part due to the perfectly unnatural separation of the three methods for medical progress. It is very rare to find in modern medical literature even an attempt at a complete scientific plan, and this is largely explained by the fear of most investigators to use the method of medical thinking, or the subjective method, in association with the objective method. Martius (Pathogenetische Grundgedanken) says: "It is a curious thing that the more scientific, the more exact the method of an investigation in medicine, the more inimical to thinking are its advocates." This is, indeed, curious. Why should exactness of a method be associated with hostility to our subjective powers of research, the power of thinking, as a help for the solution of medical problems? Darwin, Haeckel, Julius Robert Mayer, and Helmholtz were not only great investigators, but even greater thinkers. The century that produced steam railroads and the electric light gave us the law of conservation of energy by Mayer, and this law, probably the greatest discovery of the nineteenth century, is proclaimed by those most competent to judge to be the fruit of a purely thinking act, and that the experiment attached thereto was a sequence to the thought. Investigators who pursue objective methods of research, based on experiment alone, pride themselves on their exactness in methods. Exactness and accuracy constitute, in the opinion of the experimentalist, the only dependable means of securing correctness. But what is this exactness, by painful application of units of measurement, of units of weight, of instruments of precision, of accurately controlled conditions of arrangement in the experiment? What is all this exactness but frequently the translation of much complicated thinking into manipulations of science? The thought preceded the experiment. The subjective preceded the objective method.

I. P. Pawlow,[6] most scholarly of Russian physiologists, attacks the adherents of vitalism and animism in physiology in a recent contribution. These terms, vitalism and animism, refer to the tendency to return to the older conceptions in physiology, which attributed the phenomena and processes of life to a vital force," the "archeus" of Paracelsus, and the "anima" of Georg Stahl, a something which was not explainable by the laws of chemistry or physics. The application of chemistry and physics to physiology has enabled us to understand a great many of the processes of life, and to explain them on a purely chemical or physical basis. But some of the clearest thinkers in physiology admit that many of the processes of life can not be explained on such a basis, and they begin to favor views attributing such life phenomena to a special energy in the living substance (neovitalism). They who favor the vitalistic and animistic aspects of physiology are apt to confuse the standpoint of the investigator of natural science with that of the philosopher. The objective investigator has hitherto accomplished his most far-reaching results by the study and comparison of objective facts, during which he ignored questions concerning the inner subjective nature and the fundamental principle underlying the thing investigated. The philosopher, however, embodies the tendency toward synthesis of thought, and in attempting this he begins by a fusion of objective and subjective phenomena. For the investigator everything depends on the objectivity of the method. This gives the possibility of finding solid and unbreakable facts and principles. This is the view of Pawlow. In my opinion, subjective and objective methods of investigation are inseparable. Even where the investigator believes himself to be purely objective in his methods, he is unconsciously and unavoidably subjective, because the objective accuracy and precision of his methods represent but a translation, an outward projection, of his own subjective train of thoughts, and when his objective facts are established they must again be linked and amalgamated into his own thinking, or that of others, in order to be of value for the progress of knowledge.

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[6] "Psychische Erregung der Speicheldrüsen" (Ergebnisse d. Physiologie), 3d year, vol. I, p. 177.

DIFFERENCE BETWEEN TRUTH AND FACTS

We are living in a period in the history of medicine in which the experimental tendency has gained supremacy over speculative philosophy in medicine. But we are in the possession of such an enormous amount of new material and facts, which by additional experiments is daily increasing, that the new facts frequently must be allowed to remain unused, and are, for the time being, of no assistance in the advancement of our science. In this connection, I must again repeat what I have emphasized in the preface of the second volume of my work on "Diseases of the Intestines," namely, the difference between truth and mere facts. These two are often, unfortunately, considered synonymous. Facts are little truths that our senses are capable for the present of comprehending; but back of, and beyond these facts, later experience often reveals the higher and greater truth. An experimental fact which to-day seems absolutely disconnected and, therefore, without meaning, may to-morrow, when viewed in another light, suddenly assume a far-reaching significance and importance. No new fact of experience or experiment, be it at present apparently ever so remote from practical bearing, need be considered worthless, provided it is correct. It may be allowed to rest as raw material for a time, but it is probable that in another association it may acquire an importance which we did not anticipate.

But this I must emphasize, that an isolated, disconnected fact of experience or experiment has, for the time being, no significance for the progress of medicine. This significance comes only, then, when we can arrange and fix this fact into the already existing and firmly established architecture of our knowledge. There exists a danger in overrating the value of single facts of experience and experiment. Individual facts discovered this way are accumulating to such an extent that we are completely submerged under an ocean of experimental results, and the intellectual interpretation which fits them into the synthetic structure of our science is missing. Physicians who are not participants in experimental undertakings feel very painfully this absence of the connecting link between an enormous number of new acquisitions which, though experimental, are in a sense empiric. This is true of the experimental acquisitions in bacteriologic as well as biochemic domains. In the eighteenth and during the first part of the nineteenth century medicine was comparable to a sterile unproductive heath, in which some evil spirits drove about the speculating medical philosophers in a circle. Now we have gotten into an overfruitful swamp or jungle in which the facts grow so luxuriantly that they threaten to smother our thinking powers. The tendency of all laboratories is to bring out new facts. Let us have all of them if it must be, but what we need as much, if not more than new facts, are master minds who will instruct us in the interpretation of these and old facts, and give them a meaning and value by fitting them into the synthetic structure of physiology and medicine. Martius compares modern medicine to a sense-confusing concert, and what is needed is a disciplinarian to instruct us concerning the leading motives, to seek the familiar law in the revealed wonders of the present time.

> Aber im stillen Gemach entwirft bedeutende Zirkel
> Sinnend der Weise, beschleicht forschend den Schaffenden Geist
> Prüft der Stoffe Gewalt, der Magnete Hassen und Lieben,
> Folgt durch die Lüfte dem Klang, folgt durch den Aether dem Strahl
> Sucht das vertraute Gesetz in des Zufalls grausenden Wundern,
> Sucht den Ruhenden Pol in der Erscheinungen Flucht.
>
> —Schiller.

XIII

RUDOLF VIRCHOW'S ACCOMPLISHMENTS IN THE FIELD OF SCIENTIFIC ANTHROPOLOGY[1]

INTRODUCTION

SINCE it was indubitable that this assembly of distinguished clinicians was sufficiently acquainted with the medical, special, pathological, and anatomical accomplishments of Virchow, I have, at the request of our honorable chairman, undertaken to discuss a portion of Virchow's activity which seldom comes under the notice of the clinical worker and the practicing physician; it becomes therefore my agreeable duty to speak of the contributions made in the field of anthropology by this genius. I hope to be pardoned for omitting an exhaustive biography and bibliography. Most of the references will be found in the Reports of the Anthropological Societies, mentioned in the text of this discourse, or in the History of Pathological Anatomy by H. Chiari in Neuburger and Pagel's "Handbook of the History of Medicine," Vol. III, 536–541, or in a speech of Prof. Franz Boas, who occupied the chair of Anthropology at Columbia University in New York. From this latter (published in "Science," New York, 1902), I have borrowed many interesting points of view. This essay makes no claim to scientific completeness, but is designed to present on the occasion of this celebration one of Virchow's most important talents and thereby contribute to the comprehension of his wonderful versatility as an investigator.

RUDOLF VIRCHOW'S CONTRIBUTION TO ANTHROPOLOGY

In the death of Rudolf Virchow science lost one of its leading spirits, Germany one of her greatest sons and the world one of its most distinguished men. For sixty years Virchow dedicated himself, with eminent power and unwearying energy, to the service of mankind.

Medical, as well as anatomical, and anthropological sciences, point to him as one of its worthiest scholars. For years he exercised a powerful influence on the political life in Germany, where he maintained intact the principle of personal freedom.

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[1] Address on the occasion of the presentation of Virchow's bust to the Medical and Chirurgical Faculty of Maryland at Baltimore, Jan. 1, 1907.

The beginnings of his anthropological studies coincide almost exactly with the very first beginnings of modern physical anthropology in Germany. Among those who created this science no one contributed more to its inception, direction, and development than Rudolf Virchow.

His attention was drawn at that time to the science upon which he was to set the stamp of his personality, as a result of his investigations concerning the causes of cretinism and of his efforts to find ways and means of determining the decisive factors in the development of the skull. The similarity of pathological shapes of the skull and the shapes of the skull in different races likewise led him to investigations of the variations of structure in the human body.

The horizon of his anthropological interest soon broadened very rapidly and the impulse to far-reaching studies, which he gave especially in physical anthropology and in prehistoric archeology, was so great that one may say truly that the development of both these branches of science in Germany revolves about Virchow as its center.

At the time when Virchow took up his work, anthropology was still in its incipient stage. During the eighteenth century von Soemmering and Blumenbach in Germany, and Camper in Holland had turned their attention to the study of anatomical peculiarities of the human race, but the new anthropology did not make its appearance until the last half of the past century. The strong impulse which the theory of Evolution gave to all branches of science, together with the interest which the pre-history of the European races was awakening and the ever-increasing knowledge of hitherto little known races were the factors to which we owe modern anthropology.

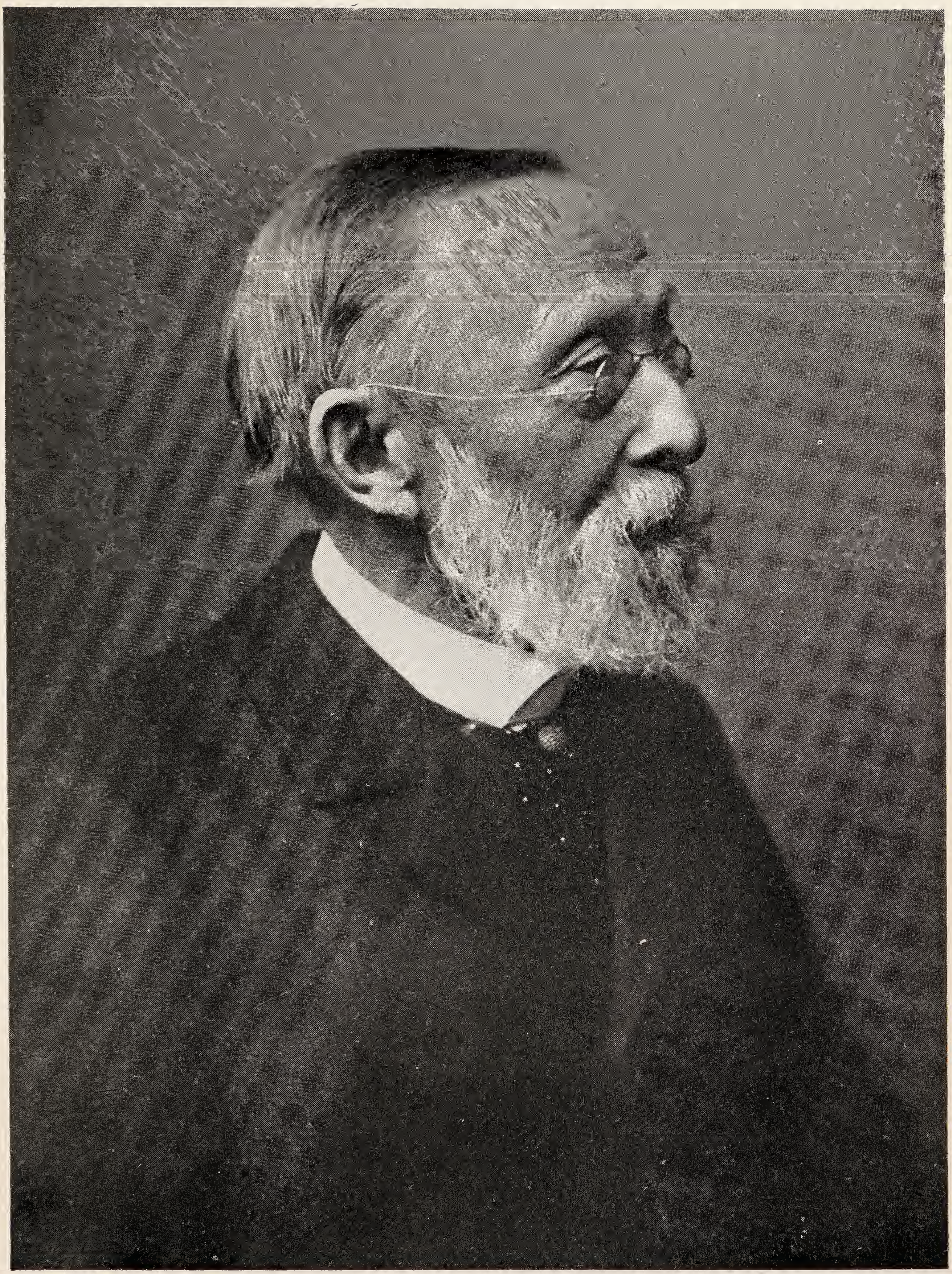
Virchow with his extraordinary gift for organization was destined to augment the domain of anthropology in all directions. He took a leading part in the creation of the German Anthropological Society, of the Berlin Society of Anthropology, Ethnology, and Pre-history, and in the establishment of the monumental Archive for Anthropology, which occupies a high rank in the field of anthropological literature. Both the societies soon became the centers of anthropological activity in Germany. The German Anthropological Society consecrated its efforts to the study of physical characteristics and to pre-history of the German race. Under Virchow's direction the Society undertook a collection of statistics concerning the distribution of skin, eye, and hair-coloring in Germany and investigations were carried on in all the public schools of the empire. The result of this extensive research, which included a cartographical representation of the distribution of types in Germany and a treatise concerning their probable history, was published by Virchow.

The Berlin Society for Anthropology, Ethnology, and Pre-history soon became a center, where a quantity of anthropological material from all the quarters of the globe was collected and where important scientific questions were treated by recognized authorities. By means of its intimate connections with German travelers, the Society contributed essentially to the development of the Berlin Ethnological Museum, the founding and importance of which is due to Adolf Bastian. Through Virchow's influence the Society came into the possession of a large and valuable collection of human skulls and skeletons. Among the subjects which were discussed, European archeology acquired an increasingly prominent place. Virchow had a large share in this work, which contributed essentially to the development of the prehistoric collections in Berlin.

As the director of the Pathological Institute and Museum of the Berlin University, Virchow had further opportunity to promote the study of racial anatomy. He collected much valuable anthropological material in this institution. His study of the prehistoric archeology brought him into touch with investigators of folk-customs and uses and in that way he became a co-founder of the Museum for folk-customs.

From all this it appears that Virchow occupied the first place in the organization of anthropological activity in Germany. It is therefore not surprising that his views have exercised a far-reaching influence, so far-reaching in fact, that one cannot understand at all the particularity of German physical anthropology and of German prehistoric archeology without being intimately acquainted with his work.

Of the greatest importance is his attitude toward the theories which have to do with the doctrine of heredity. His views on this subject were the result of his thorough investigations concerning the functions of the cell in the animal-organism. He expressed his views in the remark that every cell descends from another. No matter how the cells differ from one another in form, an older form determines each new one. Cells may, in the course of their existence, change their form either due to age or through other influences, which they encounter. Such changes take place in the healthy as well as in the sick organism and it is often impossible to distinguish between normal, or physiological, and abnormal, or pathological, changes. Virchow himself says that it is absolutely impossible to draw a dividing line between physiological and pathological processes, which take place under aggravating conditions. The cell, which changes its form during its life, may, for that reason, be already regarded as variable; in Virchow's own words, it possesses mutability.



Virchow

According to his theory much of the question of heredity turns on the problem of the connection between the variability of the organism and the variability of the cells. The comparative study of the forms of the organism and of the organs may be the point of departure for the investigations concerning the variability, but the study of the whole organism or organ must itself depend upon the study of the variability of the cells, since the physiological changes of the whole body depend upon the correlated physiological changes of the separate cells. Without a comprehension of the processes which take place in the changing cells, it is impossible to determine whether a deviation from the normal form was produced by secondary causes, which attack the *already fully developed* organ, or whether they were caused by primary changes, which take place *before the organ attains its first phase.*

From all this, two questions suggest themselves:

I. First, whether secondary changes can become transmissible. This is discussed in Chapters XXX and XXXII.

II. Secondly, whether primary changes actually take place and whether, if so, they are transmissible.

Starting with these views Virchow demands that investigations concerning the origin of species be supported by investigations concerning the mutability of cells and cell groups and therefore he refuses to undertake inquiries into the origin of species until a firm basis has been laid by the investigations concerning cell tissue. Sometimes it would seem as if Virchow cast doubt upon the scientific value of the theory of evolution. Such a conjecture cannot however be proven. He simply emphasizes again and again the methodological point of view, that the relation of the shapes of the body must rest upon the relation of the shapes of reciprocal connections and functions of the cells and that therefore the question of the mutability must be settled only by investigation of the kind referred to.

Moreover his position rests upon the generally accepted scientific principle that it is dangerous to attribute to facts, which one but partially understands, the value of general theories and that the true progress of a science depends upon one's keeping steadfastly in mind, in the presence of every scientific contribution, whether one is dealing with hypotheses or with facts which have been arrived at after exact observation. To this principle Virchow is so firmly faithful that some overzealous pupils and scholars even regarded his quiet, careful criticism as an obstacle to genuine progress. He found himself therefore exposed to many a bitter attack, until finally progressive investigation proved that the master was right in refusing to draw conclusions from inadequate proof. There are few scholars who possess such a quiet and comprehending bent for science that they are able instinctively to draw a sharp distinction between what is an attractive theory and what is an observation arrived at after long and tedious labor.

There are two anthropological problems which possess especial importance in their relation to the doctrine of evolution; the one concerns the age of the human race and the other concerns the interpretation of anatomical characteristics of the lower races. Knowledge concerning the anatomical formation of the earliest races is very slight. For years we have been occupied with the study of the Neanderthal skull which presents some noteworthy peculiarities, in particular a flat, shallow formation of skull and considerable prominences over the eyebrows. Virchow proved that the skull had undergone many pathological changes and he took the position that it would be imprudent to deduce from this single example a new race which then should be regarded as a precursor of the human race. He preferred to regard this skull therefore as a variation until further finds should offer confirming testimony. Virchow was also equally cautious in the interpretation of thermomorphic changes in the human body. He asserted that such forms do not necessarily point to atavism; that they can come from peculiar physiological processes and that, without special investigation of their origin, they are not to be regarded as proof of a lower organization of the races in which one finds them to-day. There are no proofs available that such forms go hand in hand with a lower civilization of the people among whom one finds them. One finds them, for example, among the Malays and among the old Peruvians, both of which peoples certainly attained a highly developed civilization.

It is impossible in this place and within the limits of this sketch to take up Virchow's many investigations in the field of the anatomy of the human races. Many contain explanations of general principles. Of quite especial importance, however, are his researches in the physical anthropology of the Germans and his descriptions of American skulls.

His investigations in the field of anatomical peculiarities of the Germans led him naturally to the study of prehistoric archeology, to which he devoted much time and ability. For a long time the body-structures had been considered as national characteristics. Skull forms were described as Teutonic or as Slavic; one heard also of Turanian and many other kinds of skulls. No one contributed more than Virchow to show that these assumptions were untenable.

Questions concerning the history of the Slavic colonies in East-Germany have been treated often by German archeologists and still much remains obscure concerning them. Although burial rites, prehistoric objects, place names, plans of villages and of houses point strongly to earlier Slavic colonies, it is impossible to draw a conclusion regarding the nationality of the original inhabitants by comparing the present anatomical structure of the population with the old skeletons, since neither the Germans nor the Slavs present a homogeneous or characteristic anatomical type. Virchow always declared that the boundaries between human types did not coincide with the boundaries between civilizations and languages. Members of a certain type may have different languages and different civilizations; and on the other hand, as it happens in Germany, different types are comprised in one nation.

These phenomena are intimately connected with the complicated migrations of the European races, with the military expeditions of the Teutonic peoples into the countries of Southern Europe and the development of a Northern European civilization with the influence of the Eastern Mediterranean civilization. The gradual introduction of metals and the disappearance of the civilization of the Stone-Age are phenomena of the greatest importance to the elucidation of the mutual relations of the different European peoples. The exchange of culture, indicated by the introduction of bronze, renders possible the theory that the new civilization sprang up in the far East. For this reason Virchow undertook his extensive prehistoric studies in Asia Minor and in the Caucasus. His studies in the field of prehistoric archeology, which are apparently so far removed from his first anatomical work, are in reality intimately connected with his investigations concerning the pre-history of the European races. Anatomical facts alone cannot solve these complicated problems and Virchow's extensive activity in the field of prehistoric archeology is a further proof of his thorough and comprehensive method, which uses all means in the solving of scientific problems.

Physical anthropology and prehistoric archeology in Germany have become what they are largely through Virchow's influence and work. His method, his views, his ideas were and are still the most prominent. His greatness as a scholar he owes to the rare combination of critical judgment, extraordinary insight, efficiency, and thoroughness united with an encyclopedic knowledge and the capacity to recognize correctly the unessential aspects of a phenomenon as such.

His critical judgment was so amazingly developed that he actually, in an address delivered in the summer of 1900, questioned the desirability of his enormous influence on public opinion.

With great admiration and profound gratitude we regard his life work, which defined the character of a new science.

In his classic work, "Greek Thinkers," Theodor Gomperz (Vol. III, 27) says:

"One can distinguish between two fundamental types of Savant. In the one, desire for complete knowledge, insatiability in the acquisition of ever new and manifold material, preponderates; in the other, the effort to attain inner harmony and absolutely logical thinking. There is however, obviously, only a difference in degree; neither of the two elements can have been completely lacking where eminent philosophic accomplishments have been attained. But the difference is none the less real. A Descartes or a Spinoza, adding stone upon stone in the erection of a harmonious temple of philosophy and a Leibnitz or an Aristotle, rushing indefatigably into isolated investigations of every kind, present two widely different subspecies of a common family. However earnestly the encyclopedist, absorbed in his endless studies of detail, may aspire to strict logic in his reasoning, his effort will never be crowned with as complete success as that of an equally notable intellect which is not driven by that urge toward universal knowledge and whose energy is, therefore, not dissipated by it. His impulse towards clarity will take on a peculiar character. He will contribute to the arrangement and classification of an enormous amount of scientific material. The encyclopedist will think out artifices for mastering this enormous material, for example Leibnitz's monadology, or he will become the highest type of classifying genius."

To which of these two types does Virchow belong? The more thoroughly we become acquainted with the intellectual development of this gifted scientist, the more insight we gain into the value of his accomplishments, the more we become convinced of the tremendous impetus which he gave to human thought and action, the more honestly must we acknowledge our inability to assign this man to either of the types described by Gomperz.

For we have before us a thinker, who unites in himself the characteristics of both types: (a) desire for complete knowledge, insatiability in the acquisition of ever new and manifold material and (b) the effort to attain inner harmony and absolute logic in his thinking.

In the present enormous development of the arts and the sciences it is probably impossible for the human race to produce a thinker, who, like Aristotle, would be capable of acquiring all the human knowledge of his time. Nor would such a thing necessarily be best for the development of the race. For Heraclitus expressed long ago and sufficiently clearly the undesirability of this achivement. In his sixteenth aphorism the Greek thinker says: *πολυμαθίη νόον ἐχειν ὀυ διδάσκει.* "Much knowledge does not bring insight (or comprehension)." The present condition of our intellectual attainments demands, as a condition of progress, the greatest possible concentration of mental energy, whereas the possession of a tremendous number of facts, the *πολνμαθίη*, leads to a dissipation of the intellect. And such a condition of the mind cannot possibly lead to the discovery of new truths, is indeed scarcely compatible with objectivity in detail work.

The amazing feature of the intellectual make-up of our modern Asclepiad and natural philosopher is the rare combination of fullness of knowledge and wealth of thought together with a distinct talent for classifying and objective observation. On the other hand one finds a facility in systematizing, in arranging and in the organization of scientific matter, which surpasses by far the ancient syllogism of Aristotle in objectivity and exactness.

In the life of Virchow one gains new confidence in the destiny of our race, and although the short span of human life offers for the individual but slight opportunity to progress toward the goal of perfection, yet such a life and such a work as his proves that human knowledge and effectiveness, as far as their capacity of attaining objective development and perfection is concerned, are unlimited ‘O *μὲν βίος βραχύς ἥ δέ τέχνη μακρα* (Hippocrates).

XIV

ALBRECHT VON HALLER

SCIENTIFIC, LITERARY, AND POETICAL ACTIVITY [1]

ALBRECHT VON HALLER was born October 16, 1708, at Bern, Switzerland. He was the fourth and youngest son of the attorney-at-law, Nicholas Emanuel von Haller. According to his own biography and the statements of his most reliable biographers, Ludwig Hirtzel and Jacob Baechtold,[2] he was possessed of extraordinary and precocious powers of observation, versatility in language, poetic talent, and unusual industry in collecting facts and objects. He is said to have been a very weakly, timid, and always seriously disposed child, and was taught by an old pedantic theologian, whose curious character aroused the poetic instinct of his tantalized pupil to satiric sallies.

Haller himself narrates to his oldest biographer that at the age of nine years, he had compiled an extensive lexicon of all the Hebrew and Greek words of the Old and New Testament, a Chaldean grammar, and between one and two thousand biographies of distinguished personages.

However incredible these statements may appear, Ludwig Hirtzel, who, according to my friend, Professor Henry Wood, formerly of the Germanic Department of the Johns Hopkins University, is an absolutely reliable biographer, gives an authentic poem of Haller's of the year 1721, which is an elegy on the death of Samuel Frisching. The poem was written accordingly when Haller was 13 years old, and in its title the poet calls himself a "poet who is a lover of virtue and disciple of wisdom." * Whilst this poem does not show the perfect rhythm and majestic sentiment of the poems of his later years, it is nevertheless an effort that must be considered genuinely imaginative.

In 1722 he left his native city and became a student under the physician John Neuhaus in Biel, who was a worshiper of the doctrines of Cartesius (Descartes), which, however, repelled his brilliant pupil.

At that time Haller was 14 years old, and now we are told of his first morbid inclination. He was continuously sick, avoided playmates, locked himself up for months and consoled himself with writing poetry in various languages. He wrote a long epic poem on the Origin of the Swiss Union of States, several tragedies, and translated Ovid, Horace, and Virgil. As another evidence of a morbid inclination, it may be cited that he once saved this mass of verse and literary compilation from a burning house at great risk, but later on burnt them up, part and parcel.

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[1] *The Johns Hopkins Hospital Bulletin,* March, 1908.

[2] Geschichte d. Deutschen Literatur in d. Schweiz, p. 489.

* Haller's daughter, Frau Amalia Haller Wildenstein, wrote a note on this manuscript stating that H. Albrecht Haller was only eleven years old when this poem was written.

In 1723 he went to the University of Tübingen, but was not well impressed with the restless student life there, nor made much progress in his special studies, and in April, 1725, he went to Holland to study under the renowned Boerhaave at the University of Leyden. A month before his departure, as Haller himself writes, he composed the beautiful hymn "Morning Thoughts." This is an apotheosis on the Omnipotence of the Creator and is undoubtedly one of the most impressive poems in the German language. A few lines to illustrate this poem may be pardoned:

Der Mond verbirget sich, der Nebel grauer Schleier
Deckt Luft und Erde nicht mehr zu;
Der Sterne Glanz erblasst, der Sonne reges Feuer
Stört alle Wesen aus der Ruh—
Durchs rote Morgentor der heitern Sternenbühne
Naht das verklärte Licht der Welt;
Die falben Wolken glühn von blitzendem Rubine
Und brennend Gold bedeckt das Feld.

An then our poet, addressing the Creator of Nature, continues:

Du hast der Berge Stoff aus Thon und Staub gedrehet
Der Schachten Erz aus Sand geschmelzt;
Du hast das Firmament an seinen Ort erhöhet,
Der Wolken Kleid darum gewälzt.

At that time Boerhaave was in the fullness of his power. The maturity of his broad experience undoubtedly laid the foundation of all the future works of Haller, but here Haller met two other men of scientific greatness; the younger Albinus, Frederick Bernard Albinus, a skillful and sagacious anatomist, who in 1745 became Professor of Anatomy; and also Ruysch, who was his teacher at 90 years of age. He took his degree of Doctor of Medicine in 1727 at Leyden on the basis of a thesis in which he exposed an error of Professor Coschwitz of Halle, who had maintained that he had discovered a new salivary duct of the submaxillary and sublingual glands, which Haller proved to be a vein. Thereafter he traveled extensively in England, visited Belgium and Paris in 1728, and studied mathematics in Basel with Bernouilli. In 1730 he returned to Bern and there practiced medicine and continued his researches in anatomy and physiology, spending his leisure hours in noting down poetic inspirations and making botanical explorations. In 1736 his fame had spread to such an extent that George II of England, who was also Elector of Hanover and Braunschweig, offered him a chair of anatomy, botany, and medicine at the newly-founded University of Göttingen. Haller accepted, and labored in Göttingen for 17 years, carrying out his most important inquiries and compiling most of his literary work. He founded the anatomical museum and laboratory, the botanical school and garden, and the obstetrical department at Göttingen. He was one of the founders of the scientific association and editor of its commentaries. Later on, he refused several calls to other universities, noteworthy among which was one by Frederick the Great, to a chair at the University of Berlin.

He returned to Bern in 1753, prompted partly by illness and partly by ambition for official station in his Fatherland. In Switzerland he passed the last 24 years of his life, taking his share of municipal and state duties. He eventually was elected a member of the great National Council of Switzerland.

Towards the last years of his life, persistent severe pain led him to the continuous use of opium. He passed away quietly on December 12, 1777. In the last moments of his life he had his fingers on his own pulse and said to his friend, who was standing at his bedside, "The artery no longer beats."

Haller was married three times. His most extraordinary versatility as a poet, litterateur, political economist, botanist, physiologist, physician and surgeon, has been the wonder of his many biographers.

He applied his tremendous intellect to the solution of many questions throughout physiology, and in the preface of the sixth volume of his "Elementa," he gives a list of what he claims as some of his own discoveries. There is no doubt whatever that he correctly recognized the mechanism of respiration, and his researches on the "formation of bone" and the "development of the embryo" are of the highest importance.

HIS LITERARY AND POETICAL ACTIVITY

In the year 1728, Haller, in company with a friend, traveled through his native country, which up to that time had been to him an unknown territory. The real object of this trip was not so much to observe the works of man as to get into communion with Mother Nature. In accordance with this plan he made a collection of rare specimens of the Swiss flora, and in fact everything, whether of high or low degree, became the object of his undivided admiration. Never did the uplifting thought leave him that God indeed had made everything beautiful, and devoted to some purpose in nature's economy. The glacier and the gentian, the tumbling brook and the dew drop, one and all, united our enthusiastic teleologist to God's service in His boundless temple of Nature.

The impressions made upon him were put into poetic form in the following year in a poem entitled "Die Alpen" (The Alps). Haller compared with biting sarcasm the low morality of his native town, Bern, with that of the older Swiss type, and thus emphasized his sympathy with Muralt's staid reformatory tendencies, revealed in the latter's "Letters about Englishmen, Frenchmen, and about my Journeys." He advises sufferers from the low morality of large cities not to seek recovery by going to Paris, but by travel to Switzerland where liberty and sincere morality prevail. Haller, the sentimental precursor of Rousseau, calls the people of rural Switzerland happy because of their ignorance about those evils which are the usual concomitants of growing cities. In his poem, in which he gives evidence of all that manly strength of which he is capable, and in which moreover he displays intense longing for pastoral quietude, he described the inhabitants of the Alps, not as peaceful Arcadian shepherds of the well-known stage type but as children of Nature, honest, unsophisticated people of the good old time. He praises their high ideals of matrimony, their harmless, good-natured festivities, their brawn, their various occupations as called for by the changing seasons, and brings all these things to the notice of the dwellers in cities. He pictures to us a beautiful landscape, surrounded by the Alps, which serve as a natural defense against the evil influence of the outer world. Winter approaches and you enter the hut high up in the mountains. Three generations gather around the fire-place—a young poet of nature sings his simple melodies, three older members follow him in turn, one speaking of the wonders of nature, and the others relating stories of heroism in the battles for independence, and praises:

> Tell who removed with intrepid courage the yoke which is still borne by half of Europe.

Thus from the Swiss mountains resounds first the battle cry "In tyrannos," which Schiller, Haller's successor in many respects, so frequently used, Goethe having previously made use of a democratic motto from Haller for his "Goetz von Berlichingen." Liberty and decent moderation are the unflinching precepts in a poem which forms the sentimental and purposeful supplement to his "Vitiated Morals," and the satire "The Man after the World." The young citizen of Bern thus described his countrymen:

No, surely 'twas not so before France got to know us,
Unknown to us were then the very names of crime;
Harmful extravagance our poverty withheld.
Sin in its wake was foiled by gentle singleness.
We had one fatherland, one God, and one free heart;
But now, alas—we fall!
The courage of our citizens which sanctified a state,
The marrow of our fatherland grows old and dies away,
And once again in history the world will surely read:
How states must go to ruin when moral law they will not heed.

Later on he renounced his praise of these Alpine children of nature, for it subsequently became evident that the youthful traveler had transported himself in his poetic fervor to a beautifully conceived past, or to a place that existed merely in his fancy when he said that the shepherds prefer the sparkling water from the mountain spring to the golden wine, or the son of nature despises the gold sand in his rivers: "The shepherd sees this treasure, he merely looks at it and lets it float away." This sentimental bias and pessimistic view of civilized life was supplemented, however, by a very practical common-sense view of nature on the one hand, and on the other by an exuberant optimisim of a religious character. Haller believed, like Leibnitz and Pope, "Everything which exists is good and for the benefit of humanity." Thus the Alps furnish us in their vegetation, with medicinal herbs, the mountains with crystals and curative springs, and the icy glaciers even are there for a purpose, in that they irrigate the surrounding country. In brief, the Creator has done everything for the best of humanity. But such doctrine made it incumbent upon him as a faithful follower of Leibnitz to work out a so-called theodicy, an explanation of God's indulgence in permitting the existence of evil at all. He good-naturedly pacifies us with the assurance that God's divine kindness will work out everything for the best, whereby humanity, "the pitiable, intermediate type between angel and beast," does not become the wiser.

During the first half of the eighteenth century one fanciful theodicy followed another. The fearful earthquake of Lisbon in 1755 gave the optimists a severe blow. Voltaire, Haller's opponent, in a poem devoted to this awful catastrophe, referred to the latter as a terrible argument against the above-stated doctrine. Voltaire himself, had deduced the existence of God from the established order of creation, without resorting to such ridiculously trivial teleology as to praise the creator of the cork-tree as the furnisher of the highly useful stopper. His often quoted saying, "If there were no God he would have to be invented, but all creation proclaims his existence," should be treated with as much consideration as Haller's maxim—"Enough! There is a God, for nature doth proclaim him."

(Psalm XIX). But when the best sections of a flourishing city go to ruin, this pious teleological hallucination is mercilessly scourged in the deliciously satirical tale, "Candide." Haller, who took pleasure in displaying his antagonism to Voltaire, had by that time already given up his poetic activity. One small volume of poems represents about the extent of it. It is this feature, in conjunction with an unusual intensity of thought, which gave to his "Swiss Poems" an epoch-making value. By way of contrast to a whole cluster of poetasters of the "quantity not quality" variety, a serious philosophic poet had made his appearance. These made rhymes after rhymes in quick succession; but he, conscientious about the wording of his verses and the creation of new forms wherever he felt the need of such, was content to complete about ten verses an evening. The former showered fulsome, insipid eulogies on their friends and patrons, but he pointed out the line of demarcation which separated a fine poem like his Nuptial Song to a Swiss Cato [3] from the usual cheap congratulatory poems. On the one side chaff, on the other side grain, although of a small quantity; on the one side dazzling raiment, on the other a heavy suit of armor; there prodigality, here economy, almost penury; there superficial amusement, here high ideals pertaining to life and its problems.

Haller was the first to impart force and depth to German poetry, even if he did not assist in bringing about facility of expression. One should read his truly great fragment "About Eternity," in which he portrays a desolate landscape, crags, sinister trees, a bird that has lost its way, and an idly-flowing brook—a spot where the lonely pilgrim directs his attention to the contemplation of eternity and is overwhelmed by the thought of a beginning without an end. But how can he, who is subject to finite conditions, comprehend the infinite:

On awful numbers I place numbers,
And millions of mountains I heap;
I roll cycles upon cycles and worlds upon worlds;
And when from this tremendous height
With trembling fervor I again thee seek,
O God! All might of numbers,
Increased a thousandfold,
Is not yet a part of Thee!

Kant cites these lines of "the most sublime German poet" in his essay on the "Infinite in Creation." To regard life from a pleasant point of view, to sing the songs of youthful gayety, was foreign to Haller's ponderous nature.* Inclined to loneliness, reserved and sensitive, lacking resiliency to experiences of a disagreeable character, he kept aloof, as he himself, stated from a real understanding of the joy of youth. Love was to him the most serious occupation to which he devoted himself. Although penurious in his lyric effusions relating to it, he however created in his "Doris" the ideal of womanhood; his deeply-conceived elegies on the occasion of the death of his first wife and also on that of his second were evidences of his intense feeling. With this tearful offering Haller bid the world of poetry farewell.

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[3] M. Porcius Cato, the elder, was noted as a rigid judge of morals, and this Swiss Cato must have been of the same character. Haller's Fabius and Cato is a historical narrative not a poem.

As a Göttingen professor, soon afterwards Albrecht von Haller became famous throughout Europe as the "Great Haller." The Academy of Berlin sought him, but the freethinking tendencies of Frederick's court were distasteful to the pious Christian. Possessed of immense learning, he devoted himself with indefatigable industry to scientific study. Histories of botany, physiology, and anatomy must each give him his due share of honor. An examination of the mural decorations of the exterior of the University of Vienna reveals his name as combining in one person the rarest abilities of the investigator and experimenter in the domain of natural science with an almost unattainable knowledge of litterature, and withal pervaded by an unusual sense of modesty. He was a veritable encyclopedia of information, "*πολνμαθιής*," wrote for many years reviews on books relating to all departments of knowledge, at one time appeared in the rôle of a theologian, at another as a politician, and in his last period became a statesman and administrator of public affairs. He rehabilitated the poetic and scientific fame of Switzerland. The very fact that a scholar of his type should not disdain to write a volume of poems, exalted poetry and the poetical profession in the estimation of the people.

CONTRIBUTIONS TO ANATOMY AND SURGERY

Albrecht von Haller must be mentioned as one of the first to investigate the etiology of septicæmia. He made the experiment of injecting putrescent substances into the veins of living animals, establishing the fact that they were killed thereby.[4]

In another direction he stimulated pure surgical research—namely, in an investigation concerning the development of a collateral circulation after the ligation of larger vessels—the larger anastomoses had in fact been made out by Haller.[5] Concerning echinococcus of the liver, Haller held the view that it was an exuberant formation of follicles.[6]

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* Except during his youth (see Albrecht Haller's Reisebücher 1723–1727).

[4] Friedrich Helfreich. Geschichte d. Chirurgie. In Neuburger and Pagel's Handbuch d. Gesch. d. Med., iii, 20.

The founder of the surgery of the diseased states of the biliary passages was J. L. Petit, who recommended puncture of the gall-bladder for advanced stagnation of bile, and the removal of gall-stones by incision; but to make both operations feasible, he postulated the necessity of adhesion of gall-bladder with the abdominal wall. The first to support the views of Petit were Haller and Morgagni.[7]

Haller described exactly the invagination of the colon into the rectum and gave its differential diagnosis from rectal prolapse.[8] In writing the history of hernia as a pathological entity, it should never be forgotten that Haller clearly described the peritoneal process extending into the scrotum as the persistence of a fœtal physiological formation; he opposed the view of Reneaulme, then prevalent (since 1721), that it was an abnormal place for the peritoneum, for he and J. Hunter demonstrated the protrusion of the peritoneum that preceded the descent of the testicle, and argued that in congenital scrotal hernia the intestinal loops must of necessity follow the same channel. Haller correctly described a large hydronephrosis discovered by him at autopsy.[9]

His principal achievements in anatomy were: 1, a demonstration that the "salivary duct," discovered in 1724 by Coschwitz, was a vein; 2, an investigation of the respiratory muscles and an exhaustive description of the diaphragm, with an interpretation of the intercostal muscles as elevators of the ribs; 3, a demonstration of the uterine musculature; 4, a demonstration of the coni vasculosi, Vasculum aberrans Halleri; 5, a correct description of the musculature of the heart and an accurate description of the pericardium and of the valves in the veins; 6, a description of a number of unknown or, at least imperfectly known, arteries (Tripus Halleri, triple branching of the cœliac artery, description of the course of the musculo-phrenic and of the internal mammary artery, anastomoses of the internal mammary with the intercostal artery); 7, the higher location above the pubes of the bladder in children; 8, a description of the omentum; 9, a demonstration of the Tela cellulosa as a connective tissue substance.

His successors in Göttingen were Johann Georg Roederer, 1726–1763; Johann Gottfried Zinn, 1726–1759, and Heinrich August Wrisberg. Zinn and Wrisberg, two undoubtedly brilliant anatomists, were direct pupils of Haller. Zinn has become immortal through his classical description of the eye and its surrounding Zonula of Zinn (ligament of Zinn). This illustrious pupil of Haller became professor of medicine and director of the botanical gardens in Göttingen, 1753–1759.

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[5] Loc. cit., p. 86.
[6] Loc. cit., p. 219.
[7] Loc. cit., p. 222.
[8] Loc. cit., p. 239.
[9] Loc. cit., p. 276.

The Swiss clinician, John George Zimmermann, was also a pupil of Haller. Both Zinn and Zimmermann assisted him in his investigations concerning the brain. Among the successors at Göttingen was Samuel Thomas Soemmering, 1755–1830, whose comprehensive textbook is a monument to the history of German anatomy. Soemmering was undoubtedly the most talented German anatomist at the beginning of the nineteenth century. Among the other intellectual descendants of Haller are Johann Friedrich Blumenbach, the founder of modern anthropology, and Johann Friedrich Meckel. Thus Haller's influence extended to the entire anatomical world of his period.

Concerning the functions of the Eustachian tubes, however, he did not recognize the full truth, for he believed them to serve the conduction of sound and not as Schellhammer experimentally proved in 1716 for ventilation of the tympanum.

Exact medical historic research must credit Haller with a valuable discovery concerning the anatomy of the eye, for he was the first to describe the "lamina cribrosa" at the entrance of the optic nerve into the eye-ball (see Comment in Boerhaavii prælectis, Götting. 1749), and he also, as well as Zinn, made clear the structure of the choroideal tract in the eye,[10] but he again failed to grasp the actual fact when he denied the existence of muscle fibers in the ciliary body and that they could influence the size of the pupil. But he shared this error with investigators no less noted than Morgagni, Zinn, and Fontana. Whilst he paved the way for a correct physiology of vision by his doctrine of the irritability of nerves, he seems to have failed in grasping what was known of refraction. As I understand Haller (Elementa Physiologica, Lib. XVI) he regarded light as a form of matter and ascribed to it a remarkable degree of substantiality.[11] Although he attributed refraction erroneously to contraction and dilatation of the pupil, his conception of the projection of the visible object on the retina was correct. It was a valuable service to the physiology of vision to emphasize the retina as the organ for light perception and not the choroid as Marriotte had argued previously.

Haller's analytical thinking and patient observation gave much new information in regard to neurological and mental diseases that eventually led to searching inquiries. In his Elementa Physiologiae,[12] he not only compiled that which was of interest up to that date, but added his own rich experiences and observations at autopsies. He emphasized that the brain was abnormal in all diseases of the mind and that the pathological alterations frequently extended to the spinal cord and nerves, and he asserted that if nothing abnormal could be detected in these parts in rare cases, it should not be concluded that they were normal, but he preferred to think that the disease process was located in the "finest organizations" of these parts or that perhaps the examination had not been exact or careful.

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[10] C. Horstmann. Geschichte d. Augenheilkunde, pp. 496 and 497.

[11] Vide supra, p. 499.

The history of the development of gynecology would not be complete without including his anatomical, physiological, and pathological contributions, which are embodied in part of the 28th book of the "Elementa Physiologiae." [13]

CONTRIBUTION TO THE PHYSIOLOGY OF CIRCULATION AND RESPIRATION

One of the most definite facts of this greatest of modern medical encyclopedists, and a fact upon which all later anatomists and physiologists agree, is that by his exhaustive work in anatomy and physiology references to literary sources earlier than his time have not only been greatly facilitated but in great part have been made superfluous; i. e., one need as a rule only refer to his writings to learn the views of his predecessors on any important fact in these subjects.

In his preface to his *Elementa* he opposes the separation of anatomy and physiology—for him they were inseparable and yet he himself was the first to make physiology independent by his objective way of thinking and the significance he gave to experiments on the living animal. Sir Michael Foster correctly says: [14] "When we turn from any writers of physiology preceding his time and open the pages of Haller's 'Elementa,' we feel that we have passed into modern times."

Microscopic work with higher powers was quite unknown to Haller, but all that could be observed by anatomical investigation simply,—even general histology,—whatever could be elicited by simple vivisection with the application of the means of stimulation known in those days—all that could be accomplished by these means, is described in his *Elementa* in words that even to-day retain their full meaning; his facts are critically weighed and he cites the complete literature known to him.

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[12] Lib. XVII, Sect. I, par. 17, Tom. V, Lausanne, 1763.

[13] Bernae, 1765; Muliebria: Sectio II, Uteri fabrica.

[14] Lectures on the History of Physiology, p. 207.

The finer anatomical and physiological techniques of to-day were not dreamt of by him, and those processes of living matter that could only be studied by the methods and in the light of the chemistry and physics of those days were necessarily incorrectly or incompletely understood. In a study of the dynamics of the circulation whose foundation was laid by Harvey, Bellini, and Borelli, or even earlier by Michael Servetus, Matheo Realdo Columbo, and Cesalpinus,[15] the work and views of Haller are noteworthy. He was the contemporary of the versatile English divine, Stephen Hales (1677–1761) a man whose mind was replete with original thought. Whose work on the physiology of plants and on hygiene is exceptionally meritorious, and in his work on "Hemostatics"[16] Hales describes his classical experiment of determining the hydrostatic pressure of the blood by tying a long straight glass tube into the artery of a horse. Singularly enough Haller, though familiar with this pioneer contribution, does not appear to estimate it sufficiently or correctly; in fact he does not cite Hales in his discussion of arterial pressure, although he makes use of his observations in his consideration of means "to determine the force of the ventricular systole," which perhaps was permissible in those days, for soon after, more direct methods became available for this study.

Haller discusses exhaustively the determination of circulation time. He denies the so-called self-regulating mechanism of the heart, the coronary circulation, and even disproves it. This controversy arose again in the nineteenth century between Hyrtl and Brücke.

In his description of the changes of the form of the heart during contraction and the cardiac impulse, he lays more emphasis on the changes of form than did Harvey. He shows familiarity with the influence of gravity and of the respiratory aspiration of the thorax on the circulation in the veins.

One of his most brilliant experiments as well as arguments is the demonstration of the automatism of the heart. Anatomists, physiologists, naturalists, and medical men in general at the time of Haller were under the ban of the doctrines of Georg Ernst Stahl (1660–1734), a brilliant metaphysical philosopher, and unfortunately for the science of his day, an influential writer and a man of exceptional individual force. He assumed that all physical and chemical processes in the living creature, even the very simplest, were fundamentally different from those in the lifeless world, in that they were induced and controlled by a "sensitive soul," the "Anima Sensitiva." This conception is entirely different from that of

[15] Hemmeter. Johns Hopkins Hospital Bulletin, 1905, XVI, 165.
[16] Statical Essays, Vol. II, 1732.

the "reasonable soul" of Descartes, by which this philosopher meant to differentiate man from animals. Stahl's "Anima" reminds one more of the "φυσίς of Hippocrates, or the "Archeus" of Paracelsus and of van Helmont, for it is present in all that is living and disappears from it when death occurs. Stahl was the first "Vitalist" and his "Anima" was inseparable from the central nervous system. It is evident from this definition that an organ that could function when entirely separated from brain and spinal cord was unthinkable to the medical world of Haller's day, when the doctrines of Stahl exerted a kind of tyranny over the opinions of physiologists. Now when the Bernese Aristotle with his then unassailable objective demonstrations and the force of his logic correctly proved the entire independence of the activity of the heart from the central nervous system—not only this, but even more, for he emphasized the irritability of the myocardium itself as the cause of the cardiac rhythmic activity,[17]—he dealt a killing blow, from which the hypothesis of Stahl could not recover.

As said before, Haller must be credited with the first correct and complete presentation of the mechanism of respiration, for there were other prominent anatomists, Hamberger (1697–1755) for one, who held that the lung contracted by its own inherent power, like a muscle, and that the pleural space between the lung and thoracic wall contained air. Haller succeeded in preparing the costal pleura in a living animal without injuring it in the least, so that the lung could be seen through it. He showed that the lung passively expanded, whilst it followed the receding thorax and descending diaphragm. His description of the diaphragm is anatomically perfect.[18]

It is regrettable that the chemistry of respiration was a "terra incognita" to him, who like many other thinkers of his time was held perplexed in the dark maze of Stahl's phlogiston theory. He knew of John Mayow's work on Respiration, and even cites it, but he failed to understand what Mayow meant by his "Nitro-Aereal" or "Igneo-Aereal" particles. The appreciative mind of to-day reading Mayow's ideas in the light of later progress by van Helmont, Lavoisier, etc., can understand that Mayow meant that the part of the atmosphere that was essential for burning was essential for all the chemical changes on which life depends and that the "Nitro-Aereal" particles of air are the oxygen of to-day. It is questionable whether any of Haller's contemporaries understood Mayow, or even whether this English apostle of physiology was understood in his own country. I do not think it correct to assert, as Sir

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[17] Elementa, Vol I, p. 488.

[18] De diaphragmate. Göttingen, 1791.

Michael Foster does,[19] that Haller rejected the advances of the English school (Boyle, Hooke, Lower, and Mayow). He gave them fair consideration; he did not accept them; neither did he clearly reject them. He was an agnostic for the time, as most conservative thinkers should be. One must not overlook the fact that these men expressed themselves largely in terms of their own invention concerning their views of respiratory gases, and that they were unavoidably vague, and were not in entire agreement. After discussing all views known to him, Haller declines to accept the view that particles of air actually pass into the blood; but immediately thereafter warns the reader that his doubt concerning the theories of the English school "does not or should not lead to the conclusion that in breathing we derive nothing from the air."

Haller induced his disciple Rhodes [20] to write a dissertation on the iron content of the blood (Göttingen, 1753) and he attributed the red color of the blood to the iron.

The physiology of the larynx is excellently presented in his *Elementa*,[21] but he appears not to have known the function of the lateral cricoarytenoid muscles in opening the glottis.

CONTRIBUTION TO THE PHYSIOLOGY OF DIGESTION

To the modern student of Haller's physiology it soon becomes apparent that the weakest points of Haller's work are where he comes in contact with purely chemical phenomena in the explanations of the processes of life, and this is not surprising, since the chemistry of to-day was practically unknown then. It was not until after Haller's death that Scheele, contemporaneously with Priestley, discovered oxygen in 1786. I have already referred to the retarding effect of Stahl's phlogiston hypothesis, which impeded the development of chemistry fully as much as the delusion that the septum of the heart is perforated did retard the development of the physiology of the circulation of the blood.[22] Even Scheele was an ardent adherent of the phlogiston theory. It is quite pardonable, therefore, that Haller, who had no special training in chemistry and had not even been an apothecary, like some of the clinicians and chemists of his time, should have no comprehensive knowledge of the chemical progress taking place during his life and which was giving birth to a new chemistry under the stimulus of Black, Mayow, and Priestley in England and Lavoisier in France.

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[19] Loc. cit., 230.

[20] Borettan, Gesch. d. Physiol. (Julius Pagel writes this name Rhades, in Part I of Neuburger and Pagel's Gesch. d. Med.)

[21] Vol. III, p. 366.

[22] Hemmeter. *Johns Hopkins Hospital Bulletin,* 1905, Vol. XVI, 167.

This same defect in interpretation we find in Haller's presentation of the functions of the digestive organs, where we find many erroneous conceptions; but his description of digestion is distinctly in advance of the time. The activity of the salivary glands as dependent upon nerve influence (irritation by smell or taste) is an idea that does not appeal to him, although he concedes a certain degree of irritability to the salivary glands. To him the saliva is neither alkaline nor acid, and he interprets its function to be merely a mechanical one to aid in the formation of the bolus and facilitate swallowing. He does not know its starch-digesting property, for ptyalin was not discovered until 1831 by Leuchs.

The glands of the stomach furnish only mucus according to Haller. The gastric juice is a kind of transudate from the arteries; it is neither alkaline nor acid, but neutral. Acid is not present in the stomach normally; if present at all, it is derived from abnormal decompositions of the gastric contents. In the sixth volume of his *Elementa,* page 57, he looks with disfavor on the use of the word "ferment," to explain the action of the gastric juice, which is, according to him, much assisted by the grinding and mechanical effects of the movements of the stomach.

The various functions of the pancreas are not known to him. Though he speaks of neutralization (Milderung) as being one of them, he does not mean neutralization of the gastric juice but of the bile.

The bile he considers to be an especially effective secretion for digesting the fats, which he declares are immediately emulsified by it. It is according to him not an excretion but a secretion and this he attempts to demonstrate by the effects of excluding bile from the intestinal canal. It was still thought by many physiologists that bile was produced in the gall-bladder, but he was an opponent of this belief, as he knew that bile is produced in animals who have no gall-bladder. In this connection he emphasized the importance of comparative physiology. In connection with his doctrines concerning digestion, his discussion of the foods and diet of human beings is very interesting, but naturally imperfect in the light of our present knowledge.[23]

His description of the structure and the gross functions of the kidneys is admirable, but the chapter on the urine (this being again a chemical subject) is in accordance with the deficient knowledge of those days.

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[23] "Elementa," Vol. VI, pp. 188, 258.

CONTRIBUTIONS, VIEWS, AND EXPERIMENTS CONCERNING THE CENTRAL NERVOUS SYSTEM

Haller's work on the brain and spinal cord will be better understood after his doctrine of irritability has been studied. In testing the irritability of the brain and its membranes, he frequently went too far, because of the incompleteness of his methods and crudeness of his instruments. A part of his first brain studies were upon the cerebral pulsations. In 1750 the Dutch physician Schlichting had shown that the pulsatory movements of the brain were caused by the activity of the heart and the respiratory movements, but at the same time had been led to assume a kind of independent brain movement. The question was taken up by the Paris professor Lorry, 1725–1786, who disproved the theory of an independent movement of the brain, but at the same time held that the brain pulsations were pathological. Haller [24] attributed the brain pulsations to venous stagnation. In his experiments on the gray cortex of the brain, he was assisted by his pupils Zinn and Zimmermann. The physiological techniques of these experiments were very faulty, viewed from our present standpoint. They made use of needles and sticks of wood saturated in acid, which were stuck into the gray matter. It is not surprising that they always obtained the same results, namely collapse of the animals, and convulsions—the well-known epileptic attacks when the cortex is stimulated too powerfully. He considered the gray cortex as devoid of sensibility and attributes positive sensibility only to the white medullary substance. Thus he is opposed to ideas of localization, the beginnings of which had already been made by other physiologists. He recognized that the cerebellum, by carefully conducted experiments, could not be proven to be an organ that is essential to life,—that is, not more so than the cerebrum; but he conceded the great importance of the medulla as a portion of the brain that was immediately essential to life. This concession was, however, forced from him by his brilliant pupil Zinn. Lorry had shown that the only locality in the whole central nervous system through which convulsions could be caused invariably—that is of course after clean and careful experimentation—was situated in the medulla oblongata. Isolated injury of this spot could cause death, according to Lorry, and Haller recognized this also. Another epoch-making discovery which he confirmed without reservation was the discovery of the contralateral innervation, by the French physician, Pourfour du Petit (1664–1741). This brilliant observer accurately described the crossed paralysis which occurred in trephined animals whose cortex had been injured on one side,

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[24] "Elementa," Vol. IV.

Haller

and then demonstrated the decussation of the pyramids. There had been some intimation that such a crossing of the motor fibers probably existed, for the ancients had already described autopsy findings in persons who had died of apoplexy which foreshadowed the great physiological discovery of Pourfour du Petit.

THE NEW CONCEPTION OF IRRITABILITY AS FIRST PRESENTED BY HALLER

In presenting this new aspect of living matter as first conceived by Haller it will be necessary to review the opinions held on this same subject by physiologists prior to him. Sir Michael Foster says: "And to call attention to the general view of Glisson (a pupil of Harvey) because this was the mother idea which led him to a special conception of the properties of muscular tissue, through which he anticipated modern teaching by nearly a hundred years. In his work on the liver, in discussing how it comes about that the bile is discharged into the intestines at certain times only, namely, when it is wanted, he shows that the gall-bladder and biliary duct bring about a greater excretion when they are 'irritated.' And he argues that they cannot be irritated unless they possess the power of being irritated. This power of being irritated he proposes to denote by the term *irritability*. And he develops this view again in his work on the stomach (De Ventriculo), published the year of his death, though completely written as early as 1662, but laid aside in order that he might devote himself to his work entitled *De Natura*.

"Thus it is undoubtedly to Glisson that we owe the first introduction not only of the word but of the idea of irritability, which, revived by Haller, as we shall immediately see, in the next century became firmly established in physiology and has played an important part in the development both of physiological and pathological views. Haller used the word in its narrower sense as the property through which muscle responds by movement to an external stimulus; since then it has been extended to mean response in any way, not by movement or change of form only, but by any kind of change, chemical change, change of growth, and the like. And it is worthy of note that Glisson from the very first used the word in its widest sense, distinguishing the various ways in which irritability may be manifested and the various agents by which it may be called forth."

It was perhaps by reason of the fundamental and highly philosophical character of Glisson's conception that it did not meet with immediate recognition. The idea had to be put forth in the narrower form, which Haller gave it, in order to be understood by physiologists; but to continue to use a comparison of Sir Michael Foster's: "Glisson's "irritability" and his notable experiment were, like Mayow's igneo-aereal spirit, forgotten as the seventeenth century passed into the eighteenth. We have to wait until the latter century, when the truth was brought to light again by the sagacious Haller in his views of nervous action and its relation to muscular contraction."

From this narrative it is evident how the merits of an older investigator may be overlooked in the future development of a new discovery. Glisson was undoubtedly correct in the broader conception and application of the term irritability. Haller's inseparable connection with this discovery is, however, justified through the large number of objective demonstrations and experiments by which he succeeded in proving this new quality of living matter. To us of the twentieth century it might occasionally seem as if irritability had from time immemorial been one of the primordial conceptions of natural philosophers; and yet this property which at the present day appears as one of the most natural attributes of living matter was not even dreamt of before the days of Glisson nor understood before the days of Haller. The introduction of the idea into physiology constituted as great an innovation and brought about as many reforms and advances in physiology as did the conception and development of the new physical chemistry of our present day.

The most correct course to pursue in speaking of Haller's views concerning irritability will be to quote his own words in his *Elementa*. "There is widely present not only in the animal, but also in the vegetable kingdom, a contractile force by which the elements are brought nearer to each other. This not only seems to be the cause of cohesion in general, but is rendered manifest by the fact that a fiber drawn out lengthways when let go very soon returns to its previous length." This is more properly the elastic force. Besides this there is a contractile force by which the tissues, dead or alive, shrink when treated in various ways, when for instance they are heated. A contractile force of such a kind is present in almost all animal tissue, unless it be very soft and pulpy like brain, or very hard like bones and teeth. But there is in addition a special contractile force proper to muscles alone. "In a living animal or one only just dead there very frequently appears spontaneously in muscular tissue a swift contractile movement by which the ends of the muscle are alternately brought nearer to the middle belly and then again recede from it. And even when this contractile movement does not appear spontaneously, it may be excited if a stimulus, such as pricking, or pinching, or some chemical substance is applied."

"Many writers consider this living contractile force as identical with the dead one just described as belonging more or less to all tissues." This view Haller discusses and concludes, "That muscular fiber is the only one which is moved spontaneously in the living animal, or is brought by irritants from rest to movement," and that "the living contractile force must be held to be distinct from the dead contractile force, since the two agree neither in the laws which govern them, nor in their duration, nor in their seat."

This force he calls "Vis Insita," the Inherent Force, and the tissues possessing it he calls after Glisson "irritable."

He then discusses whether this property of irritability is identical with that of feeling, and concludes that it is not. "There are many parts which feel, but which are not irritable, and in particular, a nerve, which is above everything sensitive, and yet possess no contractile force except that common one found, as stated above, even in dead things.

"Wherefore this force since it is different from mere elasticity and from that dead contraction which is common to all fibers, seems to constitute a special property, peculiar to the muscular fiber, and indeed to mark the character of that fiber, so that every muscular fiber is irritable, and on the other hand you may almost designate as muscular fiber everything that is irritable. It is, however, a force of its own kind, different from every other power, and to be classed among the sources of production of motion, the ultimate cause of which is unknown. This same force is inherent in the fiber itself and not brought to it from without.

"I (by my experiments published first in 1739, and again in 1743) differentiated this irritable nature on the one hand from a mere dead force, and on the other from the nervous force and from the power of the soul. I shewed that the movement of the heart and the irritable nature of the intestines depended on it alone. I confined it entirely to the muscular fiber, in which point the Batavian school does not agree with me, but they will I hope do so when they are willing to distinguish the contractile force common to all animal fiber from the irritable force proper to muscle alone. I also shewed that this force was something perpetually living, and that it often broke out into movement though no external stimulus, such as could be recognized by us, was acting. By a stimulus, however, it could at any time be called back from rest into action. In a movement produced through it, I distinguished between the stimulus, which might be very slight, and the movement called forth by the stimulus, which might be very powerful."

"Some," says he, "have wished to call this force the vital force, but this does not quite please me, since the force may for some little time survive the life of the body. Hence I prefer to call it the force inherent in or proper to muscle."

Further on he says: "Besides this force inherent in muscular fiber, another force is exercised in it, so much like the former that it alone has its seat in muscular fiber. But it is different from the inherent force inasmuch as it comes from without and is carried to the muscles from the brain by the nerves; it is the power by which muscles are called into action." This he calls the "Vis Nervosa." "It too may survive the death of the body, and in cold-blooded animals is of the same constancy as the inherent force; so that in such an animal recently killed, in which no sensation or voluntary movement remains, a muscle, provided it be moist and whole, is thrown into convulsions when its nerve is irritated. And the same is true of warm-blooded animals." Haller reached these conclusions by countless experiments upon animals, the results of which he reported to the Göttingen Scientific Society, under the title of "De partibus corporis sentientibus et irritabilibus."

In his *Elementa,*[25] he described exhaustively all older theories of muscle contraction and emphasizes that the nerve has no inherent contractility of its own, nor can it move actively in any way, as had been asserted before his time. He reports concerning all former conceptions of the nature of nerve function and declines to believe the electrical hypotheses that arose for the first time in those days, and appears more willing to accept a theory explaining nerve function by the flowing of an actual nerve spirit, not in a gelatinous substance as Borelli thought, but in actual tubes.

In observing the expressions of pain and the movements of defense in the animal during his experiments on irritability, he finds that the sensibility of the various organs depends upon their wealth of nerves. The muscles also possess sensibility, side by side with irritability. The tendons and joints, according to him, do not possess sensibility to a marked degree and the peritoneum very little sensibility. The incompletenes of his physiological technic brought it about that he frequently went too far, particularly with his experiments in stimulating the dura mater and pia mater, as well as the cerebral cortex.

A CHARACTER STUDY OF HALLER

To discern the intellectual peculiarity and the inner character of all human beings that have achieved greatness is always a difficult problem. One of the principal features in our philosopher and naturalist is his astonishing pleasure in detail and the second most prominent trait is his unusual joy and power of observation. Biographers frequently speak of the secret of the individuality, and correctly so. It is a secret, but not because individuality is the work of forces which are more subtile and puzzling than other forces. The riddle consists in the abundance and complication of the many coacting forces into which we can very rarely gain exhaustive insight. According to Theodor Gompertz (Griechische Denker) we might distinguish two fundamental types of world sages. In one there is a preponderance of thirst for fullness of knowledge and an insatiable reception of ever new and manifold material for a better insight. In the second, the most prominent feature is the striving toward inner freedom from contradiction, toward unconditional mental consequentialness, and consistency. Insatiable thirst for knowledge on the one hand, and the desire to have a mind free from contradictions, a so-called logical and orderly mind on the other hand, are not necessarily diametrically opposed to each other. The two represent evidently only differences of degree, but the difference is for that reason none the less real. For a critical study of the state of the inner mind of great human beings reveals the surprising fact that these two qualities are rarely present in one and the same individual. Thinkers like Descartes or Spinoza, who erect a homogeneous and uniform thought structure, stone for stone, idea upon idea, and two detail workers like Aristotle and Leibnitz, who are ceaselessly engaged in special investigations of every kind, represent two deviating varieties of a common genus; for the sake of argument, we may designate those engaged in restless detail work, the encyclopedists. This human mind may try ever so hard in seeking strict limitation and definiteness of his thought structure, for clearness in the inner arrangement of his mind, yet he will never be as successful as an equally versatile intellect that is not animated by so strong a desire for fullness of knowledge, and hence not equally distracted. Insatiable thirst for knowledge in the encylcopedist must, however, eventually bring about a tendency toward clearing up his thoughts. Such a man will serve the requirements for the arrangement and the systematization of an enormous mass of knowledge. The encyclopedist will meditate upon and invent artifices and tricks controlling the material of thought. An example of this we have in the language of concepts of Leibnitz; or the encyclopedist will become a classifier and systematizer, and as a matter of fact we find that all great human beings with a comprehensive grasp of knowledge tend toward this achievement. Classification and systematization were the great intellectual feats of Aristotle and Humboldt. But strangely enough the best biographers of both of these do not concede that they accomplished research work of enduring excellence. Aristotle was not a research worker in the modern sense of the word; his interpretation of actual facts is often willful and controlled by preconceived opinions. His interpretations show the inexhaustible resources of a head rich in inventive power, rather than the severe training of a mind that conquers its intuition and bows under the hard yoke of facts.

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[25] Vol. iv, p. 532.

In Haller, however, we find for once a human intellect eminently fitted to serve classification and systematization (he has an orderly, logical, or sequential mind) and an insatiable thirst for fullness of knowledge, and yet, it is conceded by the most capable judges of the present day, that he was a research worker "par excellence."

XV

THE UNITED STATES ARMY YELLOW FEVER COMMISSION OF 1900, AND THE DISCOVERY OF THE TRANSMISSION OF YELLOW FEVER BY THE BITE OF THE MOSQUITO "STEGOMYIA FASCIATA"

A SCIENTIFIC and medical discovery so far-reaching in the blessings it bestows upon the human race, that it is not exceeded in this respect by any other discovery in the history of medicine, has been made by three American Army surgeons. The names of these three men are Major Walter Reed, Major James Carroll, both Surgeons in the United States Army, and Dr. Jesse W. Lazear, Acting Assistant Surgeon, U. S. A. The fourth member of the U. S. Army Yellow Fever Commission was Dr. Aristides Agramonte, Acting Assistant Surgeon in the U. S. Army, a Cuban by birth and, as I was informed by former Surgeon-General, U. S. A., George M. Sternberg, he was an immune. Dr. Agramonte is the only one of the U. S. A. Yellow Fever Commission who is still living; he resides in Havana.

When we consider, that this Commission first came together in June, 1900, all of its members being then vigorous and healthy young men, and that the last American of this Commission, the third to die, departed this life in September, 1907, from indirect results of a severe attack of yellow fever, experienced seven years before, we will gain an insight into the severity of the duties of these philanthropic pioneers of American Medical Science. Dr. Jesse W. Lazear died on September 28, 1900. He had been stung on September 13th by a mosquito, which chanced to alight upon his hand. He believed this insect to be a "common ordinary brown mosquito," and not a "Culex Fasciatus," but as he permitted this insect to fill itself with his blood, there is no exaggeration in saying, that he knowingly and willingly accepted all the chances of a voluntary inoculation, which brought on a violent attack of yellow fever, that ended his highly useful life in a hospital at Quemados, Cuba. His infection by the mosquito took place in Las Animas Hospital, located in the outskirts of Havana.

Major Walter Reed, U. S. A., who was the head of the Yellow Fever Commission and acted as chairman and head of affairs, died on November

22, 1902, in Washington, six days after an operation undertaken for appendicitis.

Dr. James Carroll, Major and Surgeon, U. S. A., with whom this article deals more especially, died in Washington on the 16th of September, 1907, after a protracted illness, which had given manifestations of a more or less severe character for the previous six or seven years. In fact, Carroll told me personally during a banquet, which was given to Professor Paul Ehrlich, of Frankfurt a/M., by Professor William H. Welch, of Baltimore, "I have never been a well man since that awful attack of yellow fever in August, 1900."

We may justly assert then, in contemplation of such a colossal human sacrifice, that two of the three American members of this commission died as martyrs. The chief of the commission, Dr. Walter Reed, though he was fortunate to escape an attack of yellow fever, and as head of the commission was ordered not to submit personally to the sting of the yellow fever mosquito, as has been suggested by Professor Welch, yet when we reflect that appendicitis is frequently caused by certain chronic catarrhs and microbic diseases of the colon (acquired especially by a sojourn in the tropics, and by the constant change of diet, as well as the exposures of travel), we may, without much stretch of imagination, assume that the indirect cause of Reed's early death, was the result of his work and exposures as a member of the commission. It would appear then that in reality, all three died as martyrs.

No other such pilgrimage of three clinicians into a country, infected with a dangerous disease and sealing with their lives a brilliant discovery, is known to me in the entire history of medicine. On making this statement I do not depend entirely upon my own knowledge, for I have consulted many specialists on the history of medicine; nor is the achievement of these three men, considered either from the purely scientific or from the humanitarian aspect, exceeded by any other single medical discovery, except perhaps the discovery of anesthesia, which is also an American achievement, or the discovery of protective vaccination by Jenner, or the Bacillus of Tuberculosis by Koch. The weight of this assertion may be better understood by reading an article published by Reed and Carroll in the New York *Medical Record,* October 26, 1901, entitled "The Prevention of Yellow Fever." From the statistics there collected, covering a period from 1793 to 1900, there have been not less than 500,000 cases of yellow fever in the United States within that period.

In addition to this we cannot disregard the loss to commerce, interstate and international transportation, and the direct financial losses. It is stated by Horlbeck, Chairman of the Committee appointed in 1897 to investigate the cause of Yellow Fever, that the total loss to the country caused by the epidemic of 1878, was not less than $100,000,000.

When Karl Lamprecht in his *Americana* (Freiburg i/B) states that all the achievements of the American Nation could be wiped out, as if they never had occurred, and still the human race would be none the worse, he makes a disparaging assertion, not in agreement with the great historians of his own country; but certain it is beyond all doubt, that the work of the U. S. A. Yellow Fever Commission has produced a result, viz., the discovery of the transmission of yellow fever by a specific mosquito; the blessings of this discovery will last forever.

The facts narrated in the following were obtained largely from Major James Carroll himself during many interviews, from General George M, Sternberg, Dr. William H. Welch and from the articles given as references in the text.

For a biography of Major Walter Reed and his scientific work, the reader is referred to the volume by Dr. Howard A. Kelly, entitled "Walter Reed and Yellow Fever," New York, 1906. About ten pages of this book are devoted to a brief sketch of the life and work of Major Carroll, and about eight pages to the much beloved and lamented Dr. Jesse W. Lazear, who was a Baltimorean by birth, and a man of exceptional personal magnetism, in addition to possessing rare scientific versatility.

Before and during the year of 1897 there were two views proposed regarding the infectious or causative agent of yellow fever: One was by Dr. (then Major-Surgeon) George M. Sternberg, who in 1897 was a member of the first Yellow Fever Commission appointed by the United States Government. For years Dr. Sternberg, later on Surgeon-General of the United States Army, had made systematic investigations of the bacteriology of yellow fever, in New Orleans, Vera Cruz, Rio de Janeiro, and other harbor cities of South America. He was unable to continue his research after he entered on the duties of Surgeon-General, but as a result of his work up to that time he gained the view, that an organism, the Bacillus *"X,"* was the infectious agent. The second organism proposed as the cause was the Bacillus Icteroides of Sanarelli (*Semaine Médicale,* Paris, 1897, XVII, p. 253).

Professor William H. Welch, of the Johns Hopkins University, has expressed his views concerning the work of Surgeon-General Sternberg, done since the first Yellow Fever Commission was appointed in 1879, as follows, in the *Medical News,* June 21, 1902, p. 1198:

"His work with yellow fever would stand forever. He said it was a common thing in these busy days to forget the steps, which led up to an important discovery. All that Dr. Sternberg had done in the study of yellow fever was necessary work, and it had to be done just in the way, that he did it. The ground had first to be cleared; if it were not so the discovery had not been possible, and later discoverers themselves would have had to hunt out the large host of microörganisms, which Dr. Sternberg had described and laid aside. His careful work practically resulted in the view, that a bacteriological origin of this disease could not be claimed, and it was on 'a priori' grounds, that he himself felt, that Sanarelli's bacillus was not the cause of yellow fever. His study of other discoveries was most careful and most critical; it was not wasted endeavor."

In the demise of Dr. James Carroll, Surgeon-Major, U. S. A., September, 1907, there was lost to Medical Science a most brilliant investigator. Among the many useful men, that ever lived, there stand few, whose immense contributions to the welfare of mankind equal that of James Carroll; indeed it can be said, that as an example of keen insight, indefatigable energy and superhuman generosity, this man was peer to any medical pioneer.

The efforts by which he came to acquire the technical knowledge, which made him famous, are best set forth in a plain simple statement of his early life; and then all may know the ineffaceable trait of perseverance, which characterized every effort in his worthy life, the acceptance of every duty as it came, and the fullest discharge thereof, with constant steady strides towards his goal.

Major Carroll was born in Woolwich, England, June 5, 1854. His parents were James and Harriet Carroll. His early training was at the Albion House Academy, Woolwich, England, until 1869. In 1874 he enlisted as a private in the U. S. Army, and was progressively promoted to Corporal, Sergeant and Hospital Steward. Here was created his desire for the study of medicine, and by dint of hard work and perseverance he received the degree of Doctor of Medicine at the University of Maryland, Baltimore, in 1891. In 1888 Major (then Sergeant) Carroll married Miss Jennie M. George Lucas, of Cleveland.

After his graduation at the University of Maryland, Baltimore, in 1891, Dr. Carroll studied Pathology and Bacteriology in a special course of postgraduate work under Professor William H. Welch at the Johns Hopkins University until 1893. During the summer of this year he worked independently at the World's Fair in Chicago, in the Army Laboratory, and it was not until September, 1893, that he met Dr. Reed, in the Army Medical School, Washington, D. C., where he reported by order of Surgeon-General Sternberg, U. S. A. At this time Dr. Carroll was Lieut. Assistant Surgeon, U. S. A., and in this capacity his first great work began. He was first ordered to assist in the investigation of the statement of Sanarelli, that he had demonstrated the specific etiologic organism of Yellow Fever. This work was done in Cuba in 1897–1902, and with Reed and others he determined conclusively the error of Sanarelli, and at the same time disproved Surgeon-General Sternberg's theory, that the "Bacillus X" was the specific causative factor. "Bacillus X" of Sternberg was found to be an atypical Colon Bacillus, and that of Sanarelli, the "Bacilles Icteroides," to belong to the hog cholera group. Concerning the publication of these findings, the "Medical News," April 29, 1900, editorially says: "There remains open for ambitious American bacteriologists a very interesting problem of etiology (referring to the cause of yellow fever), whose study the United States' possession of Cuba and Porto Rico will greatly facilitate, and whose importance can scarcely be overestimated. Any discoveries in the matter will confer lasting fame upon investigators."

The report of Drs. Eugene Wasdin and H. D. Meddings, of U. S. Marine Hospital Services in an official report (Report of Medical Officers detailed by authority of President McKinley), adopted the ground occupied by Sanarelli, and proclaimed in their report: "That the micro-organism, discovered by Prof. Guiseppe Sanarelli of the University of Bologna, Italy, and by him named the 'Bacillus Icteroides,' is the cause of yellow fever."

The work at Quemados, Cuba, of Major Reed, Major Carroll (then Lieutenant), Dr. Jesse W. Lazear and Dr. Aristides Agramonte, appointed at the suggestion of Surgeon-General Sternberg, as a Board of Army Medical Officers, established by repeated autopsies, that the "Bacillus Icteroides" was, when present, a secondary infection in yellow fever, but was present in not a single case of repeated examinations of the blood of living cases of yellow fever, nor in the organs at autopsy of fatal cases of a long series, examined by them. "Therefore," said they, "Bacillus Icteroides stands in no causative relation to yellow fever, but when present should be considered as a secondary invader in this disease."

At that time there stood rather prominently before the medical profession repeated assertions by Dr. Carlos J. Finlay, of Havana, Cuba, that the mosquito was a possible agent in the transmission of yellow fever. Dr. Finlay had no well defined idea as to the relation of the disease and the mosquito, but suggested, that yellow fever was principally due to an inflammation of the endothelium and intima of the blood vessels, and that the penetration of the proboscis of the mosquito into these vessels resulted in infection of the proboscis, and then an infected mosquito might, by biting a non-immune, mechanically transfer the infection to the party thus bitten.

Drs. Reed, Carroll, and Lazear, then (1900) stationed at Columbia Barracks, Cuba, directed their attention to Dr. Finlay's theory (advanced nearly twenty years previously), as nearly all of Dr. Finlay's experiments to sustain his theory had been performed in such a loose manner, that it was not proper to attach much importance to them (because his results might have been coincidental as well as rational). Certainly nothing had been proved. Professor William H. Welch is of the opinion, that Dr. Carlos J. Finlay's efforts to prove his hypothesis were so contradictory, that they tended rather to invalidate than strengthen his assertions.

Now it appeared to those, who frequently and freely discussed the possible theories of yellow fever infection, that the disease of yellow fever bore a marked relationship to malaria in that it occurred only when mosquitoes were present, and under such conditions as suggested, that the organism, whatever it might be, was possibly like the malarial one, in that it underwent apparently the same or a similar life cycle with the mosquito as intermediate host. To no one of these in particular can all the credit for this idea be given, as it seems, that the frequent daily interchange of ideas was so close, that the resolution to pursue this line of investigation was really an offspring of their conjoined suggestions. Medicine owes to the brilliant discoveries of Sir Patrick Manson and Major Ronald Ross, of the British Army, the first definite knowledge of the part played by mosquitoes in the transmission of malaria (see Nuttal's paper, Johns Hopkins Hospital Report, 1899; also the work by H. A. Kelly, "Walter Reed and Yellow Fever," p. 104–108). Dr. Agramonte, who was stationed at Havana, shared in these deliberations to investigate in a systematic manner this method of infection. The primary step of this method was to test, whether or not the disease could be transmitted by the mosquito. The solution of the problem was determined upon, and with this determination arose the resolution to use human beings as experimental objects. In order to justify such a responsibility, the commission, at Carroll's suggestion, determined to share the tremendous risk of self-sacrifice along with whomsoever they could secure among the non-immune for such dangerous experiments.

At this time, August 4, 1900, Major Reed was recalled to the United States from Cuba, and new responsibility, both moral and medical, fell upon Dr. James Carroll. Doubtless, had Dr. Reed remained upon the island, he too would have offered himself, as did his colleagues, upon the altar of self-experimentation in their pursuit of such much needed knowledge. He was recalled to the United States, when these tremendous strides towards the light, which revolutionized prophylaxis and hygiene as concerns yellow fever, were made. Two years later, on November 22, 1902, that most highly esteemed investigator, Major Reed, fell a victim to a fatal attack of appendicitis, and his death, occurring in Washington, D. C. at the time that his work was going on, and closely following that of the martyr Lazear, and the illness of Major Carroll, led many of the laity to believe, that Reed died as a result of yellow fever, inflicted upon himself in the interest of humanity. Such inoculation is suggested by Samuel H. Adams' article in "McClure's Magazine" for June, 1906, where he says, concerning yellow fever, "Lazear died a martyr to humanity and is remembered by one, when the lesser heroes of our Cuban battle-fields are acclaimed by the thousands. Carroll barely escaped with his life, and Major Reed, shrinking from no peril, which his companions braved, came through unscathed, by virtue of some natural immunity, only to die of another illness the following year." The actual fact, however, remains that, virtually, Carroll was the hero of the hour. It was he, who first proposed experiment on himself; it was he, who offered his arm in the laboratory on August 27, 1900, and permitted Dr. Lazear to apply to it a mosquito, that had previously been known to have bitten four severe cases of yellow fever; it was he, who was at the helm during the crucial hour of the trial; it was he, who first actually demonstrated by positive proof, that yellow fever is transmitted by the "Stegomyia Fasciata"; it was he, who later on prevented acceptance of erroneous ideas concerning the specific organism. Although these experiments were continued in a large number of cases after Dr. Reed returned to Cuba to carry out the work, which Drs. Carroll and Lazear had begun, Major Reed did not assume the risk of auto-inoculation. In explanation of this, it has been suggested, that he had orders not to do so, as nothing could be further accomplished by such human sacrifice of the Chief of the Commission. So really this most herioc act of self-sacrifice was the distinction of Carroll, and as his example was so conspicuous, it can scarcely be denied, that it served in no small way as an incentive to the many other offers of acts of philanthropic patriotism in the Army by way of volunteering for this dangerous service. In truth may it be said, that without this almost incredible self-sacrifice of Carroll, the experiments would have been so hampered, that it is doubtful whether any positive conclusions could have been attained.

The reports of the United States Yellow Fever Commission, because of army rank, bear Dr. (Major) Walter Reed's signature as senior officer; and such he was, but Carroll was an able second, whose marvelous heroism and self-sacrifice, whose persistency, accuracy, indefatigable energy stood ever ready to execute his superior officers' orders and, as we shall learn, brought them to a brilliant and successful termination—without detracting in the slightest, however, from the most valuable guidance of Reed, and assistance of Lazear and Agramonte.

When Major Reed left Cuba for the first time after the initial visit of the Commission to this island (Aug. 4, 1900), Carroll and Lazear began their systematic investigation of Finlay's theory, that the mosquito was the source of infection. Dr. Finlay furnished the eggs, from which were hatched the mosquitoes first used in these experiments. These mosquitoes were kept until maturity free from any source of contamination. The men, who were to be experimented upon, were likewise kept in quarantine, so as to guard against other sources of infection (vomits from yellow fever patients being then regarded as a very dangerous source of infection). Then Dr. Lazear infected nine healthy non-immunes, including himself, with these mosquitoes, which had been allowed to bite yellow fever patients. But either because the mosquitoes had not been kept sufficiently long after their having thus bitten these patients, or had bitten them too late or too early in the course of the disease, no experimental cases of yellow fever developed from these experiments. Dr. Carroll, nothing daunted, then had one mosquito bite a severe case of yellow fever on the second day of the disease, and afterwards three others at intervals of 6, 8, and 10 days, and then allowed Dr. Lazear to apply the mosquito to his own arm, *causing the first experimental form of this disease to be produced,* August 27, 1900. The illness of Dr. Carroll was that of a typical severe case of yellow fever, charts of which can be found in his subsequent reports. (See American Public Health Association Reports 1900.) Throughout this critical illness of Dr. Carroll, Surgeon-General Sternberg was considerate enough of Mrs. Carroll and her five children in Washington to keep them posted of Major Carroll's condition by daily cable. Four days after Major Carroll's infection, Dr. Lazear, with four infected mosquitoes (one of which being the same that was used upon Dr. Carroll), succeeded in producing another case, in a private soldier, who, with full knowledge of the danger, and with free consent, allowed himself to be used in the interest of these experiments.

The following is quoted from "A Brief Review of the Etiology of Yellow Fever," *New York Medical Journal and Philadelphia Medical Journal* (consol.), February 6 and 13, 1904, by James Carroll:

"The insect, which had been hatched and reared in the laboratory, had been caused to feed upon four cases of yellow fever, two of them severe and two mild. The first patient, a severe case, was bitten twelve days before; the second, third and fourth patients had been bitten six, four and two days previously, and their attacks were in character, mild, severe, and mild, respectively. In writing to Dr. Reed on the night after the incident I remarked jokingly, that if there were anything in the mosquito theory I should have a good dose; and so it happened. After having slight premonitory symptoms for two days, I was taken sick on August 31st, and on September 1st I was carried to the yellow fever camp. My life was in the balance for three days, and my chart shows on the fifth, sixth and seventh days my urine contained eight-tenths and nine-tenths of one per cent of moist albumen. The tests were made by Dr. Lazear. I mention this particularly, because the results obtained in this case do not agree with the twentieth conclusion of Marchoux, Salimbeni, and Simond, that the longer the interval that elapses after infection of the mosquito, the more dangerous he becomes. Twelve days, the period above cited, is the shortest time, in which the mosquito has been proved to be capable of conveying the infection. It is my opinion, that the susceptibility of the individual bitten is a much more potent factor in determining the severity of the attack, than the duration of the infection in the mosquito, or the number of mosquitoes applied. On the day that I was taken sick, August 31, 1900, Dr. Lazear applied the same mosquito with three others to another individual, who suffered a comparatively mild attack, and was well before I left my bed. *Thus it happened, that I was the first person, to whom the mosquito was proved to convey the disease.* On the 18th day of September, five days after I was permitted to leave my bed, Dr. Lazear was stricken and died in convulsions just one week later, after several days of delirium with black vomit. Such is yellow fever."

Dr. Lazear, while attending to his duties in a yellow fever hospital, was accidentally bitten by a stray infected mosquito (he, while sick, called it an "ordinary brown mosquito"), and fell prey to the disease on September 25, 1900. The attempt to give due credit to the magnificent altruism and genius of these men would lead me to falter in the effort to eulogize their work.

But Dr. Henry D. Holton, in American Public Health Associations, Presidential Address of 1902, struck a sympathetic chord in the hearts of all humanitarian Asclepiads, when he says:

"The patriotism of the military as they spring to the defense of their country, always deserves and receives the applause of the populace. Their deadly conflict on the battlefield is made easy by martial music, the booming of artillery, the rattle of the infantry fire, and the advancing step of comrades. How much more should we recognize the course of such devotees of science as Drs. Carroll and Lazear, who, filled with great philanthropic love for humanity, calmly, quietly, without the cheers or even the knowledge of the multitude, silently submitted themselves to the test to determine in what way this pestilence was communicated. We are told, 'Greater love hath no man than this, that a man lay down his life for his friend.' We find, that James Carroll and Jesse W. Lazear, fired and impelled by their great love for their fellow men, did offer their bodies as a sacrifice upon the altar of scientific investigation to the end, that in the years to come, hundreds of thousands might escape this pestilential death. . . ."

The practical result of all this work and sacrifice has been that not a case of yellow fever has originated in Cuba since that period. The quarantine period has been shorter by three months, thousands of lives and multitudes of treasures have been saved, and a feeling of security has filled the communities of the southern portion of the United States.

Dr. Carroll, after his illness, returned to the United States, to recuperate his health, and remained until the middle of November, 1900, when he again went to Cuba to continue his work. In the meantime, basing his knowledge upon Carroll's experiments, Dr. Reed published the second conclusion of the Army Commission, i. e., "The Mosquito serves as the intermediate host for the parasite of yellow fever." Amer. Public Health Assoc. Meeting at Indianapolis, Oct. 22–26, 1900.

Upon Dr. Carroll's return to Cuba, he established a camp near Quemados and named it, in honor of his dead comrade, "Camp Lazear." Here he continued his investigations as to the etiology of yellow fever, and in February, 1901, the commission was able, by such investigation, to establish further valuable data, published as conclusions in "Additional Notes": On the etiology of yellow fever by Reed, Carroll and Agramonte (Journal Amer. Medical Assoc., Feb. 16, 1901).

"1. The mosquito—Culex Fasciatus—or Stegomyia Fasciata, serves as the intermediate host for the parasite of yellow fever.

2. Yellow fever is transmitted to the non-immune individual by means of the bite of the mosquito, that has previously fed on the blood of those sick with this disease.

3. An interval of about twelve days or more after contamination appears to be necessary before the mosquito is capable of conveying the infection.

4. The bite of the mosquito at an earlier period after contamination does not appear to confer any immunity against a subsequent attack.

5. Yellow fever can also be experimentally produced by the subcutaneous injection of blood taken from the general circulation during the first and second days of this disease.

6. An attack of yellow fever produced by the bite of the mosquito confers immunity against the subsequent injection of the blood of an individual suffering from the non-experimental form of the disease.

7. The period of incubation in thirteen cases of yellow fever has varied from 41 hours to 5 days and 17 hours.

8. Yellow fever is not conveyed by vomits, and hence disinfection of articles of clothing, bedding, and merchandise, supposedly contaminated by contact with those sick of this disease, is unnecessary.

9. A house may be said to be infected with yellow fever only, when there are present within its walls contaminated mosquitoes capable of conveying the disease.

10. The spread of yellow fever can be most effectively controlled by measures directed to the destruction of mosquitoes and the protection of the sick against the bites of these insects.

11. While the mode of propagation of yellow fever has now been definitely determined, the specific cause of this disease remains to be discovered."

Subsequently the U. S. Army Board submitted a report, *"Experimental yellow fever,"* by Reed, Carroll and Agramonte, American Medicine, July 6, 1901, in which are described the clinical features of the experimentally produced disease, and in *"the prevention of yellow fever,"* by Reed and Carroll, N. Y. Medical Record, October 26, 1901, they describe the *Stegomyia fasciata*, mode of life, habitat, breeding places, together with methods for its suppression.

In 1901, during the summer, Dr. Carroll was again detailed to go to Cuba for the purpose of continuing the investigation into the causation and prevention of yellow fever. During this summer Dr. Carroll demonstrated by experiments, that the etiologic agent for yellow fever would pass through a Berkefield Filter, which would prevent the passage of the "Micrococcus aureus." He demonstrated its thermal death point at 55 C., when left at this temperature for ten minutes, although he could find nothing to which to ascribe the cause of the disease itself. He autopsied a fatal case of experimental yellow fever (a case of Dr. John Guiteras), the first *fatal* case experimentally produced, and found identical lesions with non-experimental yellow fever.

All of Dr. Carroll's work has been renewed and confirmed by U. S. Marine Hospital Service at Vera Cruz, and by a Commission from the Pasteur Institute working in Brazil ("La Fièvre Jaune" by M. M. Marchoux, etc., etc., Annales de l'Institute Pasteur, Nov., 1903) and today stands practically as he reported—without additions and without correction.

Under date of October 26, 1901, Major Reed, the head of the Yellow Fever Commission, writes Dr. Carroll concerning that summer's work saying:

"My dear Doctor:

"I have just received your letter of the 22nd and hasten to congratulate you on the thorough manner, in which you have accomplished the task assigned you. The results could not have been better, and throw a flood of light on the etiology of yellow fever. We can now go ahead and submit a contribution on the etiology of yellow fever. This we must do promptly after we have discussed all the later results.

"Again congratulations. Hoping to see you back soon.

"Sincerely yours, (W. Reed)."

This they did in an article, "The etiology of yellow fever, a supplemental note" (*American Medicine,* February 22, 1902).

This later piece of work, just like the first most important experiments, all fell upon the shoulders of Carroll. That he well bore that responsibility is best attested by the letter of Reed just cited, which at the same time shows upon whom the real responsibility rested, and gave him credit for the work done. In reporting this work as the first, Major Reed, of course, by reason of his superior military rank as Army Surgeon, appears in the Medical literature and Army medical reports, as being the head of the commission, which technically he was. At the Carroll Memorial Meeting of the Johns Hopkins Assoc. for Med. history Professor Wm. H. Welch repeatedly said to me personally, *"Carroll was the real hero of this memorable commission."*

After the lamented death of the highly gifted Reed, November, 1902, Dr. Carroll still continued to take an active part in the study of yellow fever, and in November, 1903, he published an article setting forth in detail his work investigating the "Mixococcidium Stegomyiæ," alleged by Working Party No. 1, Yellow Fever Institute. U. S. Public Health and Marine Hospital Service to be the specific cause of Yellow Fever. This organism Dr. Carroll demonstrated to be a yeast fungus, and not at all related to the causation of yellow fever. He found it present in mosquitoes fed on over-ripe bananas purposely besmeared with yeast culture, but always absent, when mosquitoes were fed on laboratory media as above described. See Journal Amer. Med. Assoc., Nov. 28, 1903, entitled "The Etiology of Yellow Fever. An Addendum."

During March, 1903, in reply to Dr. Finlay's assertions, that the U. S. A. Commission had not given him proper credit for the mosquito theory in explaining the etiology of yellow fever, Dr. Carroll published an article "The transmission of yellow fever" (*Journal American Medical Association, May* 23, 1903), in which a very accurate account of the work done by

Dr. Finlay and by the Commission is given, and in it Dr. Finlay is given full credit for *advancing the theory of the mosquito as probable causative factor,* but shows the real work and proof to have been done by the U. S. Army Commission by more precise and scientific methods.

In commenting upon the work of the U. S. Army Commission, Dr. H. H. Donnelly, George Washington University Publ. November, 1906, makes the comment: "The value of the work of Major Reed, the ranking medical officer of the Commission, has been made so prominent since his death as to appear to eclipse the essential work of his colleagues, Lazear and Carroll, by whom was first actually demonstrated that the 'Stegomyia Fasciata' is the source of yellow fever infection."

Carroll and myself were close personal friends, our friendship dating from the time, when Carroll was at the University of Maryland an undergraduate, and later when he was affectionately known as "Jim" Carroll. I had many personal interviews with him and conversations concerning his experiences in Cuba, and, in March, 1907, I wrote to my friend informing him, that the degree of Doctor of Laws, Honoris Causa, would be conferred upon him in May, 1907, at the One-Hundredth Anniversary of the University of Maryland, in recognition of his valuable contributions to science. The following is, with one omission, a copy of his last letter to me, and I am told the last he ever wrote. The remark at the end of this letter, *Great is the Truth, and it will prevail,* had reference to the fact, that Carroll knew, that I was in full possession of all data of his life work. It was but a natural expression of gladness, that his fellow-men would realize later on the real worth of his great sacrifice, for he himself had never laid claim to recognition. What had come to him, came by sheer force, and over the barrier of subordinate army rank. His last letter to me was written partly in recumbent position in bed. I had no knowledge of the gravity of his illness, otherwise I would have visited him in Washington and saved him the labor of letter writing.

This is a copy of his last letter:

Washington, D. C., 1433 Clifton St., N. W., May 27, 1907.

Dear friend Hemmeter:

Of course you will have deemed me ungrateful and unappreciative of the great honor to be conferred upon me by the Old University, my honored *Alma Mater,* but it is not so. I have been sick in bed since February 17th, running a temperature every day, except for three days, when it was temporarily controlled by aspirin. As I write now my temperature is above 100 (2 P. M.) and rising, and I am sitting up contrary to the advice of my physician friends. Nevertheless I have felt all along, that I would send nothing but an autograph reply, and this I have delayed day after day in the hope, that I would soon gain sufficient strength to write a long and satisfactory letter. The visit of my dear friend, Dr. I. S. Stone, enabled me to rest more comfortably afterward, because I felt, that he would explain the circumstances to you. I am sorry to say that during the past four or five days my heart has needed a little coaxing, for all attempts to sit up proved too much, though I felt very well while lying down.

With this explanation, I trust you will pardon my apparent rudeness in thus delaying my response to your communication, which made me very happy indeed. I assure you most sincerely that, apart from personal affliction, it gives me the deepest sorrow to feel, that I am forced to be absent from such a happy and inspiring reunion. As it is, I feel that if I am up and about with a serviceable though damaged heart within 30 days, I shall have cause for further congratulations.

Permit me to express to you, as best I can, my humble appreciation of the high honor to be bestowed upon me, and to hope that at some future date I may be able to express my gratitude in person.

In one of your letters, you ask for a list of publications, which I enclose. It will give me great pleasure to send you a set of such of the reprints as I have.

You ask me, whether I was infected as the result of our work. Certainly I was. *I was the first to propose, that we submit and the first to be infected,* though not the first to be bitten.

I am sending by this mail a paper by my friend, Dr. H. H. Donnelly, and this may be relied upon as accurate in every particular.

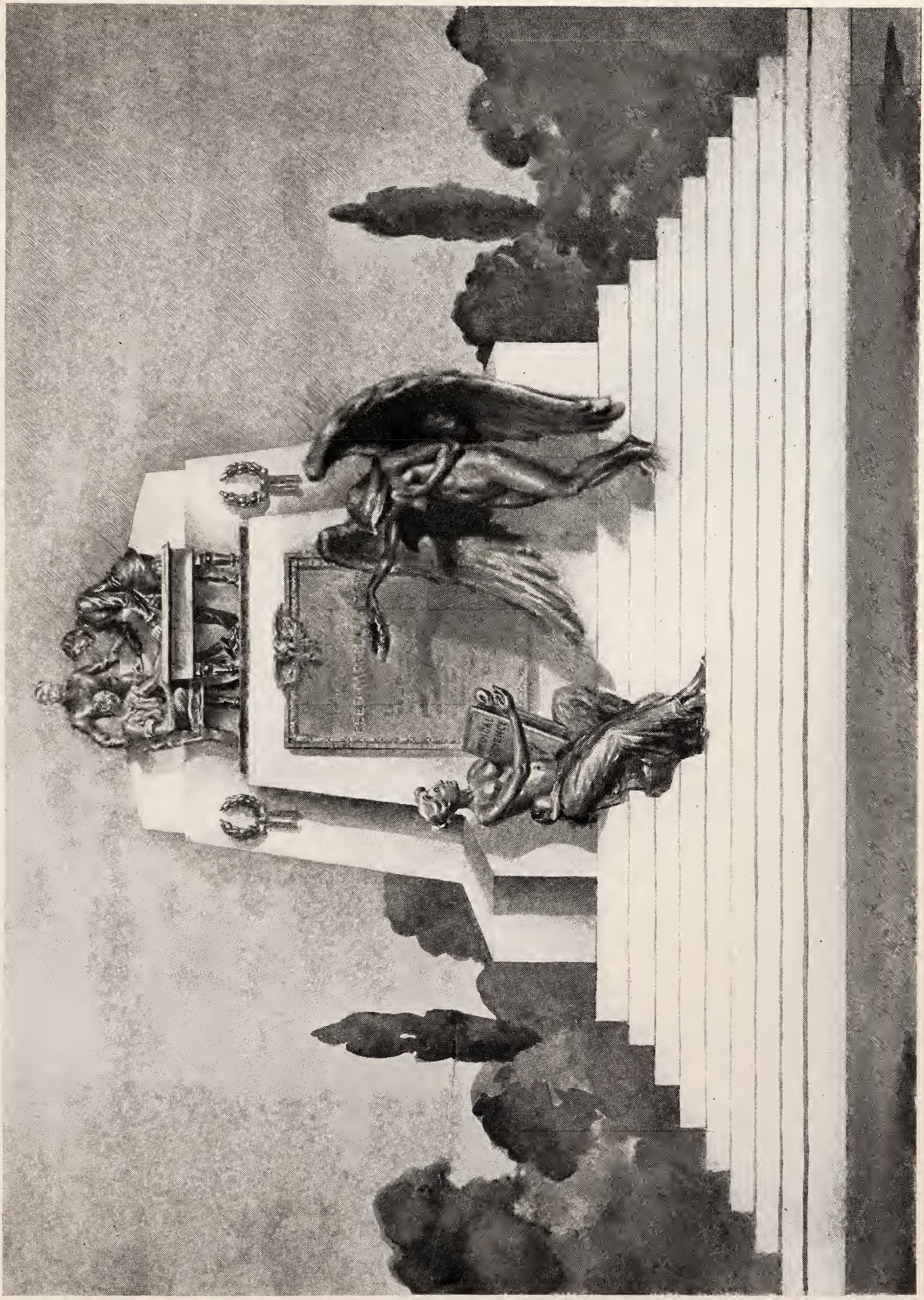
There is no foundation for the statement, that Dr. Reed aided me materially in securing my medical education. As a matter of fact, he had nothing whatever to do with it, and I was never associated with Dr. Reed in any way until I met him at the Army Medical School in Washington late in September, 1893. The order for me to proceed there was issued upon recommendation of Surgeon-General Sternberg. At this time I had obtained my medical degree, at the University of Maryland, had taken my post-graduate courses in bacteriology and pathology at the Hopkins (see Register 1891–1892 and 1892–1893) and had put in one summer working independently in the Army Laboratory at the World's Fair in 1893, for which I hold a certificate.

Great is the Truth and it will prevail!

Believe me most gratefully,

Fraternally and sincerely yours,

(Signed) James Carroll.



Proposed Monument to Yellow Fever Commission

Drawing by Dr. John C. Hemmeter, of his conception for a monument of the Genius of History presenting to American Medical Science the laurels of immortality, commemorating the discoveries and martyrdom of the United States Yellow Fever Commission.

The Degree of Doctor of Laws—"Honoris Causa" was conferred on Dr. James Carroll by his Alma Mater during the Centennial Celebration of the University of Maryland May 30th to June 2nd, 1907. As he was very sick at the time, it was the only degree conferred on an American "in absentia."

The University of Iowa also conferred the same honarary degree upon him.

In March, 1907, Carroll's military rank was raised from Lieutenant Surgeon to Major in the United States Army, by special Act of Congress. This Act of Congress was undoubtedly prompted by the following recommendation of the National Legislative Council of the American Medical Association, published in the Journal of the American Medical Association, January 20th, 1906:

REPORT ON GOVERNMENT RECOGNITION OF THE SERVICES OF DR. JAMES CARROLL

"Dr. John S. Fulton, of Maryland, introduced the following:

"Whereas, In the Year of our Lord Nineteen Hundred, a Yellow Fever Commission was appointed by the Army of the United States to investigate the causes of yellow fever and to devise means for its eradication, the said Yellow Fever Commission consisting of Dr. Walter Reed, surgeon in the Army of the United States, Dr. James Carroll, Dr. Jesse Lazear, and Dr. Aristides Agramonte, acting assistant surgeon in the Army of the United States; and

"Whereas, The said Yellow Fever Commission, consisting of Dr. Walter Reed, Dr. James Carroll and Dr. Aristides Agramonte (Dr. Jesse Lazear, deceased), did then and there determine the cause of yellow fever, and devise means for its prevention, by which means yellow fever was eradicated from Havana and Cuba, and thousands of lives have been saved in the United States and other parts of the Western Hemisphere; and

"Whereas, Dr. Jesse Lazear, an acting assistant surgeon in the Army of the United States, did subject himself to the bite of an infected mosquito, from which bite Dr. Jesse Lazear suffered death; and

"Whereas, Dr. James Carroll, an assistant surgeon in the Army of the United States, did subject himself to the bite of a mosquito infected with yellow fever, being the first attack ever experimentally produced; be it

"Resolved, That the National Legislative Council of the American Medical Association expresses its appreciation of the valuable work accomplished by the Yellow Fever Commission in the interest of humanity, the material and bodily welfare of the people and of the Army of the United

States, and of the heroism and devotion of the aforesaid Major Walter Reed (deceased), Dr. James Carroll, Dr. Aristides Agramonte and Dr. Jesse Lazear (deceased); and be it further

"*Resolved,* That this Council commend to the Government of the United States adequate recognition of the gallant and meritorious services of the said Dr. James Carroll, the only surviving member in the Army of the United States of the said Yellow Fever Commission."

This report was unanimously adopted by a standing vote.

The following quotation from the British Medical Journal, September 8th, 1906, will serve as an indication of the esteem in which the achievements of the American Yellow Fever Commission are held in England:

"Major Ronald Ross's discovery, that malaria is conveyed by mosquitoes, which act as an intermediate host, has not only led to successful measures to practically eradicate malaria with its attendant evils, but has given the clue to the cause of yellow fever and its treatment, etc. The first positive proof, that the Stegomyia was the carrier of the infecting agent of yellow fever, was given, when Carroll, in July, 1900, offered himself for a test experiment with a self-sacrifice worthy of all praise. He had a very narrow escape, but Lazear, of the American Commission, and Myers, of Liverpool, lost their lives. That the labors and said deaths of these heroic men were not in vain is amply attested by the remarkable vigour and success, with which the recent plague was stamped out, and the exemption secured by Havana and other pest centers."

In an editorial, the *British Medical Journal,* Sept. 8, 1906, proposes that the Nobel prize be divided between Dr. James Carroll and Dr. Aristides Agramonte.* This, at a date, when Carroll was still living:

"In regard to yellow fever, Panama affords as striking an object lesson as Havana of the incalculable benefit to mankind, that had followed the discovery of the cause of the disease and the manner of its transmission. The glory of the work, which has had this striking consummation is shared by several men. The credit of the conception belongs to Dr. Carlos Finlay, who propounded the idea many years ago without attracting from the profession any attention but an occasional contemptuous notice. More fortunate than many true begetters of new truths, Dr. Finlay, at the meeting of the Pan-American Medical Congress, held at Havana, in 1901, was acclaimed by the assembly as the author of the discovery, which has already been so fruitful of good effects. Dr. Henry R. Carter was another pioneer in the work, which was brought to completion by the American Commission, see Chapter XVI. Ultimately death snatched the reward from the hands of Walter Reed and Lazear, but Drs. Carroll and Agramonte still survive. It would, we think be a fitting acknowledgment of the work of these four men, if the Nobel prize were divided among them. It will scarcely be denied by any one conversant with the facts, that their work is of far greater importance than that of several, to whom the prize has been awarded in the past few years. The only original research work, whose practical results can be held to compare with it, is that, which had brought malaria, that monster, which till lately claimed so vast a tribute of human lives, within the control of man."

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* The failure of the scientists of the world to vote the Nobel prize of 1906 to James Carroll and Aristides Agramonte constitutes an outstanding injustice of history.

The following is quoted from the message of President McKinley to the Senate (59th Congress, Second Session Document No. 10):

"On August 2, 1900, before the mosquitoes were ready for experiment, Dr. Reed was called back to Washington to prepare for publication the abstract of the report of the board, appointed in 1898 to investigate the spread of typhoid fever in the volunteer camps in the United States, of which board he was president.

"During Dr. Reed's absence the inoculations by means of the mosquito were begun. On August 11th, Dr. Lazear made the first experiment, but nine distinct inoculations on persons including himself and acting Assistant Surgeon A. S. Pinto were unsuccessful. We know now, that these failures were due to two facts—first, that patients after the third day of the disease cannot convey the infection to the mosquito, and second, that after having bitten a yellow fever case, the mosquito cannot transmit the disease until after an interval of at least twelve days. On August 27th one mosquito was applied to Dr. Carroll, one which happened to fulfil both of these conditions. The result was a very severe attack of yellow fever, in which for a time his life hung in a balance. This was thus the first experimental case. The fever developed on the 31st of August, on which day Dr. Lazear applied the same mosquito, which bit Dr. Carroll with three others to another person. This man came down with a mild but well-marked case.

"The second member of the commission was Dr. James Carroll, at that time acting assistant surgeon, United States Army.

"Dr. Carroll is now 52 years old. He entered the military service June 9, 1874, and served as private, corporal, sergeant, and hospital steward from that date to May 21, 1898, when he was appointed acting assistant surgeon. He was appointed first lieutenant and assistant surgeon in the Medical Corps, October 27, 1902, which rank he still holds.

"Dr. Carroll was Dr. Reed's truest assistant and coadjutor from the

inception of the work, which resulted *in the discovery of the method of propagation of yellow fever.* As stated above, the third series of experiments were performed by Dr. Carroll alone, Dr. Reed having been refused permission to return to Cuba to complete his work.

"Dr. Carroll was the first experimental case of yellow fever, and he suffered a very severe attack, to which he attributes a heart trouble, from which he now suffers. At the time of undergoing this experiment he was 46 years old, an age from which the risk from this disease is very great, as its mortality rapidly increases with age of patient. He had at that time a wife and five children, who had no other means of support except his pay as an acting assistant surgeon.

"It is recommended, that Congress be asked to pass a special act promoting Dr. Carroll, on account of his services in connection with this discovery and the courage shown by him in subjecting himself to experiment, to the rank of lieutenant colonel, the number of medical officers in that grade being increased by one for that purpose; also his name and effigy should appear on the monument to Walter Reed.

"Dr. Jesse W. Lazear was the third member of the commission. Dr. Lazear was a native of Baltimore and a graduate of Johns Hopkins University, afterwards getting his professional degree at Columbia University and Bellevue. At the time he incurred his death in the course of these experiments, as above mentioned, he was 34 years old. He left a wife and two young children, the younger a little son born a few months before his death, whom he never saw. Mrs. Lazear received from Congress a pension of $17 a month with $2 additional for each of two minor children, until they reach the age of 16. Also a battery in Baltimore harbor was, by direction of the Secretary of War, named in his honor. It is believed that this recognition on the part of the nation for his services is utterly inadequate. His widow's pension should be increased to $100 per month, and steps should be taken to perpetuate his name in connection with the Walter Reed monument above suggested.

"Dr. A. Agramonte was the fourth member of the yellow fever commission. He was a Cuban by birth, and immune to yellow fever.*

"Twenty-three of the men, who submitted themselves for experiment by the board, contracted yellow fever, beginning with Dr. James Carroll, who was taken sick August 31st, 1900, and ending with John R. Bullard, who was taken sick October 23, 1901.

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* To the present day Dr. Agramonte has not received the reward from the United States Government which his great services on the U. S. A. Yellow Fever Commission of 1900 deserve. (November 30, 1926.)

"Conspicuous among them was John J. Moran, a civilian clerk, employed at the headquarters of General Fitzhugh Lee, at Quemados, who was one of the earlist volunteers for the second set of experiments, and whose action was dictated by the purest motives of altruism and self-devotion. Mr. Moran disclaimed, before submitting to the experiments, any desire for reward, and has never accepted any since, although he was offered the $200, which the liberality of the military governor enabled the commission to give to each experimental patient. Such was his modesty, that he has made no effort, so far as known to this office, to make known his connection with these experiments and reap the credit, which is so justly due him. Mr. Moran was a native of Ohio. His present address is not known to this office. The first inoculations in the case of Mr. Moran were for some reason unsuccessful, on November 26th and 29th. He did not suffer an attack until after the third inoculation on December 21st.

"The same remarks apply to the first experimental case of the second set, Private John R. Kissinger, Hospital Corps, who volunteered at the same time with Moran and equally disclaimed any desire for reward.

"Private Kissinger did not leave Cuba immediately after the experiments, as did Mr. Moran, and therefore the military authorities were able to reward him in some measure along with the other enlisted men, who volunteered for these experiments. He was promoted acting hospital steward, presented with a gold watch by the chief surgeon of the department in the presence of all the medical officers and hospital corps men on duty at the Columbia Barracks, and also received a present of $150 in cash. He took his discharge November 14, 1901, and has since (on December 19, 1903) made application for pension. This was refused for lack of evidence, that his ill-health was incident to the service.

"Of the other experimental cases, seven were Spanish immigrants, who submitted to experiments purely for the money, which they were promised. With regard to those, who were American soldiers, however, ten in number, in addition to those already mentioned, it cannot be doubted that, although they received pecuniary rewards, a desire to assist in what they appreciated was a great and glorious work, together with a spirit of adventure, was the most powerful motive. The same is true of the last experimental case, Mr. John R. Bullard, a graduate of Harvard, where he was a distinguished athlete and captain of the University crew. The names of these men, with the dates of their attack, is appended with this report."

“Names of persons, who submitted to experimental inoculation of yellow fever, Cuba, 1900–1901.

Infected by mosquitoes.	*Taken sick.*
1. James Carroll	Aug. 31, 1900
2. X. Y. (American soldier)	Sept. 6, 1900
3. John R. Kissinger	Dec. 8, 1900
4. Nicanor Fernandez	Dec. 13, 1900
5. Antonio Benigno	Dec. 13, 1900
6. Becente Presedo	Dec. 15, 1900
7. John J. Moran	Dec. 25, 1900
8. Jose Martinez	Jan. 3, 1901
9. Levi E. Folk	Jan. 23, 1901
10. Clyde L. West	Feb. 3, 1901
11. James L. Hanberry	Feb. 9, 1901
12. Charles G. Sonntag	Feb. 10, 1901
13. Pablo Ruiz Castillo	Sept. 19, 1901
14. Jacinto Mendez Alvarez	Oct. 13, 1901

Infected by injection of blood.	
1. Warren G. Jernegen	Jan. 8, 1901
2. William Olson	Jan. 11, 1901
3. Wallace Forbes	Jan. 24, 1901
4. John H. Andrus	Jan. 28, 1901
5. Manuel Gutierrez Moran	Oct. 20, 1901
6. John R. Bullard	Oct. 23, 1901

Infected by injections of filtered blood serum.	
1. P. Hamann, Twenty-third Battery Coast Artillery	Oct. 19, 1901
2. A. W. Covington, Twenty-third Battery Coast Artillery	Oct. 19, 1901

Exposed to Fomites.

1. Dr. R. P. Cooke, acting assistant surgeon.
2. Levi E. Folk.
3. Warren G. Jernegan.
4. James L. Hanberry.
5. Edward Wealherwalks (bitten once, negative; refused after Hanberry came down).
6. James Hildebrand (offered himself, but was declined on account of age).
7. Thomas M. England.”

Major James Carroll[2] died on the 16th of September 1907, in Washington. He had informed me on several occasions, that he had not been a sound man since that memorable experimental infection in August, 1900, which should constitute one of the most important discoveries in the history of medicine. But his interest in the subject of yellow fever did not cease with the discovery of the method of its transmission, for he continued to make many later contributions to this important subject, as the appended list of his publications will indicate. He was buried with military honors usually accorded to a major of the United States Army. Scientific and military men of the highest rank considered it an honor to act as pall bearers and escorts at this funeral.

The U. S. A. Yellow Fever Commission has through the talents of Reed, Carroll, and Lazear, contributed to humanity and to their country a service, the value of which cannot be overestimated. This discovery was made possible through the heroism of James Carroll. The United States had been invaded by yellow fever 90 times, carrying death and destruction into New Orleans, New York, Philadelphia, Memphis, Baltimore, Charleston, Galveston, Portsmouth, and many smaller cities. In 1793 it wiped out 10% of Philadelphia's population, and the epidemic of 1853 cost New Orleans 8000 lives. In the message from President McKinley to the Senate (59th Congress, Second Session, Document No. 10) dated Dec. 5th, 1906, it is stated, page 7, by Surgeon General U. S. A., R. M. O'Reilly, that the total disbursements of this great nation in the way of rewards for those who made possible this brilliant discovery, amount to $146 a month. Since Carroll's death this has been increased by $25 a month, the pension of a widow of a major U. S. A. Prof. William H. Welch, of the Johns Hopkins University, who was the distinguished teacher of Major Carroll, in the same message, calls attention in a letter to the Secretary of War to the saving of thousands of lives through this discovery, which would have been impossible without Carroll's great sacrifice.

The English Government voted Sir William Jenner, the discoverer of vaccination, various grants amounting to 30,000 pounds sterling. India presented him with a subscription of 7323 pounds. It is important to call the attention of the committee on pensions in the Senate and Congress to these liberal grants, for we believe, that it is the wish of the American people to emulate them, lest the old accusation about the ingratitude of republics shall again come true. The widow of Dr. Jesse Lazear, the third member of the Yellow Fever Commission, receives a pension of $17 a month, with an additional $2 for each of the two minor children until they reach the age of 16. The pension of the widow of Dr. Lazear, as well as of the widow of Major James Carroll, should be increased to $125 a month.

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[2] See address by H. A. Kelly, W. H. Welch, W. S. Thayer, G. M. Sternberg, H. H. Donnelly, A. F. A. King, S. Ruffin, C. E. Monroe, and J. O. Skinner, in *Johns Hopkins Hospital Bulletin,* January, 1908.

There is, however, one very important and essential desideratum, which I consider it my duty to emphasize in this connection, and that is, that the life and work of Major James Carroll, as well as Dr. Jesse Lazear, should be represented by a monument with a prominence equal to that given to Major Walter Reed; for there can be no just distinctions made between the merits of these three benefactors, and any comparisons aiming to bring out personal preferences would be decidedly invidious. Applicable to the life and work of all three is the beautiful sentiment of Horace,

Integer vitae scelerisque purus

and there rarely has been an incident in the history of medicine, in which the lives of three distinguished men were directly or indirectly sacrificed for one great purpose—thereby securing to each one of them a place in the *Walhalla* of American History.

HORACE'S MONUMENT

I've reared a monument—my own—
  More durable than brass;
Yea, kingly pyramids of stone
  In height it doth surpass.

Rain shall not sap, nor driving blast
  Disturb its settled base,
Nor countless ages rolling past
  Its symmetry deface.

I shall not wholly die. Some part,
  Nor that a little, shall
Escape the dark Destroyer's dart,
  And his grim festival.

As long as, with his Vestals mute,
  Rome's Pontifex shall climb
The Capitol, my fame shall shoot
  Fresh buds through future time.

Where brawls loud Aufidus and came
  Parched Daunus erst, a horde
Of mystic boors to sway, my name
  Shall be a household word.

As one, who rose from mean estate,
  And first, with poet's fire,
Æolic song to modulate
  To the Italian lyre.

Then grant, Melpomene, thy son
  Thy guerdon proud to wear,
And Delphic laurels, duly won,
  Bind thou upon my hair.

Inscription on Bronze Memorial Tablet erected in the Medical Building of the University of Maryland in honor of *James Carroll,* Major and Surgeon U. S. Army, M.D. (1891) and LL.D. (1907) University of Maryland, University of Nebraska, LL.D. (1907) Professor of pathology and bacteriology, Columbian Univ. Wash. D. C. Born in Woolwich, England June 5th 1856. Died in Washington D. C. September 16th 1907.

> As a member of the army commission, which succeeded in demonstrating the mode of conveyance of yellow fever, he became an eminent contributor to science by his investigations and a heroic benefactor of his country and of mankind by voluntary submission to the bite of an infected mosquito, whereby he suffered from a severe attack of yellow fever, produced for the first time by experiment.

"Greater love hath no man shown than this, that a man lay down his life for his friends."

LIST OF DR. CARROLL'S INDEPENDENT CONTRIBUTIONS TO THE LITERATURE OF YELLOW FEVER

"The Treatment of Yellow Fever," Jour. Amer. Med. Assoc. July 19, 1902.

"The Transmission of Yellow Fever," Jour. Amer. Med. Assoc. May 23, 1903.

"The Etiology of Yellow Fever, an Addendum," Jour. Amer. Med. Assoc. Nov. 28, 1902.

"Remarks on the History, Cause, and Mode of Transmission of Yellow Fever, etc." Jour. Assoc. Military Surgeons, 1903.

"A Brief Review of the Etiology of Yellow Fever," N. Y. Med. Journ. and Phil. Med. Jour. (consol) Feb. 6 and 13, 1904.

"Without Mosquitoes There Can Be No Yellow Fever," Amer. Medicine, March 17, 1906.

CONTRIBUTIONS OF DR. JAMES CARROLL TOGETHER WITH DR. WALTER REED

1899 a. Bacillus icteroides and Bacillus cholerae suis. A preliminary note. Med. News, N. Y., v. 74 (17) Apr. 29, pp. 513–514.

1899 b. The specific cause of yellow fever. A reply to Dr. G. Sanarelli. Med. News, N. Y., v. 75 (11), Sept. 9, pp. 321–329.

1900. A comparative study of the biological characters and pathogenesis of Bacillus X (Sternberg), Bacillus Icteroides (Sanarelli), and the hog-cholera bacillus (Salmon and Smith). (Received for publication Feb. 25.) J. Exper. M. (Balto). v. 5 (3), Dec. 15, pp. 215–270, figs. a–o, pl. 19, figs. 1–3.

1901. The prevention of yellow fever. (Read at 29th Ann. Meeting, Am. Pub. Health Assoc., Buffalo, Sept. 16–21.) Med. Rec., N. Y. (1616), v. 60 (17), Oct. 26, pp. 621–649, figs. 1–10.

1902. The etiology of yellow fever. A supplemental note. (Read at 3d Ann. Meeting, Soc. Am. Bacteriologists, Chicago, Dec. 31, 1901 and Jan. 1, 1902.) Am. Med. Phila., v. 3 (8), Feb. 22, pp. 301–305, charts 1–6

PUBLICATIONS OF DR. JAMES CARROLL TOGETHER WITH DR. WALTER REED AND DR. ARISTIDES AGRAMONTE

1901 a. The etiology of yellow fever. An additional note. (Read at Pan.-Amer. Med. Cong., Havana, Cuba, Feb. 4–7.) J. A. Med. Assoc. Chicago, v. 36 (7), Feb. 16, pp. 431–440, charts 1–6.

1901 b. Experimental yellow fever. Am. Med. Phila., v. 2 (1), July 6, pp. 15–23, charts 1–8, tables 1–2.

PUBLICATIONS OF DR. JAMES CARROLL TOGETHER WITH DR. WALTER REED, DR. ARISTIDES AGRAMONTE AND DR. JESSE W. LAZEAR

1900. The etiology of yellow fever. A preliminary note. (Read at meeting of Am. Pub. Health Assoc., Indianapolis, Ind., Oct. 22–26.) Phila. M. J. (148) v. 5 (17), Oct. 27, pp. 790–796, tables 1–3, charts 1–2.

Headquarters of the Army,
Adjutant General's Office,
Washington, May 24, 1900.

Special Orders No. 22.

Extract.

33. By direction of the Secretary of War, Major Walter Reed, Surgeon U. S. Army, and Acting Assistant Surgeon James Carroll, U. S. Army, upon completion of the duty assigned them by the Commanding General, Department of the East, will proceed to Camp Columbia, Quemados, Cuba, on official business pertaining to the Medical Department, and report their arrival and instructions to the Commanding General, Division of

Cuba; the Commanding General Department of Havana and Pinar Del Rio and the Commanding Officer, Camp Columbia. The travel enjoined is necessary for the public service.

34. By direction of the Secretary of War a Board of Medical Officers is appointed to meet at Camp Columbia, Quemados, Cuba, for the purpose of pursuing scientific investigations with reference to the infectious diseases prevalent on the Island of Cuba.

Detail for the Board:

Major Walter Reed, Surgeon, U. S. Army.
Acting Assistant Surgeon, James Carroll, U. S. Army.
Acting Assistant Surgeon Jesse W. Lazear, U. S. Army.

The Board will act under general instructions to be communicated to Major Reed by the Surgeon-General of the Army.

By command of Major General Miles

H. C. Corbin,
Adjutant General.

A true copy.

Camp Columbia, Quemados, Cuba.
July 16, 1900.

My Dear Jamie:

Your letter of July 11, came with Mama's letter this morning. It was very nicely written and I was glad to receive it. I am glad if David has learned to ride so soon but I hardly think he could have done so in a few minutes. Can he mount from the pedal? Can he dismount without falling and can he turn a corner without running into somebody? I think Papa will have to see David ride for himself, because you try to fool your poor old Pop sometimes.

Now about your coming down here with David and Tracy. If you fellows come, you will probably have to stay until the new year and while you are down here, I will have to pay $1 a day for board for each of you and $2 or $3 a month for washing, which would amount to about $65 a month. Now if Mama will agree for me to pay that out of her $100 all right and then she can pay your fare to New York and hotel bill and carfare and transfer etc. And then there is yellow fever in this country; they call it "Yellow Jack" because people, who have it, turn yellow after they die. And when they are sick they give them nothing to eat sometimes for a week, and I think you would not like that.

And then what would Mama do, without you boys to look after her, while Papa is away? And you would have to miss your school and music lessons too. Papa wants you to learn all you can, while you are young, because if you do not, when you grow up, you will not be able to do great work or command large salaries. Everything you learn now, my boy, will make your life easier for you, when you become a man. Learn to spell above all things; then study grammar and learn how to speak and write, and study history and geography to know about people and countries. You ought to learn something now about China because the Chinks have murdered a lot of white people, men, women and children and there will be a big war over there soon.

Good night my dear boy, and with love and kisses from your loving

Papa.

Camp Columbia, Quemados, Cuba.
July 24, 1900.

My Dear Jennie:

Your letters written on the 18th and 19th telling me about the baby's illness came yesterday and I have felt very miserable since. If her teeth are the cause of the suffering, I am surprised, that you did not scratch her gums yourself with the point of a knife soaked in carbolic acid and then rinsed in boiled water. You know it sometimes gives relief and does no harm. A few drops of nitre in water for her fever and to sleep at night in the room downstairs with the doors and windows open and frequent sponging is about all, that one can do. If she is weak a few drops of good sherry in boiled water and sweetened ought to hold her up. It is the usual attack, that all babies must go through and with care I hope she will be all right. But I shall be dreadfully worried, until I hear from you, and indeed until I learn, that she is better as I earnestly hope she will be or is by this time.

Yesterday, as the mail came in, I was about to leave for the camp at Guanajay, 22 miles from here to determine the nature of some fevers prevailing among the soldiers there. I left after lunch in a carriage with 4 mules and arrived after a pleasant drive in the evening. Today I returned, leaving at 4 this afternoon, and wrote before going to bed as to get the letter off by tomorrow's mail. I have no heart to write much and only wish, that I could be with you to attend to our dear little baby.

I think, that our babies are stronger and better cared for than the average and ought to have a better chance of pulling through. With kisses and love for the babies all.

Your affectionate husband,
James.

Blue Ridge Summit, Pa.
Tuesday, August 27, 1900.

My Dear Carroll:

I have your letters of the 18th and 22nd both of which reached me by this morning's mail. It is needless to say, that both were of extreme interest. Certainly Guiteras should be satisfied with his proof of the mosquito theory—since out of seven cases with two fatal, is evidence, that should even convince Wasden.

I wrote you a few days ago advising against further experiments on human beings in view of these fatal cases. From the tenor of your last letter I see, however, that you have made all preparations to go ahead with the observations as determined by us in Washington and I hardly know, what to say—I will suggest, that inasmuch as the injection of blood as gives four positive results, that you limit your observations to the injection of the filtered serum, without using controls injected with unfiltered blood. If the filtrate proves negative, say in 3 or 4 cases, that would decide the point conclusively. Of course since Guiteras denied you the chance of obtaining blood, I suppose, that you must obtain your first subject by means of the mosquito bite, unless there are cases in Havana. What could have been Guiteras objection to your applying mosquitos to his cases? I am so glad, that you made an autopsy in one of the fatal cases. I shall await your results with the greatest interest.

Sincerely yours,
Walter Reed.

Columbia Barracks, Quemados, Cuba.
Sept. 2, 1900.

Dear Doctor Reed:

I hasten to let you know about Carroll. Dr. Kean has just wired to the Surgeon General, who, of course will inform you at once. I did not cable to you, for I feared, that you might not be at the Museum, and that the news might get to Mrs. Carroll in an improper form.

He had a headache on Friday August 31, and in the evening I found his temperature to be 102. Next morning, however the temperature was 98.4 and we thought all was well. I could find no malarial parasites. Later in the day the temperature gradually mounted to 103.8, the pulse falling from 90 to 80. This morning temperature is still high 103.5 and there is a good trace of albumin in the urine. Headache and photophobia are marked. He took two good purges on the 31st and on Sept. 1st. He was taken to the isolation camp this morning. He seems much worried, but is not restless at present. It was hard to keep him in bed at first. I shall keep you informed. Dr. Gorgas was out this morning and I shall ask him to come frequently.

Most sincerely,
Jesse W. Lazear.

Camp Columbia, Quemados, Cuba.
Sept. 3, 1900.

My Dear Mother:

I have a very light case. No cause for alarm. A change this evening for the better. Will keep you posted by every mail.

Yours,
James.

(This was written by his nurse Mrs. Lena Wamer).

September 7, 1900.
1 : 15 p. m.

My Dear Carroll:

Hip Hip Hurrah; God be praised for the news from Cuba today—"Carroll much improved—Prognosis very good." . . . Really I can never recall such a sense of relief in all my life as the news of your recovery gives me.

Reed.

War Department,
Surgeon General's Office,
Army Medical Museum and Laboratory,
Washington, Sept. 7, 1900.

My Dear Doctor Carroll:

I cannot begin to tell you, how delighted I was to hear, through cable from Dr. Kean, that you are better. Thank Heaven for that much. Do pray keep up your courage and pull through. It is not necessary for me to tell you, that I have been utterly wretched and unhappy over the news of your sickness. Your many friends in Washington are so very anxious about you. When I told Hagner this morning, that you were better, he clapped his hands through the telephone and said "Oh Doctor Reed I never was so glad to hear anything in my life." Mrs. Carroll though shocked bears the news like the brave woman, that she is. Your children are all well, but know nothing of your sickness. Again, thank God for your improvement. Keep up the good work. Love to Doctors Ames and Dear Lazear, who have so nobly cared for you.

Affectionately,
Reed.

Camp Columbia, Quemados, Cuba.
Thursday afternoon, Sept. 13, 1900.

My Dear Jennie:

It seems impossible, that I have been sick in bed two weeks, and that I have been so nearly passing in my checks. This is my first attempt at writing and I am still very weak indeed, so weak that I cannot attempt stand or change my position without assistance. But I have been disinfected and have been taken back into my old quarters again, I look like a scarecrow, I have lost much in weight, my beard is long and I am thoroughly worn out. I shall pull up rapidly again. A steady life has saved me . . . feel too tired to write much more today.

Your affectionate husband,
James.

Camp Columbia, Quemados, Cuba.
Tuesday, Sept. 18, 1900.

My Dear Jennie:

I write to let you know, that I am picking up as rapidly as a person could, gaining every day. I heard today, that the Surgeon General leaves Washington for about two weeks, and that Doctor Reed will probably start in a day or two for here. It would do you good to see me eat at every meal and I am growing stronger and stronger every day. I weighed this morning and tipped the scales at 152½ pounds, which is only seven pounds lighter than before I was taken sick. . . .

Another case has come down here, since I have been reported convalescent. He is Major Cartwright, the chief quartermaster of the Department. He is 38 years of age and has a temperature running about the same as mine. He is probably not so strong as I was and at this stage no one can tell, what the result will be. One thing is certain Jennie, I am now entirely free from danger and incapable of again contracting the disease.

Do not worry about me now Jennie. I am perfectly safe and it is only a matter of a short time, when I will be back with you.

Your affectionate husband,
James.

Blue Ridge Summit, Pa.
Sept. 19, 1900.

My Dear Carroll:

A letter from Dr. Lazear dated Sept. 13 said, you were to return to quarters on that day and that you were rapidly picking up strength. I am so delighted to know, that you are once more yourself. Have you decided whether you will return to the United States on leave of absence or not? In conversation with Mrs. Carroll prior to your attack of yellow fever she said, that if you have to return to Cuba, she would rather that you stay until Xmas and then come home for good. In other words she did not like the idea of your returning after you once got home. I shall come down about the first of October and perhaps may still find you at Columbia Barracks. I hope to see you whether there or in Washington, before I sail. I think that we can all get back home by December 20th. That is my present expectation. How does it feel to be an immune?

Sincerely yours,
Walter Reed.

Camp Columbia, Quemados, Cuba.
Sunday, Sept. 23, 1900.

My Dear Jennie:

I write this to let you know, that I am still alive and gaining strength. I look forward to tomorrow's mail for some news from Doctor Reed as I have not heard a word from him and do not know, what to expect. Agramonte left for New York yesterday with his wife and Doctor Lazear is still very ill with about even chances for his recovery. Major Cartwright, who was the next to take yellow fever after me, died this morning. I asked for an autopsy and it was refused. Doctor John Guiteras, formerly Professor of Pathology at the University of Pa. called to see me this afternoon as also did Dr. Gorgas and Dr. Finlay. Everybody is surprised at the manner in which I am picking up flesh and strength and all these yellow fever experts remarked how strong I was upon my legs. Yellow fever has no terrors for me now, because I am an immune and cannot contract it again, which gives me a great feeling of comfort.

Your affectionate husband,
James.

Camp Columbia, Quemados, Cuba.
September 28, 1900.

My Dear Jennie:

I received no letter from you by the last mail, so I have none to answer.

I inclose one, that I received from Dr. Reed this morning so that you can see the suggestion for me to remain down here until the end of the year; I have written to him just as I feel, that I have put on flesh rapidly, but I am very weak; yet I think that the sea trip and short visit to the United States will build me up greatly. I am sick of this Cuban diet and long for some American oysters and fruits. If it is only for 10 days I can come back stronger and better able to work.

Dr. Lazear died tonight after a week's illness. He was taken sick just a week ago this evening 8:00 o'clock and died tonight at about the same time, so I suppose, that I may regard myself as a lucky individual.

As you will see from Dr. Reed's letter he expects to start October 1, and will reach here about the 5th. I shall try and get back as soon as possible after he reaches here.

Your affectionate husband,
James.

(Carroll performs an autopsy 2 weeks after his recovery)

Camp Columbia, Quemados, Cuba.
Thursday, Sept. 28, 1900.

My Dear Jennie:

There is not anything of special interest to mention today. I have spent about half the day sitting on the porch and the other half looking over Lazear's papers for notes pertaining to our work. There was quite a notice in this morning's papers about Dr. Lazear, and your husband is mentioned too. I mail you the paper. . . . By the way that paper says, that I still have yellow fever. That is not so; I am entirely well and nearly as strong as ever. I can go, where I please and I attended dear Lazear's funeral yesterday. *Last Sunday I performed an autopsy, though I was really too weak to undertake it.* But the case was really an interesting one and if I had not done it, we would have lost a great deal.

Your affectionate husband,
James.

Camp Columbia, Quemados, Cuba.
Saturday, Sept. 29, 1900.

My Dear Jennie:

Doctor Reed is coming down again. I received three letters from him today and he tells me, he expects to sail from New York on the 28th and to arrive at Cuba on Wednesday next. . . .

Kean asked me to have everything in the laboratory disinfected, so it would be safe for Dr. Reed to come and work in.

A mosquito nearly killed me and another killed poor Lazear; the same one, that bit me, was allowed to bite another man and he had a mild attack of yellow fever. This is the most important part of our work and Dr. Reed tells me, that we must publish a small paper about it. Do not mention this to any one for anything just yet, because it is to be kept a profound secret. It is one of the discoveries of the century.

Your affectionate husband,
James.

Post Hospital, Cuba.
November 20, 1900.

My Dear Jennie:

I made my first trip to the City Hospital today, where I was greeted very nicely by some of the Spanish physicians, one of whom patted me approvingly on the back. The cases of yellow fever appear to be very mild, so much so that our work may be interfered with. I applied two mosquitoes with a view to having them bite volunteers for further experiments. I have gotten entirely over my attack of cold or grippe, whichever it may have been, and feel perfectly well again.

Your affectionate husband,
James.

Camp Columbia, Cuba.
Thursday, Nov. 22, 1900.

My Dear Jennie:

I received your letters of the 15th and 17th yesterday. . . .

We have just returned from Havana, where Drs. Reed, Agramonte, and myself have been to see the Spanish Consul and General Wood's chief officer. One of the Spanish papers has taken up the matter of our experiments and accuses us of horrible cruelty in enticing Spanish emigrants out to camp by hiring them as laborers, then locking them up in a room at night and turning in a lot of infected mosquitoes to give them yellow fever, without their knowing anything about it. As a matter of fact we do take Spaniards and give them $1.00 a day and board and we offer them a reward to be bitten, if they choose. No compulsion whatever is *to be used, and we have American volunteers* for the same work, who will be bitten and treated exactly as they are. The Consul says, that so long as the men are of age and consent fully, it is nobody's business. So we will go ahead. Do not speak of this to any one, because we do not want it spread any more than is necessary, until after we have finished.

Your affectionate husband,
James.

Camp Columbia, Cuba.
Sunday, November 25, 1900.

My Dear Jennie:

I received two letters from you yesterday, one dated the 18th and the other the 20th. . . .

I have just returned from paying a happy call with Doctor Reed. We called on Mrs. Dr. Kean, who gave the card party with Mrs. Stark. Captain Slocum, who was on General Lee's staff, was very nice, and he is a neighbor to General Kean's and I was out on the porch with them. He addressed me as "Carroll" and sat with him his arm resting on the back of the bench beside me. These people here are all intensely interested in the yellow fever and mosquito question and all are awaiting the result of our experiments.

A very bitter paper has recently been published against us, so that we must repeat our experiments, before we can go any further, because many are trying to raise a laugh at our expense.

Your affectionate husband,
James.

Cuba, Columbia Barracks, Nov. 25, 1900.

My Dear Jennie:

I send you the Havana Post for yesterday and today. One states that we are creating a sensation in Havana; the other that a party of Cleveland physicians are coming down to attend the meeting of the Medical Congress in Havana next month.

We drove out to visit Santa Clara Battery, where the captain of the Battery, to which Dr. Reed's son belongs, is stationed. He wanted to call on him and incidentally we wanted to look for mosquitoes. We found some breeding in the captain's kitchen, and spent a pleasant afternoon.

Your affectionate husband,
James.[3]

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[3] The description of a "serum boil" (abscess) in this following letter and of an attack of "Grippe" in a previous letter suggests that Carroll was in feeble health even at this date, Dec. 11, 1900, and several months after his so-called recovery from yellow fever.

Camp Columbia, Cuba.
Tuesday, December 11, 1900.

My Dear Jennie:

In a conversation with Dr. Reed he stated, that he thought we would probably return to Washington about the middle or the end of February. He is very much elated, because we have obtained a true case of yellow fever from the bite of an experimental mosquito and this confirms our former results. The idea is to read another paper before the Pan-American Congress, which meets in Havana this winter. The time of meeting has been postponed from December 26 to February 5, so it will be necessary to wait here until then. He acknowledges the great importance of our work and said, it should entitle us to membership in any of the European Medical Societies. In addition the U. S. Gov't should award a sum of money for the work, but that is all speculation. I had him lance a *serum boil for me tonight, and I think my neuralgia* will be better in the morning. We have several more men, whom we expect to come down from the bite of a mosquito and a great deal more of this work to do before the report can be prepared. This is the great chance of my life time—it is a perfectly sure thing and some benefit must certainly follow.

This should be our consolation for spending a lonely Xmas and a year hence I hope our position will be much better than now. With love and kisses to the boys, Mabel and yourself.

Your affectionate husband,
James.

Columbia Barracks, Cuba.
Saturday, December 15, 1900.

My Dear Jennie:

Your letters of the 8th and 11th of December came today. . . .

I do not think it wise for you to go to any one now, because if they had the disposition to have done something, it could have been done long before now. Now that I have risked my life and earned recognition and fame by my own efforts, I prefer to trust to the friend, who assured me, he would look after my interest and who has never failed me . . .[4]

Your affectionate husband,
James.

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[4] An expression of hope of military advancement and recognition by the U. S. Government.

James Carroll

Columbia Barracks, Cuba.
Monday, December 17, 1900.

My Dear Jennie:

Tracy was foolish not to have accepted Dr. Reed's offer of $200. We have had four men bitten since and all have recovered after mild attacks of yellow fever. Tracy did not see Dr. Lazear get the bite, that killed him, though he did see him take one bite. Lazear only permitted an insect, that had been applied to a very mild case to bite him. And nothing followed that bite. The insect, that he applied to me, however, had bitten four cases, all of them *very severe* cases. My attack was a severe one also. The bite, that killed Lazear, was from an insect flying about the hospital ward, one that he knew nothing about, and from which he expected nothing. The result you know.

Your affectionate husband,
James.

Columbia Barracks, Cuba.
Xmas Evening, 1900.

My Dear Jennie:

I received your letter of the 20th yesterday. . . . We have had another case of yellow fever come down today. The poor fellow is quite sick. He is a white man, who volunteered and is to receive $200. We simply turned some mosquitoes, that had bitten another yellow fever patient two weeks before into a house and then watched the mosquitoes bite him from another portion of the room, which was partitioned off with wire gauze. After 4 days he has come down and out of 5 persons, that we have bitten, every one has come down at last. This means complete success for us and confusion for Sanarelli. Our other paper has been published in the Journal of Experimental Medicine and we have 250 reprints in Washington.

Your affectionate husband,
James.

Columbia Barracks, Cuba.
Monday, December 30, 1900.

My Dear Jennie:

I went to the city yesterday and drew $950 from the bank to pay the men, upon whom we have experimented. Out of that I paid $129.00 for a watch chain and charm to be presented to one of these men for his courage in submitting to the inoculation.

Your affectionate husband,
James.

Columbia Barracks, Cuba.
January 23, 1901.

My Dear Jennie:

We had another case come down today from the bite of a mosquito. We are now able to produce cases of yellow fever to order.

Your affectionate husband,
James.

Columbia Barracks, Cuba.
Thursday, January 31, 1901.

My Dear Jennie:

A yellow fever editorial in the Post was very sarcastic and I had lots of fun teasing Dr. Reed about it. . . .

Our paper will probably be ready next Monday, and then watch the American papers. Tell the folks at home to do the same, especially the New York papers, for it will attract some attention. I will send you the Havana papers, so that you can read for yourself, what they say here. We have spent a busy afternoon. The delegates to the Pan American Congress have begun to arrive and this afternoon we entertained the delegates from Mexico, who were very anxious to know something of our work. They have a great deal of yellow fever at Vera Cruz and their Government is anxious to stamp it out. One of the gentlemen is physician to President Diaz; one is an eminent bacteriologist; one has seen more yellow fever cases perhaps than any other man in the world, and another is a military surgeon of high rank. The latter compliments me by saying I was a hero and all were delighted, with what they saw. I send you the Havana Post of today, which contains some allusions to our Commission.

Your affectionate husband,
James.

Columbia Barracks, Cuba.
February 5, 1901.

My Dear Jennie:

I received your first letter this evening upon my return to the city, where I have been all day listening to the discussion at the Congress.

Our paper appears to have been the event of the session and of course my friend received the greatest praise. One of the Cuban physicians raised the point, that it was wrong to experiment with human beings; to this Reed stated in reply, that the first person to come down was a member of the commission—myself. That brought down the house, such as it was.

After the morning session Dr. Agramonte took Dr. Reed and myself to his house for lunch, and it was a lunch. We began with green peas, then radishes, Burgundy wine, then fish, after that fricasseed chicken, then roast beef, with these came Spanish claret, baked plantains, both green and ripe; next came Neufchatel cheese with orange preserve and this was followed by delicious pineapple and another very rich fruit, that I had not tasted before, the sabidillo; cigars and coffee finished a very elegant meal; the table appointments were elegant and very fine even to finger bowls.

Your affectionate husband,
James.

Columbia Barracks, Cuba.
Sunday, February 10, 1901.

My Dear Jennie:

We have just brought down two more cases from mosquito bites and it is well, that I remained here to take notes and observe them. One of them looks as if it might be severe, but I hope he will recover. So far we have not lost a case. . . .

In regard to the Medical Congress, our paper was the only one, that attracted general attention and our work is regarded as being of the greatest importance.

Your affectionate husband,
James.

Camp Columbia, Cuba.
February 14, 1901.

My Dear Jennie:

I believe, that I have found the parasite, but I have thought so a number of times before, so I am going slow now. I am examining a number of mosquitoes to be certain, before I make any assertions. Do not mention it to any one.

Your affectionate husband,
James.

Columbia Barracks, Cuba.
February 21, 1901.

My Dear Jennie:

I have seen something today, that leads me to believe, I am on the track of the parasite. But time will be required to determine and I must examine many insects before coming to any decision.

Your affectionate husband,
James.

War Department
Surgeon General's Office,
Army Medical Museum Library,
Washington, Feb. 26 1901.

My Dear Doctor:

I have yours of the 21st and am afraid this will not reach you in time before your departure. I want you to bring the reprints of our preliminary note, which I seem to have left behind unintentionally. There were about a dozen of them. Be sure and get from Dr. Ames the map, showing the Quamados epidemic, which he was kindly preparing for me. I want it very much and he must have completed it by this time. . . . We must present the clinical side of these cases to the Association of American Physicians in May if possible. . . . I am tied down to the Army Examining Board. . . . You made no mistake by remaining in Cuba until March as February has simply been atrocious.

Sincerely yours,
Reed.

Las Animas, Havana, Cuba.
Sept. 12, 1901.

My Dear Jennie:

I am looked upon as a bogey man down here but the mosquito is mightier than the shot gun.

James.

Columbian University,
Washington, D. C.,
Office of the President.
May 24, 1904.

Dr. James Carroll,
Washington, D. C.

Dear Doctor:

It gives me pleasure to inform you, that at a meeting of the Executive

Committee yesterday, you were appointed Professor of Pathology and Bacteriology in the Department of Medicine and also to be in charge of the Clinical Laboratory in the Hospital, and Dr. H. H. Donnelly was appointed to assist you in the latter position.

These appointments were made for the academic year 1904–5. Your compensation will hereafter be fixed.

Very truly yours
Charles W. Needham.

The New Willard, Washington.
April 21 (or 28) 1907.

Doctor Edward O. Jordan, Department of Bacteriology, University of Chicago. Chicago, Illinois.

Dear Doctor Jordan:

I owe you a letter of thanks for the kind letter of congratulation received from you several weeks ago, and assure you of my sincere appreciation.

I am unable to look the letter up this evening as I have been on my back in bed for 8 weeks, and this is the first day of the 9th week following an attack resembling influenza, and which is accompanied by *an endocarditis.*

As the endocarditis existed three years ago, it is difficult to say, what rôle it plays in my present illness, but at any rate I have had a continuous fever, with a temperature at first intermittent and now remittent.

Fortunately the infection appears to be somewhat mild, although very persistent, and I am holding my own very well.

Blood cultures have been made on two occasions each time with negative results. Agglutination tests have been negative. The blood shows no leucocytosis and the counts are practically normal. This of course excluded the ordinary pus organisms and I will still cling to the possibility, that the infection may have been set up by the bacillus of Pfeifer.

I have written to Doctor Ford of the Johns Hopkins, Doctor Ernst of Harvard and Doctor Winslow of the Massachusetts Institute of Technology in my endeavors to secure a culture of this organism.

My attention has been called to the fact, that Dr. Davis has recently studied some cultures of B. influenzae in your laboratory and as I do not know him personally, I take the liberty to write and ask, if you will kindly request him to forward me two or three type cultures of B. Influenzae in order, that I may have them tested with my serum for aggulutination, and should the reaction be obtained, I fully intend to be vaccinated with the cultures.

Trusting you will not find it inconvenient to comply with this request, I am with sincere regards,

Very sincerely yours,
(Signed) James Carroll.

P. S. Kindly ask Dr. Davis or whoever may send the cultures to forward them by Express collect to me at my house address 1433 Clifton Street, North west.

XVI

A PATHFINDER IN THE ETIOLOGY AND PROPHYLAXIS OF YELLOW FEVER

HENRY R. CARTER, SURGEON UNITED STATES PUBLIC HEALTH AND MARINE HOSPITAL SERVICE[1]

THE first time I met Harry R. Carter shortly after the Spanish American War, was at an informal dinner during which the discussion was on anything but on medical subjects. Carter was particularly interested in speaking to me concerning the life and history of Richard Wagner, and from this our conversation drifted to musical doctors in general. What stood out mostly during the conversation with him was a simple naïve personality. He seemed to me to be almost the personification of humility and one could not suspect back of this sincere undissembled character the discoverer of an epoch-making truth in the epidemiology of Yellow Fever, for it was Henry R. Carter, surgeon in the U. S. Marine Hospital Service, who in 1898 made the valuable observation at Orwood and Taylor, Miss., of the regularity of the interval between the probable date of infection and the development of a secondary case of Yellow Fever. At the localities mentioned, Henry R. Carter found the circumstances favorable for observing and recording the time that elapsed between the arrival of the infecting cases and the breaking out of secondary cases. Carter states (*New Orleans Medical and Surgical Journal,* May, 1900): "The period from the first infecting case to the first group of cases secondarily infected is generally from two to three weeks, the houses having become infected, susceptible individuals who had visited them for a few hours fell sick in the usual period of incubation." This modest U. S. Army Officer then is the originator of the thought that yellow fever must be due to an intermediate host, such as the mosquito, and the U. S. Army Yellow Fever Commission (see chapter on this subject) confirmed Carter's observations, and led Major Walter Reed to change the course of his investigations. Ronald Ross in his "Memoirs" (1923), refers to Carter on pages 4–5, the following words: "That most sagacious observer Henry R. Carter, to whom my highest respects, had actually proved that an interval of about twelve days must elapse between the arrival of a case of yellow fever in a locality and the infection of the first secondary case there—we now know that this interval is spent in the development of the germs within the carrier, the infecting specific mosquito."

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[1] *The Hospital Bulletin of the University of Maryland,* April 15, 1911.

The discovery of the transmission of yellow fever has been credited, and justly so, to the work of Reed, Lazear, and Carroll, the latter a member of the class of 1891 of the Medical Department of the University of Maryland. These men, it is true, carried out the experiments which ultimately led to the detection of the manner of conveyance by which yellow fever is communicated from one individual to another. No fair-minded man can deny this fact, but one name which is closely linked to this epochal discovery has been singularly overlooked—that of Henry R. Carter, class of 1879, University of Maryland. His work was no less important than that of the three above mentioned. Therefore we gladly tender this tribute with the view of placing on permanent record the exact part played by Carter in the investigation of yellow fever.

Dr. Henry R. Carter is a native of Virginia; attended the University of Virginia three years; studied medicine at the University of Maryland, taking the degree of Doctor of Medicine therein in 1879. He entered the United States Marine Hospital Service the same year, in which he has served ever since, mainly in sanitary work, especially in connection with yellow fever. On June 1, 1910, his Alma Mater bestowed on him the degree of Doctor of Laws, Honoris Causa.

This honorary degree was bestowed for reasons that will be set forth in the following.

The work on which he looks back with most satisfaction is:

I. The establishment in quarantine practice of the correct relation between the disinfection (fumigation) of vessels and the detention of their personnel for prevention of yellow fever.

In the Regulations of the United States Gulf Quarantine Station of 1888, formulated by him, the detention of the personnel of the vessel, to cover the period of incubation of yellow fever, was first dated from the *completion of the disinfection* (fumigation by sulphur) of the vessel, their last chance of exposure and infection.

At that time the ports of the United States which required fumigation of vessels for yellow fever dated the period of detention "——days from date of *arrival* in quarantine," or "from date of departure from last infected port," the number of days varying from "three" to "forty" or "to after frost." But in no case did this period bear any relation to the date of freeing the vessel from infection. It not infrequently happened, then, that a vessel would lie her full time—say 20 days—in quarantine and be freed from infection (fumigated) only on the day of leaving. Some members of the crew exposed to infection just preceding or during this process in the hold or other seldom-visited parts of the ship would contract yellow fever and develop it after the vessel had docked and they had gone ashore, the period of incubation—six to ten days—from the time the fever was contracted being sufficient to allow of this.

This principle is entirely obvious and is the pivot of the whole system of combined disinfection and detention, yet it was ignored not only by United States ports, but in all the British ports considered infectible by yellow fever—Gibraltar, Malta, Jamaica, Trinidad, *et al.*—as well as the French and Danish West Indies, and it was not until the United States Quarantine Regulations became mandatory that it became universal in the United States.

II. Devising a system of maritime quarantine by which such sanitary measures, prevention of infection and disinfection, are taken for vessels in a foreign infected port as will enable them to sail free from infection for the United States from such ports, these measures being rendered possible by corresponding privileges granted at the port of entry to the vessels which adopt them.

This was begun in principle in 1890 and applied extensively in 1893 at the cholera-infected ports of Europe. It is now an integral part of Maritime Quarantine System of the United States, and has been of great value both as a purely sanitary measure and as removing restrictions to commerce, especially the last.

III. Work on the epidemiology of yellow fever, including the determination of its period of incubation in man, and especially the discovery of the "extrinsic period incubation" of that disease.

Dr. Henry R. Carter showed that while the existence of a case of yellow fever in an infectible environment can render that environment infect-*ed* in a very short time, a few hours, yet a considerable period of time must elapse before that environment becomes infect-*ive*—that is, capable of communicating yellow fever to other men. This Dr. Carter called the "extrinsic incubation of yellow fever," and fixed its period at "somewhat over 10 days as a minimum." This is in close analogy with the time, as determined by Celli and others, that elapses between the date of an anopheles mosquito becoming infected with the parasite of malaria and becoming capable of conveying that disease. This fact was used with much advantage in epidemiological work in 1898 and 1899. The genius of Reed, James Carroll, and Lazear (it was Lazear who first spoke to Dr. Carter of its implying a living host) used this as a clew to the problem of the conveyance of yellow fever. It is this that gives it its especial importance, and it was no small gratification to Dr. Carter to think that his work assisted in the epoch-making discovery of Reed, Carroll, and Lazear.

IV. In addition to the above permanent work of enduring excellence, presumably we should call attention to Dr. Carter's work in the yellow-fever epidemics of 1893, 1897, 1898 and 1899, the details of which are on record in the United States Public Health and Marine Hospital Service. In these epidemics he represented the United States; in Louisiana only, in 1897; in the entire infected area, in 1898 and 1899. This was purely executive work, and was done with practically no authority, "in co-operation with State and local health authorities." Yet it was adjudged to have been successful. In his districts commerce was carried on fairly well, panic was quieted, and there was no spread of fever to new sections. In 1898, working with the Mississippi health authorities, the epidemic of yellow fever at McHenry, a town in Southern Mississippi, was suppressed. This was the first instance of an epidemic of yellow fever in the far South, well under way, in June and July, being suppressed.

The method used was the control of the human host. The conveyance by the stegomyia was *not* then known, and, except the unnecessary disinfection of clothing, was not all adapted to that end, even in the modern light of the mosquito conveyance. The same methods here instituted were used successfully by White and Von Ezdorf in an outbreak of yellow fever at Hampton, Va., two years later.

We have contrasted this executive work with permanent original or research work. This class of executive work bears a relation to, the determination of the period of extrinsic incubation of yellow fever that is comparable to a campaign, by troops using firearms, does to the discovery of gunpowder. The strategist avails himself of what the investigator discovers, for his pragmatic application.

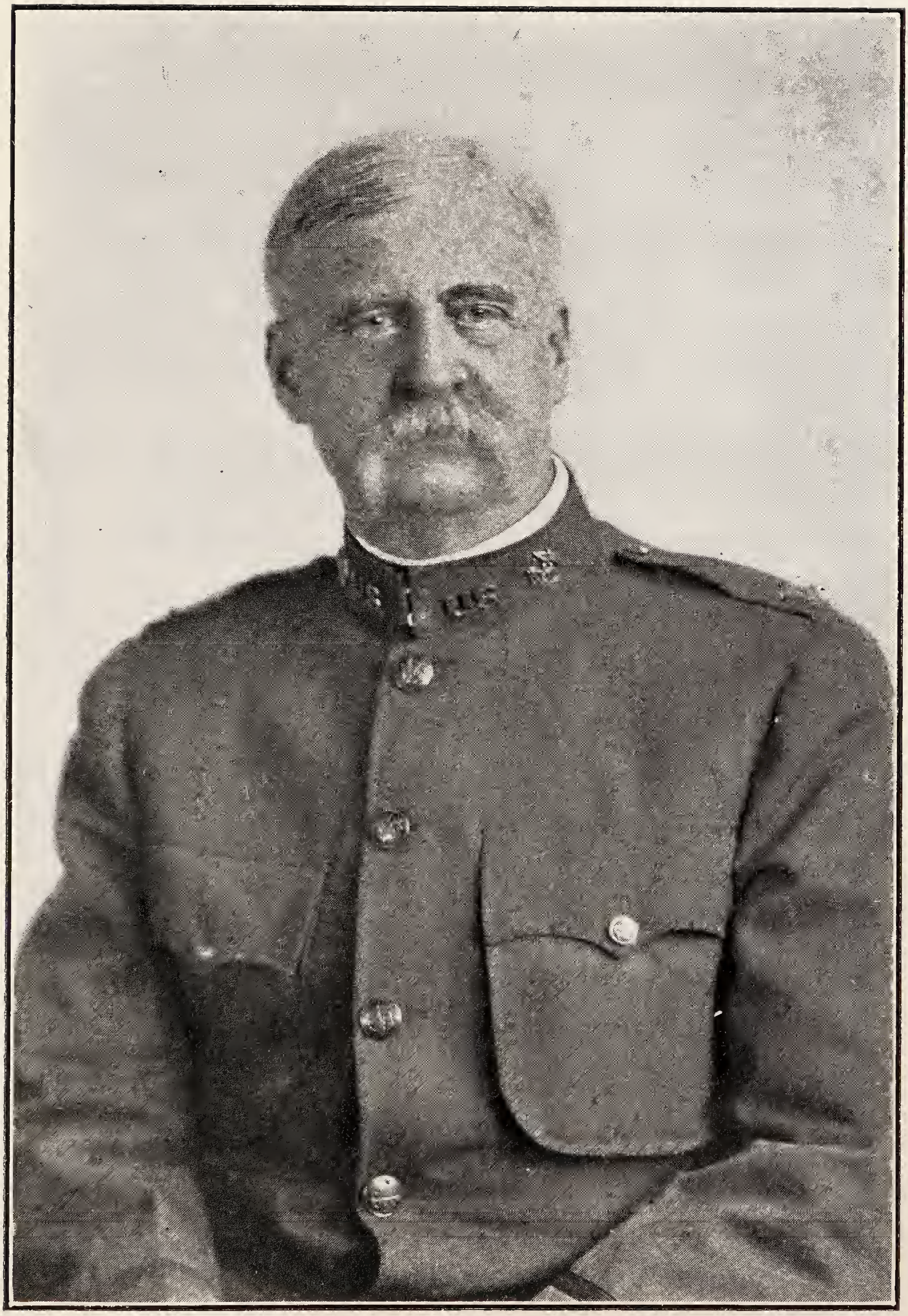
As an estimate of the value of Dr. Carter's work the following letters are appended:

Board of Health Laboratory,
Ancon, Canal Zone, October 4, 1909.

Dr. William Royal Stokes,
City Hall Annex,
Baltimore, Md.

Dear Dr. Stokes:

Before I forget it again I must write to you about a matter that has been on my mind for several months. Dr. H. R. Carter, who was our first



Henry R. Carter

Chief Quarantine Officer, and for four and one-half years Director of Hospitals here, has returned to the States, to be located at Louisville, Ky.

Dr. Carter, as you know, by his wonderfully keen observations made during several epidemics of yellow fever, particularly the one in Louisiana, brought out his theory of an "extrinsic period of incubation," which was of the greatest value in helping Reed to a successful issue in his yellow-fever experiments.

Ronald Ross told me in Liverpool three years ago that he had put in Dr. Carter's name for the Nobel prize. Dr. Carter has never received quite the recognition he deserves for his yellow-fever observations and for his pioneer quarantine work.

As you and he are alumni of the University of Maryland, I want to suggest that you use your influence in getting an honorary degree from the University for him next year. I cannot see why he has been neglected, for his work is recognized abroad and in all literature on the subject, and his name will always be linked with Finlay's in connection with yellow fever.

Reed in letters to Dr. Carter told him how valuable Dr. Carter's observations had been to him in his yellow-fever work.

I sent some slides showing malarial zygotes and sporozotes to Dr. Pearce of Winnipeg, and hope they were of some value to him.

With kindest regards to Mrs. Stokes and the children, I am,

Yours very truly,
S. T. Darling.

I have also made the suggestion to Dr. Perry, our present quarantine officer, about the honorary degree for Dr. Carter.

Board of Health Laboratory,
Ancon, Canal Zone, December 1, 1909.

Dr. William Royal Stokes,
City Hall Annex,
Baltimore, Md.

Dear Dr. Stokes:

I have received from the members of Dr. Carter's family data relating to his work, investigations, and published researches in compliance with your request of October 14.

1st. *The determination of the period of incubation of yellow fever:*

The determination of the period of incubation of yellow fever in man and the placing of the period of quarantine detention of the personnel for this disease on a rational basis, dating it from their last exposure.

The completion of disinfection to cover the period of incubation, while all ports of the United States and Great Britain were dating it from the hour of arrival in quarantine to leaving the infected port, the period of this detention being apparently chosen arbitrarily, as no two ports had the same. The disinfection of vessels on leaving ports infected with yellow fever and dating the time of detention from this disinfection so that the days *en route* would count as days in quarantine. This, of course, he applied to all vessels leaving ports where quarantinable diseases carried by vessels prevailed.

2d. *The discovery that there was a period of "Extrinsic Incubation" for yellow fever and the approximate determination of its minimum duration as somewhat over* 10 *days:*

By "extrinsic incubation" was meant that a certain considerable period of time must elapse between the date that a place received infection from a yellow-fever patient before it became infective, i. e., capable of conveying it to other men. These investigations were begun in 1888. The article was written in December, 1899, and January, 1900, and published in May, 1900. He himself, however, had been using it as a working basis for several years before—no one else accepting it.

3d. *Quarantine work at Chandeleur and Ship Island from* 1888 *to* 1892:

What was really done at Ship Island and Chandeleur was the establishment of the United States Maritime Quarantine System and the systematization of the quarantine methods for yellow fever. Prior to this time the State quarantine stations of the South Atlantic and Gulf ports differed among themselves in regulations, methods of disinfection and period of incubation, and no one station was willing to take the practice of the other. His effort at Ship Island was to get them all to agree upon one method of handling vessels, doing all that was necessary and nothing that was believed to be unnecessary. That was successful, and before he left the Southern ports accepted the practice of the United States stations.

It was while here that he formulated his ideas of disinfecting vessels at the port of departure so that the days *en route* should count as days in quarantine. This was proposed at the meeting of the American Public Health Association at Charleston, S. C., in 1890. It was regarded as impracticable by some and dangerous by others, but is now universally accepted by sanitarians and is the key to practically all the methods of the United States quarantine. Its benefits to commerce can readily be imagined. It was also here, while working upon the problem of the incubation of yellow fever in man, that he discovered the phenomenon of the "extrinsic incubation" of that disease in infected places, which he subsequently worked out in 1898. Prior to this time, however, he was sufficiently convinced of its truth to use it as a working basis.

Since leaving Ship Island and Chandeleur, he has held every quarantine station of the United States from Ship Island to Delaware Breakwater, and had much to do with founding and equipping them. He had charge of the train inspection, communication, and everything save measures in the city in the Brunswick epidemic of yellow fever in 1893. He was in charge of the measures in Louisiana and the train inspection in the epidemic of 1897. He was in charge of the South in the epidemics of 1898 and 1899. In 1898 he was in charge of the epidemic of yellow fever in the little town of McHenry, Miss., which was, it is believed, the first place in the South in which an epidemic of yellow fever had ever been stamped out in the summer time. He organized and had charge of the quarantine of the Island of Cuba in 1899–1900.

SOME OF HIS PUBLISHED RESEARCHES

1st. "A Note on the Interval Between Infecting and Secondary Cases of Yellow Fever from the Records of Yellow Fever at Orwood and Taylor, Miss., 1898."—*The New Orleans Medical and Surgical Journal,* May, 1900.

2d. "A Note on the Spread of Yellow Fever in Houses—the 'Extrinsic Incubation.'"—*Medical Record,* June 15, 1901.

This was written for the medical convention that met at Havana, February, 1901.

3d. "A Correlation of Some Facts in the Propagation of Yellow Fever, with the Theory of Its Conveyance by the *Culex fasciatus.*"—*Philadelphia Medical Journal,* April 6, 1901.

This was written while he was sick in hospital in Baltimore in November, 1900.

4th. "Are Vessels Infected with Yellow Fever? Some Personal Observations."—*Medical Record,* March 22, 1902.

5th. "Some Characteristics of the *Stegomyia fasciatus* which Affect Its Conveyance of Yellow Fever."—*Medical Record,* May 14, 1904.

6th. "The Conveyance of Yellow Fever."—*Medical News,* November 5, 1904.

Read before the Texas State Medical Association, April 27, 1904.

7th. A little memorandum written April 6, 1906; published in the *Medical Record*—date not known—aiming to show that yellow fever was habitualy contracted in the daytime, at least in Panama, in contravention of the statement of Marchanx, Simon, and Salimbini.

Besides this, there have been a number of articles written for publication by the service, giving in detail the measures necessary in handling yellow-fever epidemics—i. e., train inspection, communications with a town infected with yellow fever, etc. There have been, also, articles on other professional subjects, the first one in 1882, entitled "Syphilis in the Negro."

RECENT PAPERS

"Recent Advances in Tropical Medicine." Read before the Pan-American Scientific Congress held at Santiago, Chili, December 25, 1908, to January 5, 1909.

"Malarial Fever Work on the Isthmus." Read before the Canal Zone Medical Association in 1908.

In 1905 or 1906 Dr. Carter's name was presented for the Nobel prize in medicine. At that time he prepared a statement of his relations to the study of yellow fever as a sanitarian and an epidemiologist. The substance of Dr. Carter's statement is outlined above. There is no doubt that Dr. Carter's researches induced Durham and Walter Meyer to make the yellow-fever investigation at Pará, and that it actually influenced (and as to time determined) the direction of Reed's investigations as is well borne out by many verbal and written statements by him. Dr. Reed had been much impressed by the valuable observations made at Orwood and Taylor, Miss., by Dr. Carter on the interval between infecting and secondary cases of yellow fever. Experiments by Dr. Reed confirmed those already mentioned by Dr. Carter in 1898 that at least 12 days must elapse between contamination of the mosquito by the blood of a yellow-fever patient and the communication of the disease. Ronald Ross regarded Dr. Carter's observations on the extrinsic period of incubation of yellow fever as an extremely important one in its relation to the work of the Yellow Fever Commission in Cuba. Dr. Ross has recently invited Dr. Carter to contribute a chapter to his forth-coming book on tropical hygiene.

The above memoranda expresses in bare outlines Dr. Carter's work in connection with yellow fever, sanitation, and quarantine.

Very truly yours,
S. T. Darling.
Baltimore, Md.

To Prof. John C. Hemmeter, Baltimore, Md.

Dear Doctor Hemmeter—A number of the friends of Dr. H. R. Carter have thought for some time that it would be a deserving honor for him if his Alma Mater, the University of Maryland, could confer an honorary degree upon him on account of the early and important work which he carried out and which led directly to the brilliant discoveries of Reed and of his assistants concerning the transmission of yellow fever by the mosquito. I am informed by Dr. S. T. Darling, who has charge of the hygienic laboratory of the Canal Zone, that Dr. Carter has left the Isthmus and is about to settle in Louisville. Dr. Darling has very carefully collected the data which we enclose, and I think that you will agree that this work is deserving of proper recogntion. On the other hand, it adds to the glory of our Alma Mater to emphasize such humanitarian work when performed by one of her sons. *We therefore respectfully express the wish and hope that you will attempt to secure an honorary degree for Dr. Carter.*[2]

Yours very sincerely,
(Signed) Wm. Royal Stokes.

The rest of this letter is quoted from Dr. S. T. Darling's communication which is given in full in the preceding.

CONCLUSION

We have now reached a sufficient degree of familiarity with Carter's work and the method of his research and thinking to recognize that he belongs to Goethe's *Auschauenden.* He is clearly an objective worker who seeks to avoid imagination as far as possible, and humbly submits to the hard facts of observation and experiment.

The day will come when some great thinker—a medical Bancroft—will write the history of American medicine, and then such pioneers and heroes as Jesse Lazear, Walter Reed, James Carroll, Henry R. Carter and Rupert Blue will stand out on the roll of honor in the American Medical *Walhalla* as brilliantly as the names of any scientists, artists, statesmen, naval or military heroes our nation has ever produced.

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[2] In May, 1911, Dr. J. C. Hemmeter proposed Dr. Henry R. Carter for the Degree of Doctor of Laws. *Honoris Causa,* which was conferred upon him at the academic ceremony of the University of Maryland in June, 1911.

XVII

IN MEMORIAM—CHRISTIAN A. HERTER[1]

GOETHE once classified the various kinds of nature contemplation in a comprehensive way: First, the lowest grade is represented by the *Nutzensuchenden*—the utility seekers—who apply that which Nature offers for their utilitarian purposes. The second are the *Wissbegierigen*—or those simply eager for knowledge—the "curious for Nature." The third are the *Anschauenden*—who seek to avoid imagination as far as possible and reduce everything to intuition (from the Latin *intueri,* to look on or into). The fourth group are the *Umfassenden*—these minds operate in a manner opposite to the intuitionalists, for they start from preconceived ideas and seek to encompass (*umfassen*) their problem by seeking a realization of their ideas in Nature.

This classification is a helpful one in endeavoring to understand great minds like that of Herter.

Christian A. Herter died December 5, 1910, in the forty-sixth year of his age, at his residence in New York, of pneumonia. At the time of his death he was Professor of Pharmacology and Therapeutics in Columbia University, New York.

Through his death, scientific medicine has suffered a profound loss, for not only was the manifold character of his scientific activity astonishing and crowned with the most felicitous results, but, in addition, Herter was a philanthropist in the strict sense of that term—for we must distinguish between philanthropy in a *strict sense* and in a *general sense* when considering great men such as he was.

Philanthropy in the *strict sense* I designate as that which can help only by material gifts—the founding of institutions, hospitals, and academic chairs, etc.—by the *other kind* of philanthropy I mean that which benefits mankind through the effects of intellectual effort, by special scientific activity and the evolution of great constructive ideas.

In both of these respects Herter was a philanthropist. Because he had means, it was possible for him to establish lectureships in the University and Bellevue Hospital Medical College, New York, and the Johns Hopkins

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[1] *New York Medical Journal,* June 24, 1911.

University in Baltimore. Through these it became possible to call the most advanced thinkers in scientific medicine and the allied sciences annually to these institutions.

He maintained in New York a private laboratory for physiology and chemistry. It was also only through his financial support that the *Journal of Biological Chemistry* could be founded and maintained. He was, therefore, a practical philanthropist in the strict sense of the term. But the greatest benefits will prove to be those of his intellectual creations, for his many works in the fields of biological chemistry, pathology, pharmacology, and clinical medicine give evidence of an enthusiastic and talented investigator.

His sense of duty to the public welfare was shown by his accepting public duties of responsibility without thought of compensation. He was a member of the first board of directors of the Rockefeller Institute for Medical Research in New York, treasurer of that institute, and consulting physician of the new hospital recently added. He also accepted the position of scientific referee at the invitation of the United States Government, becoming a member of the Referee Board of the United States Department of Agriculture. This tribunal consisted of five counsellors appointed by the President to formulate a proper administration law for regulating foods and chemical products.

Herter was born in 1864, at Glenville, Conn., received an academic education in New York and the degree of Doctor of Medicine from the College of Physicians and Surgeons, N. Y., in 1885. He was an industrious writer, and space is lacking to enumerate other than the more important of his publications. In 1890 he published a work on the *Diagnosis of Nervous Diseases,* and in 1902 his *Lectures on Clinical Pathology.* In more recent years he was especially interested in *Autointoxications from the Digestive Tract,* in which field he completed noteworthy bacteriological and chemical studies.

As collaborator in the International Contributions to the Pathology and Therapy of Disturbances of Nutrition (Berlin), he contributed a fine piece of work to the first volume (p. 275). This contribution on *Experimental Variation of Intestinal Flora by Changes of Diet* was one of his last works.

In his inspiring book *Great Men,* Wilhelm Ostwald divides the great investigators into two groups: 1, The classicists; and 2, the romanticists. They are distinguished by the differences in the reaction times of their minds. The classicists are the slow workers, the romanticists the more rapid. The classicists are the thorough, penetrating, objective workers, who exhaust their problems. The romanticists are the subjective workers, who, according to Goethe (*die Umfasseden,* he calls them), would bound the multiplicity of their problems by the limits of their own ideas. They are more frequently guided by preconceived ideas, are not so thorough, but have great wealth of thought. This superfluity of ideas, in fact, determines the mental pulse rate. For this reason the romanticists, such as Davy, Liebig, Benjamin Thompson (Count Rumford), cannot be so exhaustive as the classicists, because they undertake too many problems and are diverted from the same degree of concentration by the superabundance of their conceptions.

In his sixteenth aphorism, Heraclitus expressed this as follows: *πολυμαθίη νόον ἔχειν οὐ διδάσκει,* i. e., Much knowledge does not lead to understanding. This expression does not strictly apply to the great romanticists, for their services have often led to a deep insight.

Was Dr. Herter a classicist or a romanticist? I knew him well, personally, and have eagerly read his works, but must acknowledge being at a loss how to place this man in Ostwald's classification. He produced much and rapidly, his intellectual reaction was quick, he possessed a remarkable wealth of ideas, and was filled with enthusiasm. These are characteristic of the romanticist.

On the other hand, he lacked certain characteristics of the romanticist —for he was thorough, not superficial, he did not allow himself to be led by *a priori,* i. e., intuitive, conceptions; he belonged to Goethe's *Anschauenden,* contemplative, believing that all experience comes from contemplation.

It remains to be recorded that Christian Herter was endowed with an artistic temperament. He was a technically developed violoncellist, and it often appeared to me that his musical accomplishment had reached the plane of virtuosity. It was my personal privilege occasionally to accompany him on the piano. Unforgettable are the evenings in Herter's home when the physiologist, Jacques Loeb, Herter, and I, after a discourse on physiological topics, enjoyed the relaxation afforded by the compositions of Beethoven, Bach, Brahms, and other master composers.

A great heart, beating warmly for its fellowmen, has been stilled, a profound thinker has found peace; an artist in scientific technique, as well as music, has been called to participate in the "harmony of the spheres." According to Heraclitus, Aph. LXVII: Immortals are mortal,—mortals immortal, living in their death, dying in their life.

XVIII

PRIORITY OF EMPLOYMENT OF THE ROENTGEN RAY IN STUDYING THE PHYSIOLOGY AND PATHOLOGY OF THE DIGESTIVE TRACT[1]

IN June, 1896, an article appeared in the *Boston Medical and Surgical Journal,* page 609, entitled "Photography of the Human Stomach by the Roentgen Method," by John C. Hemmeter. This report, although not published until two months after it was sent to this journal (for I received acknowledgment of its acceptance April 10, 1896), is printed under the date of June 18, 1896.

This antedates any other publication on this subject in the English language. Wolf Becker (*Deutsch. med. Wchnschr.,* March 26, 1896) at about the same time had experimented on the stomachs of animals by filling them with solutions of lead acetate (sugar of lead) after laparotomy, and in doing so he tore the walls of the stomach, which were sewed up again, but not so tightly as to prevent leakage. The solution therefore leaked out into the abdominal cavity and only a diffuse, badly defined photograph was obtained by the x-rays. In April of the same year Carl Wegele (*Deutsch. Med. Wchnschr.,* July, 1896) had advised experimentation with a spiral metallic electrode introduced into the stomach. His paper was merely a suggestion—he did not report that he had produced any real skiagrams of the stomach; in fact, by this method it was impossible to make them. One might photograph the electrode, but not the stomach. Becker from his own statements worked only on animals and Wegele could produce no photographs whatever of the stomach.

In 1896 I had already made skiagrams of the entire stomach by the methods described in the *Boston Medical and Surgical Journal,* June 18, 1896. The work was done in the physical laboratory of the State Normal School in Baltimore with the assistance of the professor of physics at that time, Prof. A. Hammer, and my associate, Dr. Harry Adler. The work was elementary and there were failures that were unavoidable with the apparatus available nearly thirty years ago. It cannot be denied, however, that x-ray photography of the digestive canal was originated as pioneer work then and there, and the work of Rieder, Haudek, Holzknecht, and other meritorious clinicians in Germany and this country came after mine. It has occasionally been asserted even by American writers that the *x*-rays were not employed in the diagnosis of precisely stated conditions of the digestive tract until 1907, and then only by Germans. It has been acknowledged by Ewald and Boas as well as Haudek and the entire school of clinical medicine in Vienna, where much of this special kind of clinical radiology was done, that the first employment of *x*-rays for the diagnosis of ulcer of the stomach was conceived and undertaken by me.

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[1] Journal American Medical Association, October 5, 1912.

In fact, I possess letters of appreciation and congratulation from my friends, Professors Ewald and Boas of Berlin, in which they compliment me and my co-workers as being the pioneers of this special type of radiology. The main reason why they are so conversant with the history and character of the radiologic work of my clinic is to be found in the fact that I published the first complete account of it in a German journal ("Neue Methoden zur Diagnose des Magengeschwürs," *Arch. f. Verdauungskr.*, 1905, xii, No. 5, p. 357; also Experimente welche die Möglichkeit demonstrieren Läsionen der gastrischen Mucosa durch *X*-Strahlen sichtbar zu machen, *ibid.*, p. 360; also Demonstration des Magengeschwürs beim Menschen mittelst der Bismuth Subnitricum und *X*-Strahlen Methode, *ibid.*, p. 362, 1906).

Since then I have added nothing new to the first method, for all later advancement has consisted in the improvement of apparatus—unless one is willing to accept as an advancement the use of calcium carbonate and calcium phosphate or of aluminum silicate instead of bismuth salts as first used by me. Leading German, Austrian, and French clinicians have in the friendliest terms conceded to my clinic the historic priority of the methods of special radiology mentioned in the preceding. Furthermore, in the German article of July, 1906 (*Arch. f. Veraduungskr.*, xii. p. 363), I pointed out that the method was useful in the diagnosis of carcinoma of the stomach but that it had failed in two cases of cholelithiasis in which the gall-stones were later removed by operation.

Prof. Harry Adler and Dr. Robert Bay have repeated my experimental and clinical work in gastric radiology and Dr. Adler published his first results, which are confirmatory, in *The Journal of the A. M. A.* ("The Diagnosis of Ulcer of the Stomach and Duodenum by the Roentgen Ray," Nov. 12, 1910, p. 1725). Adler in this article also reported a number of cases of duodenal ulcer recognized by radiology and found later at operation.

It has sometimes been erroneously stated that Jolasse preceded me in employing radiology for the diagnosis of gastric ulcer. A scrutiny of

Jolasse's publication (*München. med. Wchnschr.*, 1907, No. 29) will clearly prove that it appeared more than a year after my contribution to the *Archiv für Verdauungskrankheiten* (July, 1906). The most recent German text-books on diseases of the stomach (Hans Elsner, "Lehrbuch der Magenkrankheiten," p. 286, and also the new edition of Boas) have stated correctly the chronologic sequence of this radiologic endeavor. Other publications by my associates are by Adler and Ashbury (Am. Roentgen Ray Soc., September, 1910; also Adler and Ashbury, *New York Med. Jour.*, Oct. 7, 1911).

XIX

HISTORY OF TENDENCIES OF MODERN PHYSIOLOGICAL DISCIPLINE IN MEDICAL SCHOOLS[1]

THE development and growth of physiology in the last twenty-five years has been so great, that even the well trained mind of the technical physiologist finds it difficult to keep abreast of the progress that has been, and is still being made. It is all the more astonishing, therefore, that in medical schools in which the councils of men prevail who have only an exclusive medical training, one still meets with the idea that physiology could be taught by men who are not trained physiologists, but by practitioners of medicine who obtained their knowledge at second hand from textbooks only, and have no personal experience with research work, nor even an intimate knowledge of the truths, doctrines, and theories of modern physiology.

With regard to chemistry, even the orthodox medical mind has long ago abandoned this idea, and in even the poorest medical schools, chemistry is taught by a technically trained chemist, and pathology usually by a trained pathologist. It is for the purpose of clearing up this erroneous opinion that this article was written. It was first discussed with a number of eminent physiologists in this country and abroad and many of the ideas were received from Jacques Loeb in an almost daily intercourse at Woods Hole, Massachusetts, during the summer of 1911, and also from other experienced teachers in physiology who were engaged at the Woods Hole Marine Biologic Laboratory. The article by Verworn entitled, *Aufgaben des physiologischen Unterrichts,* has inspired the main arguments. I am also indebted to Professor I. P. Pavloff, of St. Petersburg, for much inspiration in writing this article, gained as well from his publications as privately in conversation with this great physiologist and brilliant scholar.

Some of the principal thoughts I have quoted from the preface to my *Manual of Physiology* (Philadelphia), the principal one of which is contained in the remark on page 10 of the preface. There are systems of intellectual economics and in physiological pedagogy and physiological discipline they demand no cast-iron rigid schemes, but elastic fundamental concepts—lifelike, plastic aspects, not a mass of disconnected facts. Only that which is assimilated, of and by itself, continues to live and work in the intellect.

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[1] Address delivered at opening of the course in physiology at the University of Maryland, October 1, 1912. New York Medical Journal, December 7, 1912.

In the following I shall endeavor also to give the reason why the secret of success in the future teaching of physiology is to be found to a large extent in general and comparative physiology.

Physiology is, at the present day, not a finished science with subject matter established and immovable in every particular, in the same sense as is her sister science, descriptive anatomy. In this respect the anatomist is far more happily placed. So far as the descriptive anatomy of man is concerned, at least, the structure of the science is, in a manner, complete. The subject matter for academic instruction is outlined within certain circumscribed boundaries. Hence the teachings of anatomy delivered in the various universities agree in the main. Not so in physiology. True, there is also a vast area of facts in physiology that can be considered established, and which must self-evidently form the basis of all physiological instruction. But these facts are interrelated in a very loose and rather superficial manner, and frequently stand dissociated and alone. The science of physiology is still very much in a state of development.

On the other hand no biological discipline makes as many demands on its votary. The number of auxiliary sciences is so great, the methods of investigation are so complicated, the problems so difficult, the subject matter so delicate, that it can easily be understood why investigators confine themselves to one or but few of its many special branches. It is no longer possible for an investigator to devote himself with searching thoroughness to all the departments of physiology as was the case in the days of Johannes Müller, before physiological investigation and method assumed the present proportions. To-day physiologists become conversant, one in the physiology of digestion and the metabolism change of living matter, or of electrophysiology, another in that of the central nervous system; one confines himself almost exclusively to the chemical composition of certain forms of animal matter, another to the mechanical principles underlying circulation; one to experiments on change of matter, another to the physiology of the organs of perception; some, again, to the physiology of the enzymes. This onesidedness in investigation, which is due to the natural limitations of human life, and necessary in a measure if research work is not to drop to a dead level of mediocrity, brings with it a certain differentiation in physiological instruction according as the topics are considered highly important or insignificant with the time limitations of the academic course.

Thus it happens that someone attending physiological lectures at two extremely diverging universities, would almost receive the impression that he is taking courses in two different sciences and, all the more so, if the methods of instruction are at variance (Verworn).

It now behooves us to explain the importance of physiology in the training of a physician. Physiology is the science of life or of the biological phenomena in organisms or, as has been said, of the chemical transactions in, or functions of, the organism and its parts. In this vast field of general physiology, the special physiology of the human body is of prime importance to the future physician. The fundamental importance of this branch of physiology in the study of medicine has never been questioned. The physiology of man, together with human anatomy, constitutes the indispensable basis on which the structure of medical science and skill has been reared since the days of Galen and this will ever be so. Without physiology there will be no practice of medicine. Physiology is to picture clearly the biological activities in the normal body, the part the several organs in the human body play in the activity of life under normal conditions, so that at any given moment an account can be rendered of what is actually going on in the human body. The more plastic and accurate the picture, the more clearly possible this visualization of the activities in the body and its parts under normal conditions, the more profitable it will be to the physician at the bedside, for sickness is nothing but life under difficult conditions.

But how can the abnormal be recognized and understood when the normal is not sufficiently known? How could a physician restore to health,—that is, correct the course of abnormal digestion,—if he did not know these biological functions in a sound body? The more thoroughly grounded in physiology a man is, the better a physican he becomes. The reverse may also be stated as true, that the brilliant physiologists in the history of the science have been such as were also physicians. But the training in clinical medicine alone does not fit one for the teaching of physiology as well as the training in the latter fits one for the pursuit of clinical medicine (see Hemmeter, The Technical and Scientific Qualifications of a Teacher of Physiology, in *University Hospital Bulletin,* Baltimore, May, 1912).

It is not out of place to point to the dangers into which lack of physiological knowledge can draw the science and practice of medicine, and into which it has been led by various pilots in the present day. It is unnecessary to say that physicians do make mistakes at the bedside for which they must pay the penalty, mistakes that could easily have been avoided with a fair knowledge of physiological processes, but it would be well to glance at two great blights that are exceedingly annoying to the medical profession at the present time.

Much has been said recently about quackery and of the struggle of physicians against it. Few are perhaps correctly informed on the enormous spread of quackery and that, too, in an age that points with pride to its general culture. In what an alarming light this reveals the true status of the education and discriminating ability of the general public! The consideration of this question ought to be taken seriously to heart; unpleasant contact with it is inevitable in the practice of medicine. The whole matter would be comparatively harmless if it were confined to barbers and teachers and patent medicine men, or people of like professional training, as it was in former days. The doings of men of that stamp could be witnessed with indifference; for those who surrender consciously to their treatment would only have themselves to blame and, although generally too late, they would eventually fall into some physician's hands.

But the situation has become far more ominous to-day. Persons equipped with the title of doctor of medicine are not abashed to practice quackery much after the manner of the charlatans and wonder doctors of the sixteenth and seventeenth centuries, who meandered from pillar to post in their carts and in fanciful garbs, with tinkling bells, lauded their remedies before the multitudes of those alarmed at their ailments. Whenever a man on whom a medical faculty has bestowed the title of medical doctor, practices medicine under such guise, the public cannot escape being shamefully imposed on. Such conditions are possible only where the foundations of medical science are lacking. A man who is reasonably acquainted with the biological activities in the body, and who has gained but one clear glimpse into the physiological happenings in the organism, could never lower himself to the fallacious practice of quackery, unless he did so against his better judgment and from base motives.

Another danger to which the physician is exposed in consequence of inadequate ideas about the nature of biological phenomena and the physiological processes in the organism, lies in the total misconception of the physiological significance and the therapeutic value of many present so-called specifics. On this score, practice has made many blunders in recent times. When the untutored patient demands of his physician as large a bottle as possible of intensively colored bitter medicine for every trifling ailment, this is excusable on the ground that the quaint ideas on the healing of the sick of bygone centuries yield reluctantly to the better insight of the public mind. Hence, the physician is often obliged to prescribe for no other reason than to "do something." But when, instead of treating diseases with an insight into their causes, into the processes of the organism, and in harmony with physiological demands, the physician simply seizes a chemical of doubtful value, recently thrust on the market with a big "splurge" by some chemical concern, he becomes guilty, rigorously speaking, of malpractice committed out of regard for his convenience: for what does he or anyone else know of the physiological effects of the preparation in question?

The danger would, however, not be very great, if the state of modern trade in pharmacology were not in the most deplorable condition imaginable. Instead of originating solely in medical research and science, as formerly, modern scientific pharmacology is overshadowed by the product of manufacturers of chemicals. A large number of the scientific representatives of pharmacology devotes itself, not to minute investigations on the physiological effects of remedial substances, but principally to the purely chemical questions on the constitution and chemical structure of substances contained in the animal body, or to experiments on metabolism. The pharmacologists are frequently only physiological chemists. In the meantime the chemical factories are producing multitudinous quantities of "new specifics."

Experiments after the same general scheme are conducted on the effect of the most varied chemical combinations, and as is often the case, in the most slipshod and superficial manner. It is usually the same procedure in each case. A chemical is injected into a rabbit and then blood pressure and respiration are graphically noted. If a change occurs in these external manifestations, the substance can pass as a remedy. Then the dosing of human beings is tested in amounts that can do no very serious damage, the article is manufactured in large quantities, is dubbed with a beautiful name, and in a short time floods the market with the highest recommendations only to disappear entirely within a few years. In the meantime hundreds of new remedies have been "discovered" in the same way, with therapeutic effects equally problematical.

The short time which the enormous production of pharmacological products allows for the physiological testing of the separate articles, naturally precludes establishing what their effects are in the animal body and locating their points of attack. The bare investigation into their influence on heart action, on blood pressure, and respiration throws no light on these questions. Every physiologist will scoff at this manner of examining a remedial substance, for the heart action, blood pressure, and breathing are purely external resultants of a large number of quite different and individual factors. Even after it is known that a substance influences blood pressure, next to nothing is known of its specific effect and its point of attack in the body. The same thing obtains concerning the influence on numerous other purely external physiological phenomena.

It is evident, therefore, how matters are with the great abundance of "specifics of to-day" that rise like meteors in the pharmacological heavens. It is advisable, therefore, to be sparing in the use of drugs and to adhere closely to the few thoroughly tried and approved preparations whose physiological effects are already known with some degree of precision.

From the foregoing it is evident how urgent to the physician is the need of a clear, comprehensive view of the physiological occurrences in the human body. The deeper the insight into these matters, the more nearly correctly will the physician be able to pass judgment on diseases and their causes, the more purposefully will he choose his course of treatment, and the more successful will be his practice.

It is a great mistake, common in the practice of medicine, to place treatment by drugs in the foreground of therapeutics. The physiological factors in the art of healing have for a long time retreated too far behind the use of remedies. A practical physician, who has a vivid picture of the biological activities in the human body and of their more subtle causes, will not neglect the natural, purely physiological factors for the pharmacological.

Man is the most interesting and eminent subject of all science, for there will never be any subject that more immediately and more closely concerns man than man himself. This last must swell the heart of every medical student with pride in his profession, since man is the exclusive object of his whole activity. The physician ought to be the best judge of human nature. The physiology of man, therefore, represents for him the most important part of general physiology, not only in its general human, but also in its specific scientific aspect.

But the physiology of man is extremely comprehensive, and unfortunately here, as everywhere, "art is long and time is fleeting." With due regard for the many demands and the short time allowed to physiology in the study of medicine, a selection of the most important topics must be made from the vast area of human physiology. The most important object of this teaching appears to me, not to present a great mass of isolated facts collected from every chapter of human physiology, but rather to avoid disconnected subject matter, the memorizing of which can be mechanically acquired from books, to give a plastic picture, a profound understanding of all the activities in the human body, a unified complex of fundamental concepts around which, as it were, the special facts as they present themselves are gradually to crystallize by themselves, i. e., of their own accord. Only that which is spontaneously assimilated, lives and toils and labors on in the mind. Isolated facts without organic connection, artificially and laboriously acquired, constitute dead knowledge to remain worthless and soon to be lost by the wayside. Fundamental objective visualizing, unified live concepts, not a profusion of disconnected isolated facts, is what is needed in economical mental housekeeping.

From this point of view special stress is to be laid on a part of universal physiology that has very slowly, but in the course of time, increasingly tended to crystallize out from the specialized researches in physiology as a precious gem, that is, *general physiology.*

The general physiological concepts which contain all the properties and laws common to all living bodies, furnish, in a certain sense, the key to all the doors behind which the numerous special facts concerning normal life lie hidden. Without this master key, only the outside doorplate comes into view, but the nature of the facts that slumber behind, remains unknown, and the separate facts cannot be called to life to be united into organic complexes. In the absence of summary knowledge of the facts of general physiology, the special physiology of man gains no profound understanding; without it the most beautiful attainments in this field remain lifeless. The physician, however, who has enjoyed a thorough training in general physiology, sees deeper down into the organism, before his mind's eye he visualizes the processes in the diseased cells, he beholds the causes of their changed conditions, and he will be better able to remove these causes. The less of general physiological visualizations the healing art has at command, the lower it sinks into barren and tiresome schematization. (Verworn.)

Among the chapters of general physiology of special importance to the physician, besides the knowledge of general biological phenomena, are the cellular processes, that lie at the foundation of these, above all experiences on the general hypotheses of life, and with special emphasis the laws of irritability. These chapters are of preëminent interest to the physician, since diseases represent nothing but the consequences and the external expression of changed conditions of life, or in other words, the deviations from normal biological phenomena in the cellular constituents of the body, induced by irritations. Hence it is of extraordinary importance for the physician to be acquainted with the biological phenomena in the cells under normal conditions, on the one hand, and on the other hand, to know what deviations from the normal life are superinduced in them by irritants according to their quality, intensity, and direction.

The cellular pathology of Rudolph Virchow, on which all medical conceptions rest, and which must ultimately form the starting point, and basis, for all efforts toward the modern pathology of tumors, assumes cellular physiology as a premise and can only grow extensively as the knowledge of the normal cell processes and the lawfulness of the effects of the various irritations expands.

In consequence of a sufficient number of general physiological visualizations, the special facts of the physiology of man will appear in quite a different light. The life proceedings in the human body will be regarded from a higher level, with broader view, closer connections, and relations will be discovered everywhere, where, otherwise, only isolated disconnected facts presented themselves. This is absolutely necessary with the vast amount and extensive scope of subject matter. The several departments of physiology have assumed such proportions that some of them have moved out of touch with one another and have developed into individual sciences. Thus the science of embryology and morphology, which ought to form an integral part of physiology as it formerly did, has been divorced completely from modern physiology at a serious loss to both physiology and embryology, for coincident with this separation, the mutual understanding of their several problems and mutual sympathy for their way of thinking vanished also.

Embryology is no longer lectured on in physiological courses. There is at least one justification here for the separation into a special branch. Embryology has its own special problems, that have proved so immensely manysided in the last decades and have called into existence an amount of scientific work so copious as to make even a survey well nigh impossible.

Quite different is the case of physiological chemistry, which is another department of human physiology, and which has also striven to gain a certain independence. Physiological chemistry has made special effort to branch off as an independent science under the influence of Hoppe-Seyler. The independence of physiological chemistry from physiology cannot be justified. Physiological chemistry has the same general aims as physiology, that is, the investigation of biological phenomena. This aim physiology pursues with methods varied according to the demands of every special question. In its investigations physiology must apply physical and chemical, surgical, and microscopical methods, according to the exigencies of the case. The fact that, with so many demands, each investigator selects some special field which he elaborates according to his special methods, is both tangible and justifiable.

The representative phases of the science vary considerably in the different universities, but physiology on the whole is rescued from narrowing monotony. To institute however a separate science on the ground of special methods, such as physiological chemistry, is entirely erroneous. A science is founded on a problem, not on a method; in the latter case inherent justification is wanting. If this proviso be lost sight of, then physiological physics, physiological surgery, and physiological microscopy could segregate themselves as independent sciences with the same justification as physiological chemistry. This would lead to hair-splitting dismemberment of physiology into specialties that would soon have no common ground of interpretation, and would overlook the one and only aim and purpose in physiological investigations, the study of life.

Physiological chemistry has already confined itself in the main to but few special problems. The two foremost propositions in physiological chemistry are investigations on the composition of albuminoids and experiments on the changes of matter in animals; the former, together with the study of hydrocarbons, have been and will be, the work of pure chemistry, the latter must be reincorporated into the program of physiology where it belongs (Verworn).

Separation into even more specialized departments has already been carried to such a degree of nicety as to lead to embarrassing conditions. Physiology is, and must remain an undivided science. The application of the methods of chemistry is a part of physiology as is the application of any other method, and academic instruction in physiology ought, correspondingly, to be one and undivided, and not be separated into courses on physiology and on physiological chemistry, each conducted independently by distinct lectures. Such a division of physiological instruction would lead to utter mangling of the more special chapters of physiology, for there is no chapter in the whole of physiology in which chemistry does not play an important part, or which could be discussed without entering upon chemical questions. The student would be unable to get a useful survey of the subject as a whole.

Pursuing division on principle would also seriously embarrass investigation. If each instructor is to adhere closely to his own department it will give rise to neglect or abandonment of the other departments. The difficulties attending physiological investigations preclude the possibility for every physiologist to engage in individual research work on chemical problems, and likewise for him, whose dominant interests are chemical, questions of physics and microscopy are precluded. Each specialist is obliged to work with the best methods at his command. The lecture course on physiology and the laboratory offers the only clearing house where the physiologist can preserve a unified survey of the subject as a whole if he is forced to take notice of the problems, methods, and results outside his own province. Without this opportunity and this injunction there would still be physiological chemists, physicists, etc., but ultimately no physiologists; thereafter the general survey and with this a liberal and unconstrained grasp of the problem would be utterly lost.

The preparatory study of pure chemistry should be in the hands of chemists, that of physics conducted by physicists. Instruction in physiology as a whole will then contain elements in chemistry, physics, anatomy, histology, zoölogy, and botany. The one aim, however, to be pursued in all physiological instruction should be to give a comprehensive unified picture of what is going on in the living body. In this sense the several chapters of physiology would be taken up in a certain sequence, issuing without constraint from the very nature of the subject.

The discipline is best begun with the fundamental principles of general physiology. At first these manifestations of energy, which are connected with transubstantiation in the body metabolism should be considered, such as heat, motion, evolution of electricity, the processes in the nervous system, and the functions of the organs of sense. This should be followed by a discussion of the entire change of matter in the human body, pursuing the fate of the alimentary substances to their excretion as waste matters; the properties of food, secretion, digestion, absorption, breathing, the composition of the blood and of the lymph, circulation, the changes of tissue, formation and excretion of the urine. It would thus be seen that the functions of one organ articulate with those of another, and how they keep the clockwork of the human body going in unison.

The several special departments of the physiology of man vary in educational value for the physician. With the time limitations imposed upon physiological instruction, a retrenchment in departments of minor import will be necessary. In this respect the physiological instruction of the future will have to take on a shape different from that of the latter half of the past century. Thus, it would not be admissible to allow as much time to electrophysiology as formerly. The phenomena, in view of the development of electrochemistry, have lost much of their theoretic interest for the biologist, and have become almost worthless to the physician who is concerned with such a picture of the biological processes in the body as can be of practical service to him. With all the inherent theoretical interest in the physiology of the muscles, it will be practical to devote less time to them than formerly, when it often embraced the main part of the lecture course or laboratory of a whole semester. This was formerly justifiable in a way because the general physiology of the muscles and neurology then constituted that part of physiology in which the physician became last acquainted with a general idea of physiology. Now that the conceptions, doctrines, and theories of general physiology have been segregated from the various departments of special research, and have developed into a comprehensive complex; now that physiology forms a separate department of physiology in general (collective physiology) and is treated as such in teaching, a minute course on the general physiology of the muscles is no longer offered in an academic course of medicine. The time thus gained is to be employed in other departments of physiology such as a course on the change of matter, the physiology of the nervous system and the physiology of the internal secretions, etc.

The amount of instruction in physiology of muscles and nerves has been indicated in my *Manual of Practical Physiology.* Looked upon from the extreme utilitarian standpoint, it still constitutes a valuable preliminary training for the neurological clinic.

An exceedingly difficult question on the choice of subject matter in physiological instruction still remains to be answered: Shall physiology consider psychological questions and to what extent? Johannes Müller defended the thesis: *A physiologist, nothing but a physiologist.* There is a great deal to be said on this question and it cannot be disposed of in a few words. It might be well for everyone engaged on biological problems to consider casually the question of the relation of material to psychic life, the question of the relation of body to soul, as it is called in current dualistic expression; for this question confronts us at every turn in biology. It plays into all possible contingencies and causes obscurity and confusion. Every biologist ought to come to terms on this question, and this can be done only by entering upon a critique of the understanding and especially upon the problem, what understanding is, and what its limitations are. No naturalist can get around this, the supreme question in science, unless he is willing to surrender needlessly to blind specialism.

But whatever the answers to these queries, complete elimination of psychological elements from physiology and from physiological instruction is practically impossible. The chapters on the central nervous system and on the organs of perception cannot yet be discussed without psychological concepts, such as sensations, conceptions, consciousness, etc., otherwise than to render their presentation unintelligible. Besides, the later lectures on psychiatry will engage psychological considerations abundantly. Hence it is impossible to avoid entering upon psychological questions in physiological instruction, even if it were desirable to do so, and hence it will be advisable to take a firm stand and gain some clearness right at the start.

In consideration of the great complexity of the subject of physiology, and the difficult methods of approach in gaining insight and knowledge, it is necessary, particularly in the lectures, to make the instruction as objective as possible. This is to be accomplished in two ways.

On the one hand the content should be presented in a simple, plastic, living style. The utmost attainable clearness and simplicity are emphatically to be aimed at. Burdening the memory with numbers, tables, and formulas that are difficult to master and are not required, should be avoided as much as possible. True, numbers and tables should not be withheld, but should be offered only to illustrate certain facts. The student ought to extract facts and ideas and to have at his command visualized pictures, not tables and numbers that can be obtained from any reference book when needed. An error into which physiological instruction easily falls is—a too detailed entering upon the presentation of the more complicated methods and the arrangement of apparatus and experiments in special lines of research work. Surely, for better understanding, the student ought to be informed as to the ways and means of arriving at certain physiological experiences. But this end is fully reached, if the principle of the method or of the apparatus is sketched in the most elementary outline. Exact knowledge of every minute detail, on the contrary, complicates understanding, burdens the memory, and serves no future purpose. These items belong to physiological research work, but not to physiological instruction. In either case it must not be forgotten that the live subject in hand is to stand in the foreground. Method is only means to an end. (Hemmeter, *Manual of Practical Physiology.*)

On the other hand, it is a function of physiological instruction to give a thorough knowledge of the more common and simpler methods which physicians must make constant use of. An acquaintance with certain chemical, optical, microscopic, electric, and surgical methods is indispensable for special medical purposes.

Other important factors toward attaining visualization of the subject matter of the lecture, are demonstrations and experiments. The most beautiful and clear oral presentation cannot be a substitute for that which has been "eyewitnessed," provided it has been understood. The lecture should be aided and supported as much as possible by demonstration on its subjects; but too intricate and unsurveyable experimental appliances and arrangements should be avoided, since these, according to experience, do not help understanding and often only perplex and subvert it. Clearness, perspicuity, comprehensibility, these must be the shibboleth of physiological instruction. Where the experimental contrivance or the apparatus is too complicated, it will be far better to supplant it with a single schematic sketch on the blackboard or by a correspondingly simple figure on a chart. These devices, and above all the drawing of objects on the board during the lecture, ought to be liberally employed; for, on the one hand, the instructor can omit what is subordinate, or what would obstruct understanding, and, on the other hand, the audience sees the object gradually developing before it, so that, in following the drawing, the demonstration becomes intelligible, whereas, if confronted with the finished drawing, the latter would be quite unintelligible. It is my own practice to draw on the blackboard whenever it is feasible.[2]

Finally, another important question in conducting physiological lecture courses is, whether and to what extent shall theories and hypotheses be discussed? There are many who consider it a transgression if "improved hypotheses" are spread abroad in teaching. In defense it may be said that there is no such thing as physiological instruction without theories and hypotheses. I have shown at the outset that physiology, on account of the many difficulties to be overcome in investigating biological hypotheses in order to unite facts under general points of view, and to bring about some connection between them, must arrive at new facts, by experiments that originate in hypothetical reflections. For the same reason that physiological investigation makes use of working hypotheses, physiological instruction demands consideration of such hypotheses, if this is not to come down to a worthless enumeration of disconnected, barren, isolated facts from which no visualized lasting pictures of the processes in the body can be gained.

Frequently it is only through consideration of theoretical and hypothetical conceptions in physiology that it becomes intelligible how attention was directed to some important problem in physiological investigation and why certain experiments were instituted, all of which would otherwise seem unprovoked and inconsistent. Theory and working hypotheses form the mental bond that combines the several facts to complexes of coherent consistency. But physiological instruction is under obligation to sift clearly between fact and hypothesis. The student is therefore not to be perplexed at such expressions as "this is yet unknown," "this question is still in total obscurity," "the following idea, with some degree of probability has been temporarily advanced," etc.

The physiological laboratory work is an indispensable complement to the lecture course, and for various reasons. The best theoretical instruction, the most lucid and most perspicuously comprehensive demonstration will not accomplish what an individual preparation, what an experiment, personally conducted from start to finish, what an operation individually performed can do for the student. Not by being shown how, but by completely doing so, do we acquire actual mastery. In consideration of the time, only a limited range of experiments and demonstrations can be carried out in the lecture, and that only after careful and conscientious preparation. The student does not see the preparation, he never learns of the many details requisite, he is not aware of the great amount of circumspection and forethought that the simplest physiological experiment entails. The instructor, who is constantly manipulating on the other hand, often takes no account of the many little mechanical requirements that he meets, and easily neglects to call attention to more important points. There is no answer to the questions that may arise during the lecture. For all this the physiological laboratory offers a compensation. Here the student conducts all those experiments witnessed in the lecture room and some that could not be produced in the short time of forty-five minutes.

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[2] Projection of peristaltic movements of the digestive tract and of the microscopic appearance of the living, circulating blood, on the screen by the cinematographic method teach more in one hour, than many hours of lecturing.

Of course, some physiological knowledge is necessary for all this, hence the laboratory ought not to be open to the student until the second year of physiological instruction. But there is another reason for laying great stress on practical exercises, and that is, their technical side. According to experience, many of the younger physicians have had but little opportunity up to the final examination to acquire a certain amount of dexterity, care, and cleanliness in the handling of apparatus, instruments, chemical reagents, living objects, etc. But these are all things that engage the physician constantly in his practice. Without their unfaltering and absolute mastery, he would have not only to renounce many methods of investigation and treatment, but he would commit one of the worst blunders for a physician, striving after a practice; he would forfeit the confidence of his patients by his awkwardness and indecision. Decision, adroitness, dexterity, and cleanliness in bearing, as well as in manipulations, are the most important external qualifications of a physician.

Working in the anatomical laboratory, which offers a good opportunity for practice in such things, is, however, only one-sided. There is, moreover, a difference between performing an operation on a living organism, and doing the same on a bloodless corpse. The young physician will have ample opportunity to experience this. Things lock quite different when the blood is coursing through the minutest parts, and in the end the physician will have dealings mainly with living human beings. The physiological laboratory offers a manysided possibility for the exercise of manual dexterity in every possible way. It is for this reason that so many detailed operations on animals are described in my *Manual of Practical Physiology*, of which Professor H. J. Hamburger and Doctor Laqueur speak in such favorable terms in their review of this work (*Biochemisches Zentralblatt*, 1912).

Hence there will not only be vivisection, but electrical contrivances, preparations for the microscope, graphic representations of breathing, of blood pressure, of muscle movements, chemical reactions on animal fluids, weighing, measuring, and much more. It is of great value to the student to carry out every detail himself to a nicety, as the simplest manipulation can be of great service.

But beside this practice in dexterity and adroitness in the handling of both live and dead objects, the physiological laboratory ought especially to offer opportunity for manipulating all things that find constant application in practice. The student is to make albumin; urea; sugar-tests, etc., to investigate the properties of normal urine, to examine the fluids of the body under the microscope, to execute subcutaneous and intravenous injections, to tie arteries, to stimulate with electricity, ascertain the intensity and volume of electric currents, etc.

For all these reasons it would seem that the practical laboratory work in physiology is quite indispensable and its pursuance in the second year before the final examinations is highly recommended. In order to make the laboratory work possible, in the short time devoted to the study of medicine, its time allowance has been fixed at two periods of three hours each week. This is not sufficient.

If lecture courses and laboratory work are to be considered absolute essentials in the physiological training of a physician, then it must be made possible for those who are not satisfied simply with the required course, and for those who for various reasons could not follow some part or another of the physiological lectures, to be set straight on certain chapters. The physiological "seminary" or the conference and special lectures and courses on minor or major departments of physiology serve this purpose. But opportunity must also be offered to satisfy and promote somewhat further-reaching interest in physiological questions of some students. This need is met in the individual research work performed in the physiological laboratory where instructors and assistants are ready to give the necessary stimulus and aid. Naturally only such will engage in this independent work as have finished the required instruction and training in physiology. Beside this, the "seminary" should be open even to the younger students who are still attending the regular courses on physiology and who desire some little incentive by being heard on physiological questions. This "seminary" is not to be a reviewing hour in which to prepare for examination, and for which certain tasks are scheduled and gone through with; it is rather to be an informal chat on some physiological problem of more general interest, on modern points of view, on physiological questions of the day, on important news in physiological literature, on various interpretations, opinions, issues in physiological problems, on the more interesting and more important parts of the general lectures and courses, in short, on anything that may claim attention physiologically. The sole object is to arouse interest in physiological problems and to introduce men to physiological ways of thinking.

In this unconstrained form, lacking all didactic varnish, a number of physiological visualizations, teachings, and facts will unconsciously be absorbed and assimilated, with which the formal lectures would probably not have familiarized, and all this without the impression of having been at work. An incentive is perhaps the best that research work and teaching in the science can offer. Truths are only relative and change with time. But interest begets development and development is progress.

If I were asked to express the secret of effective physiological laboratory work in one word I should say—*punctiliousness.*

In a didactic article like the preceding it is necessary that the opinions and experiences of a great many teachers and authorities on the subject should be cited and as it disrupts the continuity of the text to quote the author every time a reference is made to him and also because such authors are not always the exclusive owners of the idea they give expression to, nor even the originators of it, I did not quote their names in each and every case, but simply stated in the introductory chapter my main sources of information. In this connection, I must cite Wilhelm Ostwald in his *"Vorlesungen über Naturphilosophie"* Leipzig, 1902, page 4:

> "Concerning the sources, viewpoints and thoughts which I have presented, I cannot say whether I have read them or have found them independently myself, for I have frequently been able to ascertain that the thoughts which apparently arising independently in the intellect were only remembrances of what had been heard or read previously. I can only say that I have thought all these thoughts myself and I must leave it to the sifting effect of time to determine what is my own mental property in one or the other of these ideas, and I consider myself excused if I do not everywhere mention the names of those who were first authors of an idea, because I do not know them."

Therefore, in this article, I have not been able to mention all the authors but only cited those that inspired me most in the beginning paragraph on page 352. The same idea I found was frequently expressed by several authors.

XX

MICHAEL SERVETUS

DISCOVERER OF THE PULMONARY CIRCULATION. HIS LIFE AND WORK[1]

ESTIMATED from the standpoint of our prevailing conceptions of evolution and heredity, the human being is to be regarded as the product of his inheritance and environment. In proportion to the physical perfection of the environment, the richer its elements of geographical charm, beauty and healthfulness of climate, the more varied its range of accessible food products, in the same degree its human inhabitants should develop into a nobler type. As the high priest in Mozart's "Zauberflöte" idealizes the thought in song:

In diesen heiligen Hallen
Kennt man die Rache nicht
Und ist ein Mensch gefallen
Ruft Liebe Ihn zur Pflicht.

The human being may be assumed to develop a purer and more admirable form of physical and psychical excellence in accordance with the natural conditions that surround him. Yet the physical universe, even when revealed in its most lovely aspects, does not always tend to the ennobling and elevating of those who are constantly exposed to its influence. Despite its grace of outline, the rapture that abides among its lonely hills, its misty mountain tops, the dawning sun that furrows all the orient into gold, "while every prospect pleases," man retains his original vileness:

"Die Welt ist vollkommen überall"
"Wo der Mensch nicht hinkommt mit seiner Qual."
Schiller: *Braut von Messina.*

Among the masterworks of the Divine Artist, the Lake of Geneva assumes a foremost place. From every point of view, geographical, historical, literary, its shores are rich in complex memories and associations, tracing back for more than twenty centuries to the time described by Julius Cæsar when he met the Helvetian host at the bridge over the Rhone, and broadening through the slowly forming ages until we reach the periods forever linked with the names of Calvin, Knox, Servetus, Voltaire, Rousseau, Gibbon, Madame de Stael, Byron, Ruskin. Mt. Blanc looks down from the region of Savoy and its evening glow lights up the Lake with a brilliance that neither art nor language has been able to portray. Yet it was in Geneva that there was enacted one of the most deplorable tragedies that have darkened the annals of the modern world—the death of Michael Servetus at the stake on October 27th, 1553.

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[1] *Johns Hopkins Hospital Bulletin,* September, 1915, and *Janus,* 1915.

Apart from the sphere of the scientist and the student of theological development, the name of Michael Servetus is almost unknown to our contemporary world, and when introduced, save in the esoteric circle of the medical investigator, it is suggestive only as an echo of a vanished age. Yet no name in the long record of scientific evolution is more worthy to be rescued from the oblivion which has enveloped it and to be brought into the clear light of our modern day. Servetus may be regarded as marking one of those distinctive processes or advances by whose agency or activity science in all her phases "slowly broadens down from precedent to precedent." The range of his acquirements seems to have embraced every form of learning in his age, accessible or available. In a measure at least, he had realized the ideal of Francis Bacon and taken all human knowledge as his province. In the field of medicine, he revealed a creative power which heralded the coming of the modern era; his skill in polemic theology was not surpassed,—subtle, acute, penetrating; with these rare qualities of intellect, he blended the charm of a noble and generous spirit. Yet with all these graces of heart and intellect, he became the victim of the prevailing bigotry and intolerance which characterized every form of religious thought, Catholic or Protestant, and through the resistless power of Calvin, the political, as well as the theological autocrat of Geneva, he suffered death at the stake, October 27th, 1553.

Now that the Reformed Church of France and Switzerland have erected at Champel, the scene of his martyrdom, an expiatory monument to the memory of Servetus (November 1st, 1903) and the people of Vienne, France, have perpetuated in marble and bronze their reverence for his genius and their commiseration for the tragedy of his death, the obligation becomes more imperative than ever to reveal the truth of his history to dispel the errors and fabulous assertions with which passion and intolerance has obscured his fame, and to exhibit in abiding clearness the record of the scientific discoverer, the harbinger of the modern world in the field of medical exploration in relation to the circulation of the blood. To accomplish this purpose, we shall freely avail ourselves of the invaluable record of the Abbé d'Artigny, Canon of the Cathedral of St. Michael's at Vienne, who had also at his disposal during the time that he was engaged in the preparation of his Memoirs, the archives of the archbishopric of Vienne, then preserved in complete condition. His statements, derived from so eminent a source, will hardly be called in question by the most sceptical inquirer, or the most hostile student of the life of Servetus.

Authentic accounts represent Servetus as having been born at Tudelle in Navarre in 1511. The date is inferred from the reply which he made to his judges at Vienne in April, 1553, declaring himself as that time to be forty-two years of age. The most accurate and trustworthy biographers represent him as having been born at Villa Nueva in Aragon, as at a later period he assumed, in accordance with the usage of former ages, the name of his birth-place, calling himself Michael de Villeneuve. When at a subsequent time he was reproached by Calvin for concealing his name, he justified himself upon the ground that he had assumed the name of his native city. Yet when arraigned in Vienne, he declares to the judges that Tudelle is his native city. The discrepancy, however, is more apparent than real and can be explained upon the natural supposition that the ancestors of Servetus came originally from Villa Nueva and afterwards acquired a residence in Tudelle. During the age of Servetus and for centuries preceding, patronymics and local designations were used without the definite and precise significance, characteristic of a later time, being modified or varied as changes of place or condition might render desirable or expedient. As the surname did not affect the issue of the trial, some such cause as we have suggested will present a plausible reason for the change. His real name, Servetus, was one that he deemed it prudent to conceal, from motives of a nature such as readers of "The Antiquary" will readily recall. During the entire judicial procedure he is addressed as Michael de Villeneuve, the title he had assumed in France. Even within a comparatively recent period Coleridge and Poe, upon entering the military service, disguised their identity by the assumption of pseudonyms.

From early childhood, Servetus displayed a rare intelligence and a marked fondness for the attainment of knowledge. So devoted was his application to study that at the age of fourteen he had acquired the elements of Latin, Greek and Hebrew, also considerable acquaintance with mathematics and the crude mechanical philosophy of the time, as well as the scholastic theology then recognized in the universities of Europe as the embodiment and the vital essence of religious truth. The study of Holy Scripture and his mode of interpreting its teachings in reference to the Trinity was the specific cause that aroused the antagonism of Calvin

Servetus

and "marshalled him the way" to the stake at Champel. According to his biographers, the father of Servetus, who was by profession a notary, sent him to Toulouse in order to devote himself to the study of law. The reputation of the ancient city for sanctity had gone out into all the medieval world and the fame of its holiness was blazoned by the inscription placed over the vault which was reputed to contain the bones of the twelve apostles, "Non est in toto sanctior orbe locus." More than two centuries later than the time of Servetus this hallowed center of apostolic tradition became the scene of the Calas tragedy (1762), more atrocious in spirit and more ghastly in detail, if such be conceivable, than that enacted at Geneva in October, 1553. If the testimony of Servetus himself is to be accepted as trustworthy, he did not imbibe his heretical opinions in regard to the doctrine of the Trinity during his student days at Toulouse. His inoculation with this fatal heresy may be assigned with probability to a later period and its origin traced to the influence of his sojourn in Italy, which brought him into contact with the prevailing theological tendencies of the country, notably at variance as they were with the orthodox or accepted interpretation of the mystery of the Trinity.

According to the declaration of Servetus, he entered the service of the Holy Roman Emperor, Charles V of Germany (also King of Spain under title of Charles I), at the age of fifteen years, a circumstance which enables us to fix the year definitely as 1526, he having been born in 1511. He seems to have been associated in some capacity with the Confessor of the Emperor, and was present when Charles was crowned at Bologna instead of Rome in 1530, assuming the title of King of Italy, as well as that of Emperor. That Servetus had been in Italy was known to his biographers, as he refers to it in the preface to the first edition of his "Ptolemy," but the contemporary political events with which he was associated by virtue of his relation to Charles V make it possible to fix the duration of his residence with at least approximate accuracy. Trinitarian heresies, among the earliest to reveal themselves in the development of theological dogma, were at this time rife in cultured circles of Italy, now prostrate under the power of Charles V. The intellectual vigor of Servetus, as well as his critical acquaintance with the scholastic philosophy and the method of disputation prevailing in the universities, assured him a conspicuous place in the polemic combats of the time, when the dawning reformation and its novel teachings were the absorbing and almost exclusive theme. Luther had confronted Charles V at Worms only a few years preceding the coming of Servetus into Italy, and the thought of the world was concentrated with eager and ever broadening interest upon the attitude and the innovations of the Monk of Wittenberg. In accordance with the law of evolution in the sphere of theology, the accepted doctrine of the Trinity was selected as one of the vital points to be assailed and Servetus was chosen by general consent as the protagonist who was to deliver the first blow. Although a youth of eighteen, he had devoted himself to the preparation of his treatise, "De Trinitatis Erroribus," his relentless antagonist, Calvin, being at that time only twenty. The Confessor of Charles V being called to Germany, Servetus accompanied him, it being understood, however, that he was to maintain an intimate relation with his Italian associates through the medium of correspondence.

The following year (1531) De Quintain, the Confessor, died, and Servetus found himself without guide or monitor in the perilous realm of theological controversy. His great native force and energy of will revealed themselves in his determination to establish himself as a reformer—another aspect, it may have been, of that same creative and originative spirit which assured him an abiding place among the harbingers and heralds of scientific discovery in the still broadening field of medical research. At Basle he held conference with Occolampadius, and at Strassburg he became involved in controversy with Capiton and Bucer, the latter one of the foremost lights of the expanding Protestant Reformation, the Trinity and the doctrine of Consubstantiation being the special themes involved in the discussion. These two doctrines Servetus combated with the utmost tenacity, as well as acrimony, his violence so offending his adversaries that Bucer, though inclined to moderation and self-restraint, assailed him with the greatest vehemence, even going to such an extreme as to declare that he "deserved to be cut into pieces and to have his entrails torn out" (Volume II, p. 56–60).

The first work of Servetus, entitled "De Trinitatis Erroribus" Libri Septem per "Michaelum Servetum" was published in 1531. He was at that time only twenty years of age. Newton was three years older when he first promulgated his theory of gravitation. Bryant, Keats, Rossetti, Tennyson produced the early poems upon which their fame in a measure still abides between eighteen and twenty. In his first work Servetus assails the doctrine of the Trinity, characterizing the union of the Three Divine Persons as a mere fantasy, a chimera, gods devised by the metaphysicians. The orthodox teaching was cast aside as irrational and impossible, having no foundation save in the ignorance of theologians. During the succeeding year (1532) he issued at Hagenau a second treatise relating to this subject, entitled "Dialogorum de Trinitate, Libri Duo," but being devoid of financial resources and having been expelled from the communion of the foremost reformed churches in Germany, he established himself in France. One of his special aims was to pursue the study of mathematics, but above all to devote himself to the science of medicine. With this end in view, he placed himself under the instruction of Sylvius and Fernel in the schools of Paris, attaining the degres of Bachelor of Arts and Doctor of Medicine. For a time he occupied the Chair of Mathematics at the College of the Lombards, and in 1533 he was engaged in an active controversy with the Parisian doctors. The case was carried into the Parliament of Paris, a body whose functions were judicial, not legislative as in the English acceptation of the term, the result being a complicated and prolonged legal process which disenchanted Servetus with the French metropolis, and induced a change of residence.

We find Servetus first at Lyons in the capacity of proofreader for the publishing house of Frellon Brothers, Merciere Street, trading under the sign of *Escu de Cologne*. We find him later at Avignon. Thence he returned to Lyons, and finally established himself at Charlieu, where he practiced his profession for three years, and at the end of that time he returned to Lyons, where, meeting Pierre Palmier, with whom he had come into contact during his Parisian days, he is urged by him to make his home in Vienne, and, acquiescing in the proposal, he takes up his residence in this historical city, at no great distance from the Archiepiscopal palace.

The frequent changes of place which mark the life of Servetus may be attributed in a measure to the migratory tendencies of the medieval student and scholar. They were features of the intellectual life of the age. Traces of this vagrant character may be discerned in the drama of the Elizabethan era, see "Hamlet," II, 2, "As the indifferent children of the earth"; also, the "terrae Filii" of Johnson's "Alchemist." In Vienne he might have passed his life in tranquillity, absorbed in the study of his profession, together with the congenial pursuit of literature as embodied in the fadeless types transmitted from the antique world, or in the novel forms, then blooming into vigorous and artistic expression under the auspicious guidance of the spirit of the Renaissance. He might have become a Sir Thomas Browne in the sixteenth century, or have been one of that rare and elect circle which mingled with Montaigne and read in their early freshness the Essays which touched the thought of all Europe, fashioning the phraseology of Bacon, and contributing in at least one notable instance to the marvellous range and affluence of Shakespearean characterization. Yet no such propitious fate was in reserve for the young physician. The demon of theological controversy had entered in and assumed an inalienable possession. He had assailed the vital stronghold of the orthodox creed, and sooner or later the one issue was inevitable. Every opportunity that presented itself for the assertion of his heretical teachings was availed of with eagerness. He made frequent visits to Lyons and in 1543, while in that city, he assumed charge of a folio edition of the Bible published by Hughes de la Poite, the preface to which, written by himself, contained an exposition of his views, with marginal notes which were regarded by Calvin as not merely irreverent, but as actually blasphemous in character. Servetus again assumed the local name, Villanovanus, and Calvin mentions that he received 500 livres from the publishers for the work (Abbé d'Artigny, "Memoirs," pp. 65–663). Calvin, who had known Servetus during his life in Paris, where they were both students, corresponded with him under the pseudonym of Charles Desparville, through the agency of the publisher, John Frellon. Servetus, who was not unknown in Vienne by his designation of "Villanovanus," incurred not only the resentment and disapproval, but the implacable hostility of the Swiss reformer, whose creed he had assailed at a vital point, in language almost unqualified and untempered in its nature. The correspondence came to a close in 1548, and Servetus addressed himself to the preparation of the work which assured his tragical fate at Champel in October, 1553.

To this book, known as "Christianismi Restitutio," he devoted four years and it appeared on January 3rd, 1553, with no indication or suggestion of either author or publisher, the book having been declined by a Basle publisher (Marrinus, by name). Servetus entrusted it to Balthazar Amollet of Vienne and to Guillaume Guerault, his brother-in-law, manager of the firm. In this epoch-making work, Servetus presents and defends, his ideal religion, tracing back to the traditions of the primitive Christian age, in whose life the historic imagination reveals a unity and harmony which was in large measure the creation of its own genius. Though lacking in definiteness of language and clearness of exposition, it may be regarded as the first well defined attempt, at least in modern times, to enunciate the principles of the pantheistic philosophy, or the doctrine of a supreme being or intelligence pervading all nature, yet impersonal in essence and in character. Emil Saisset justly assigns Servetus a place among the foremost precursors of such later oracles of the Pantheistic creed as Spinoza, Schleiermacher, and Strauss. In this notable work there is revealed, despite its occasional vagueness of language and its fantasies, a conception of historic method upon which all the scientific achievement of a later age rests as its basis, though it was hardly even dimly described by the explorers and researchers of the sixteenth century. From his day of dawn, it may be clearly assumed that he had been devotedly,

> Nourishing a youth sublime,
> With the fairy tales of silence,
> And the long results of time.

The fifth book of "Christianismi Restitutio" contains a passage in reference to the circulation of the blood, in which he announces a discovery that must assure his place in the foremost ranks of scientific investigators. "La Vie Medicale," August–September, 1905, has the following comment upon this incident, forever memorable in the annals of medicine: "The first savant who saw the defects of Galen's theory was the Spaniard, Michael Servetus, who in his 'Christianismi Restitutio' denied the permeability of the septum and affirmed that the blood of the right ventricle went to the lungs where, after becoming red, it passes from the pulmonary artery into the vein of the same name." In this way, if the demonstration of the greater circulation can be justly assigned to the English physiologist, William Harvey, (1615 being the date of its first announcement, and 1628 the time of its matured development), the discovery of the minor circulation must assuredly be attributed to Michael Servetus. (Yet so late as 1669, the discovery of Servetus was ascribed to Lower. See R. Tigerstedt: Zur Geschichte des kleinen Kreislaufs, in "Ergebnisse d. Physiol, Jahrg. II, p. 533," who quotes Cheneaux and M. Roth as proving that Matheus Realdus Columbus discovered the lesser circulation and that Servetus never pursued the study of anatomy except for a short time at Paris under the guidance of Günther.)

PERSONALITY OF SERVETUS AND HOW HE CAME TO MAKE A GREAT PHYSIOLOGICAL DISCOVERY

It is always desirable to gain an insight into the personality of great men. We should like to know their psychography, not only their biography. Such a study of the personality of Servetus is fraught with great difficulties because the men who personally knew him and wrote about him belong to either one of two extreme parties—one his warm friends and admirers, among whom must be classed all the great enthusiasts for religious reform; the other made up of his enemies, of which there were two kinds: religious enemies and enemies in the scientific world of the day, interested in anatomy and physiology. That he had scientific enemies is proven by the fact that they endeavored to rob him of the priority of his discoveries. It is more than likely that the truth concerning his personality can be obtained mostly from those who were friendly inclined and that some of his characteristics as given by his enemies may be useful in throwing valuable sidelights upon the specific psychic quality of the man. Then there is a third group of men, those like P. Flourens and Ceradini, so-called historic critics, who, though trying to be objective and neutral, yet have committed errors prompted in part by ill-guided patriotism or local jealousies.

In considering the question: How did Servetus come to make a great discovery in physiology, Flourens uses the words "Such an absurd book," concerning Servetus' "Restitutio Christianismi." (See Flourens' "Histoire de la Circulation du Sang," 1857.) He also says of Servetus, p. 153, that he was a "confused head" and that he only showed genius in those few illumined lines in which he describes his discovery, and Flourens attributes this discovery to the one-sided theologic obsession to accept only the literal sense of the Bible, ". . . on s'aperçoit bien vite du parti qu'il a pris en Théologie, de s'attacher uniquement et obstinément au sens littral" (S. 155 fg.). The following is the train of ideas that Flourens imagines to have been dominant in the brain of Servetus: According to the letter of the Bible, the soul is in the blood and as the soul itself is blood, in order to know how the soul is formed, one must first know how the blood is formed; in order to know how the blood is formed, one must know how it moves, and thus Servetus incidentally, in considering the restitution of Christianity, is led to the formation of the soul and from the formation of the soul to the formation of the blood, and from the formation of the blood to the circulation through the lungs. (Flourens, p. 156.) As Tolin correctly remarks, this supposition of Flourens is nothing but a mere hypothesis and comes to a wrong conclusion because it consists of nothing but false premises.

Servetus is a man of homogeneous mental structure and of amazing versatility. He belongs to the greatest intellectual geniuses of his age. One of his opponents concedes this (see S. Stähelin on John Calvin, I. p. 428). Then it is wrong to say that Servetus only shows genius in those few lines conveying his discovery. Servetus shows genius in whatever he undertakes. At the age of nineteen he wrote a book which excited the thinkers of all Europe and brought the entire Christian world into movement, but nobody, Catholic, Lutheran, or Reformist ventured to correct this book on the errors of the Trinity. When Servetus became a corrector in the bookprinting establishment of Trechsel, the establishment had to move with him wherever he went. When he became a geographer, he became the founder of an entirely new method, that of comparative geography. When he became a pupil of a physician, Champier, in Lyon,—a medical celebrity of his day—it only required one year before "Jack beat his master" and Dr. Symphorien Champier considered it an honor to himself to edit the "Apologia, etc." When Servetus became a mathematician, Palmier, who was the Primate of France, enjoyed his teaching.

Such a great discovery as the lesser circulation could not have been the result of such irrelevant and impulsive intuition as Flourens describes, but it must have been the outcome of a very eminent intellectual gift. Servetus discovered the pulmonary circulation because he cannot enter into any subject without making discoveries. He has had no predecessor on this specific subject and no successor until the history of physiology arrives at Harvey. The "Christianismi Restitutio," according to Osler, was never published (Johns Hopkins Hospital Bulletin, Jan. 1910, p. 9), but on page 7 (ibid.) Osler speaks of the "printing" of this very book. This is confusing, as there were many manuscript copies presented by Servetus personally to his friends, and thus the impression might be gained that it was never printed but only existed in script. But there were complete copies in print exhibited at the trial of Servetus on February 26, 1553. Therefore, it appears that the book was printed in its entirety and more copies were saved after the death of Servetus than is generally known to history, because it was considered a heresy even to be the possessor of this book.

Between Servetus and Harvey there are only two real discoverers concerning the circulation. One is Cesalpinus, who knew the greater circulation but, in reality, did not claim it as his discovery, and the other is Fabricius ab Aquapendente who described the valves and the veins in 1574 and draws attention to the fact that these valves were directed toward the heart, and that the blood, therefore, goes from the veins to the heart. The valves of the veins are, therefore, the anatomical proof of the circulation.

To return once more to the book of Flourens, which is really one of the most valuable contributions to the history of physiology, it is evident that as far as the personality of Servetus is concerned, we cannot depend upon such characterization as he gives, nor credit the mental train of thought depicted by him as having eventuated in the discovery of the lesser circulation.

When Servetus was in Paris, where he had been a student under Sylvius and Guenther who were enthusiastic adherents to the anatomic views of Galen, he found there, as a fellow-worker, Andreas Vesalius (his German name was Wesel). Vesalius had pointed out to his teacher Sylvius that Galen had committed an error concerning the aortic valves and it is not surprising that Servetus should have heard Vesalius discuss this matter with Sylvius, thereby gaining his first incentive to the great discovery physiology owes to him.

The physiological discovery of Servetus remained unrecognized until 1697, when Charles Bernard, a St. Bartholomew's Hospital surgeon, called the attention of Wooton to it (William Wooton, "Reflections upon ancient and modern learning," 1697, p. 229).

What theologic authorities think of "De Trinitatis Erroribus" and other religious doctrines of Servetus, can be best illustrated by introducing the opinion of Emerton, a brilliant scholar in theological history. Quoted from the *Harvard Theological Review,* April, 1909:

"He would not admit that the eternal Son of God was to appear as man, but only that a man was to come who should be the Son of God. This is the earliest intimation we have as to the speculations which were occupying the mind of the young scholar. It is a highly significant historical view of theology. As he read the Old Testament, its writers seemed to him to be referring to things that their hearers would understand. Their gaze into the future was limited by the fortunes of the people at the moment. To imagine them possessed of all the divine mysteries, and to have in mind the person of the man Jesus as the ultimate object of all their prophetic vision, was to reflect back the knowledge of history into a past to which such knowledge was impossible. So far as I can understand him, this is the key to all of Servetus' later thought. His manner of expressing himself is confusing and intricate to the last degree, so much so that no one in his own time nor since has dared to say that he understood it. To his contemporaries he was a half-mad fanatic; to those who have studied him even sympathetically, his thought remains to a great extent enigmatical; but this one point is fairly clear: that he grasped, as no one up to his time had grasped, this one central notion, that, whatever the divine plan may have been, it must be revealed by the long slow movement of history—that to understand the record of the past it must be read, so far as that is possible, with the mind of those to whom it was immediately addressed, and must not be twisted into the meanings that may suit the fancy of later generations.

"To have seized upon such an idea as this—an idea which has begun to come to its rights only within our memories—was an achievement which marks this youth of twenty as, at all events, an extraordinary individual, a disturbing element in his world, a man who was not likely to let the authorities rest calmly in possession of all the truth there was."

An opinion by the writer on the ideas and doctrines Servetus intended to convey in his religious work "Christianismi Restitutio" could have only a limited and personal value. I will quote therefore again from Professor Emerton, the same capable theological authority, whom Osler quotes in his article on Michael Servetus:

"He finds the central fact of Christian speculation, not in the doctrine of the Trinity as formulated by the schools, but in the fact of the divine incarnation in the person of Jesus. He admits the divine birth, explaining it as in harmony with a general law of divine manifestation whereby the spiritual is revealed in the material. He would not accept the idea of an eternal Sonship, except in this sense, that the divine Word, the Logos, had always been active as the expression in outward form of the divine activity. So, in the fullness of time, this same Logos produced a being from a human mother upon whom, at the moment of his birth, the divine spirit was breathed. Obviously this is not the 'eternal Son' of the creeds and herein lay the special theological crime of Servetus. In his criticism of the church order, of the papal government, of the sacramental system, he does not differ essentially from the more radical of the reformers. On the essential matters of Baptism and the Eucharist he goes quite beyond the established reforming churches. In both cases he invokes the principle of plain reason. He rejects infant Baptism on the ground that the infant can have no faith and that the practice is therefore mere incantation. He denies trans-substantiation on the rational basis that substances and accidents may not be separated and does not spare the reforming leaders for what seemed to him their half-hearted attitude on this point. His language throughout is harsh and violent except where, as at the close of his chapters, he passes over into the forms of devotion and closes his diatribes with prayers of great beauty and spirituality."

At this point we introduce an English translation of that portion of the "Christianismi Restitutio" in which the views of Servetus in regard to the circulation are explicitly set forth:

"In order properly to understand this condition of things it becomes necessary to know beforehand the substantial generation of the life spirit itself, which is composed of the inspired air and finest blood, nourished by the same (Genes. 2, 7). This life spirit has its origin in the left chamber of the heart, the lungs especially assisting in its generation. It is a delicate spirit breath, produced by the force of warmth, of clear (flavo) color, burning force and to a certain extent composed of a transparent foam, formed out of pure blood and containing in its substance water, air, and fire. It is generated by the admixture of the inspired air and the thinned blood furnished by the right chamber of the heart to the left. This, however, does not take place through the middle wall of the heart, as has been hitherto supposed[2] but by a highly intricate mechanism the finely divided blood is conveyed by the right chamber of the heart by a devious route through the lungs.[3] The lungs prepare it for use by clarifying it and pass it from the arterial vein into the venous artery.[4] It is thus mixed with the inspired air in the venous artery itself and by expiration is again cleansed from soot (a fulgine). Finally the whole mixture is drawn through diastole, serving (if I may be permitted to use the expression), as a suitable household utensil for the life-spirit.

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[2] ut vulgo creditus.

[3] The honor of this discovery of 1553 was credited to Lower in 1669.

[4] Namely, in its origin in the lungs, as will be seen later. Furthermore, that the term Arteria venosa means the veins of the lung, and Vena arteriosa refers to the arteries of the same, needs no further explanation.

"That this preparation and assimilation take place in the lungs is manifest by the extensive union and anastomosis of the arterial vein with the venous artery of the lung. This is confirmed by the striking size of the arterial vein, which could not have reached such dimensions and have sent the blood from the heart into the lungs with such force by its own nutritive power, nor could the heart serve the lungs in a like manner, especially since in the embryo, lungs receive their nourishment from other sources. In that those membranes, or valves of the heart (an membranulos illos seu valvulas cordis) are not opened until the time of birth, as Galen has taught. For this reason the blood is poured out so profusely from the heart at that time to serve another purpose.

"The lungs also send to the heart not only mere air, but such as is mixed with blood through the venous artery. The admixture therefore takes place in the lungs, not in the heart. There is not room enough in the right side of the heart to accomplish such great and thorough admixture as to render the blood clear red. Finally, the middle wall, which dispenses with vessels and other adjuncts, is not adapted for transmission and distribution, although a portion may transpire.[5] In the same complicated manner as occurs in the liver, where transmission takes place from the portal vein to the vena cava (a vena porta ad venam cavam) on the part of the blood, so also happens in the lungs the transfer from the arterial vein to the venous artery on the part of the life spirit.[6] If one compares this with what Galen describes in Books 6 and 7 on the function of these parts, he will grasp the truths therefore, although Galen himself did not understand this (ab ipso Galeno non animadvertum).

"This life spirit is then carried from the left side of the heart by degrees into the arteries of the entire body, which, as they become smaller and narrower and extend upward, it becomes more diffused, especially in the plexiform tissues at the base of the brain (in plexu retiformi) where it is transformed from mere life spirit into soul spirit, in so much as it here approaches the seat proper of the reasoning soul.

"And now it is still more refined, elaborated, and perfected by the finer vessels or capillary arteries (capillaribus arteriis) which are found in the arterial plexuses (in plexibus choroidibus) and which contain the intellect

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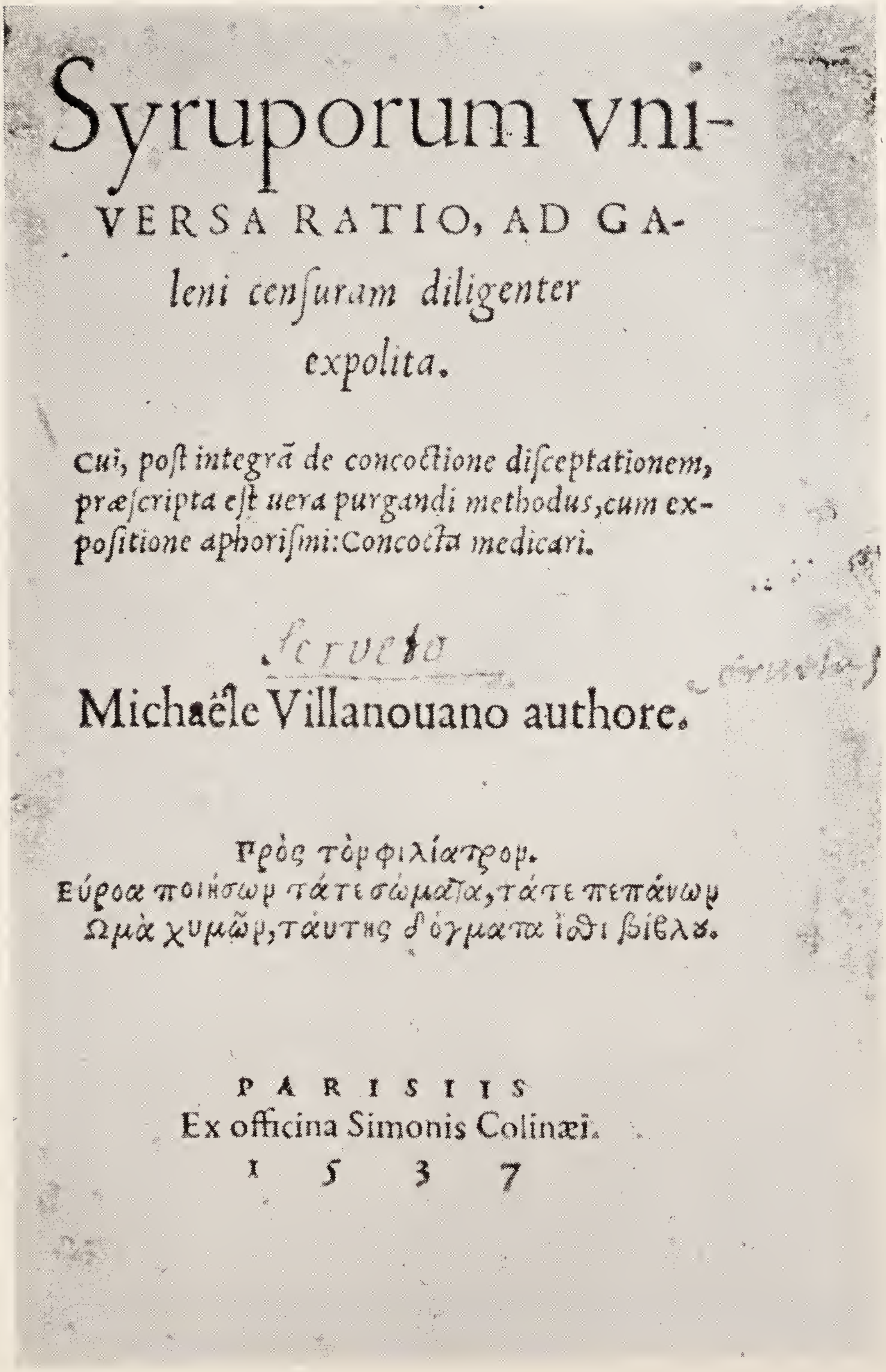
[5] "Licet aliquid resudare possit": A courtesy to Galen.

[6] "Spirit" and "Life Spirit" would here correspond to oxygen and oxygenated blood.



Syruporum vniuersa ratio, ad Galeni cenſuram diligenter expolita.

Cui, poſt integrā de concoctione diſceptationem, præſcripta eſt uera purgandi methodus, cum expoſitione aphoriſmi: Concocta medicari.

Serveto

Michaële Villanouano authore.

Πρὸς τὸν φιλίατρον.
Εὔροα ποιήσων τά τε σώματα, τά τε πεπάνων
Ὠμὰ χυμῶν, ταύτης δόγματα ἴσθι βίβλου.

PARISIIS
Ex officina Simonis Colinæi.
1537

Servetus on Syrups

itself (ipsissimam mentem). These plexuses penetrate the innermost portions of the brain and form a lining to the interior cerebral chambers, entwining and enclosing those vessels up to their origin in the nerves so that they are rendered capable to the sense of touch and motion. These vessels, by wonderful arrangement so finely constructed, are in reality end arteries, and follow the course of origin of the nerves through the coverings of the brain (ministerio meningiis). This is a new form of vessels.

"As the merging of the veins and arteries in the lungs forms a new set of vessels composed of vein and artery, so also the union of the artery and nerve[7] forms a new sort of vessel from the arterial membrane to the cerebal lining, so much the more in that the covering of the brain retains its structure even into the nerves (tunicas)."

The entire first edition of this historic work was brought to Lyons. A part of it was placed on sale in the store of Pierre Merien, type-founder near Notre Dame de Comfort. Jean Frellon assumed charge of the rest. In virtue of the relations once existing between the author and Calvin, Frellon committed the fatal indiscretion of sending a copy of the newly issued treatise to the former. The contempt exhibited for Calvin and his writings, as well as the theological attitude revealed, aroused the implacable resentment of Calvin, and from that day the tragedy of Champel was a foregone result. The storm had been long gathering, for in February, 1546, Calvin had written to Farel, his co-reformer and devoted ally, that "if his authority could still accomplish anything, he would never suffer him to leave there (Geneva) alive" (*"vivum exire nunquam patior"*). See Torcheel a a V 119 also H. Tollin Saint Vertunien Delavau Virchou's Archiv Bd CI Heft I S 44–70.

At this time Calvin had with him in Geneva a convert to his faith whose name was Guillaume Trie, and who was a native of Lyons. Trie maintained a systematic correspondence with Antoine Arnays, a relative of his, then living in Lyons, a loyal Catholic who constantly and urgently appealed to Trie to return to his original belief. In February of this eventful year (1553) Calvin requested Trie to write in the former's behalf to the latter's kinsman a letter in which Servetus was described as one of the most dangerous of heretics, whether contemplated from the Catholic or Protestant point of view. The following copious extract will illustrate the character of the correspondence, and every utterance reveals the agency of the Swiss reformer:

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[7] If the terms "Nerves" and "Nervis" were here replaced by "venas" and "venis," it would afford a more intelligent understanding of the subject. Anastomosis of the arteries and veins was spoken of above, and even now the extremely fine vessels are termed "capillary-arteries."

"With regard to doctrine and religion" he writes, "although we enjoy such as are greater than yours, nevertheless it shall not be permitted that the name of God be blasphemed, and that doctrines and bad principles be disseminated without being suppressed, and I can cite a flagrant example against you since it becomes necessary: namely, that they support among you a heretic who merits to be burned wherever he may go. When I say a heretic, I mean a man who will be condemned by the Catholics, as as well as by us, or at least he ought to be. For though we are different in many things, we have this in common, that in one essence of God alone there are three persons, and that the Father has begotten his Son, who is eternal wisdom, before the beginning of time, and that he has had his eternal virtue, which is his Holy Ghost. Now, when a man says that the Trinity in which we believe is a Cerberus and monster of Hell, and vomits forth every wickedness imaginable against every teaching of the Bible with regard to the birth of the Son of God, and derides vilely all that the Divines have said concerning it, pray tell me what respect would you have for him? . . . There is one, however, who will call Jesus Christ an idol, who will destroy all the foundations of faith, who will collect all the drams of the ancient heretics, who will even condemn the baptism of infants, describing those as diabolical inventions; and this man is held in favor by you and is approved as if he had been guilty of no wrong! Where is the zeal in which you exult? and where is the order of that beautiful hierarchy which you extol in such emphatic terms? The man that I am describing to you has been condemned by all the churches with which you are in accord. Yet you have even conceded him the privilege of publishing his books, so charged with blasphemies that I need make no further reference to them. The man I have portrayed is a Spaniard, by name Michael Servetus. At this time, however, he styles himself Villaneuve. His profession is that of a physician. For some time he made his home in Lyons; he now resides in Vienne, where the book I have in mind was printed by an obscure creature named Balthazar Arnollet, who established a foundry in that city. That I may not seem to speak without evidence and with positive knowledge, I send you the first page of the work as a specific proof. You profess to believe that books which inculcate any teaching save the pure simplicity of the Holy Scriptures are contaminating, and if they are in conflict with the divine word, they ought not to be tolerated among you. Yet, notwithstanding, you are nourishing in your own city a poison whose aim is the effacing of the Holy Scriptures and of every teaching you have received from Christ."

We have unfolded in this letter the first well defined phase in the drama which attained its final stage in the autumn of that year. The purpose of Calvin is foreshadowed with relentless clearness. The vision of the stake at Champel rises before the mental eye. . . .

The Cardinal of Tournon, combining civil and ecclesiastical functions as Governor and Archbishop, had in his service at this time an inquisitor whom he had summoned from Rome, the monk Mathiew D'Ory (whose name is never recalled without a sensation of horror), who assumed the title of Penitent of St. Siege, Inquisitor in France and in Gaul. As Calvin had anticipated, the letter of Guillaume Trie and the pages from the work of Servetus were placed in his hands. With the assistance of Benoit Buatier, canon in the Church of Vienne, Archdeacon de la Tour, Camarier of St. Paul in Lyons, Vicar General of Monseigneur of Tournon, and his archbishopric in Lyons, they were rigidly scrutinized, and it was decided to report the result to the Cardinal, who at the time was residing in a château at Roussillon. Buatier left Lyons March 13, 1553, and called upon him in order to advise him of the result of the investigation. Louis Arzellier, supreme vicar of the Archbishopric of Vienne, was summoned to the château, and after a prolonged conference, the two grand vicars were dispatched to Vienne with a letter containing the instructions of the Cardinal to Mr. Maugiroun, who, after being informed explicitly in regard to the intentions of the Cardinal, sent for Antoine de la Court, Sheriff of the city, with a view to instituting legal proceedings against Servetus, who, in official papers connected with the trial, is always described as Michael de Villaneuve. On March 16th his home was searched, but there was nothing brought to light that implicated him, or created a suspicion of heretical teaching. The judges, impressed by the depositions made on the preceding evening before the official primate, Peyrollier, by the grand vicar and vibaille of Vienne, on the 17th sent for Guillaume Guerault, but were unable to elicit any information from him. The printers, upon being questioned separately, replied that among the books which they had printed for two years, there had been none in octavo. When the proceedings were concluded, the judges summoned to appear before them the printers, compositors, and servants of Arnollet, together with their wives and servants. They were enjoined not to disclose anything that had occurred, or to make known any point in reference to which they had been cross-examined, under penalty of being regarded and punished as heretics. On the day following, March 18th, Arnollet having returned from a visit to Toulouse, was subjected to cross-examination, and made denials, the same in character as those of his brother-in-law Guerault. The judges in a session held at the house of the Archbishop decided that sufficient evidence had not been produced to justify or warrant the making of an arrest.

On March 19th the Archbishop summoned Mathiew D'Ory, the Inquisitor, to Vienne. D'Ory advised the judges that, in order to secure sufficient evidence to lead to the conviction of Michael de Villaneuve, his treatise "Christianismi Restitutio" should be examined in its complete form and not in fragments or in passages detached from their context. To render this practicable, it was decided that the Sieur Arnay should communicate with his relative in Geneva and obtain through his agency a copy of the treatise instead of specimens or illustrations which were not sufficient to exhibit its genuine character. This measure being determined upon, the inquisitor returned to Lyons and dictated the letter which Arnay was to transmit to Guillaume Trie. Through his confidential agent, Calvin replied March 26th, sending a greater variety of documentary evidence than was requisite to assure the conviction of Servetus, as, for example, two dozen letters written by him during his controversy with the reformer in preceding years. The proof thus furnished was supplemented by another letter, received on March 31st.

On April 4th the Cardinal of Tournon, the Archbishop of Vienne, Pierre Palmier, the two grand vicars, the inquisitor, several ecclesiastics and doctors of divinity assembled at the Roussillon château. The documents last received from Geneva, that is, the two letters of Guillaume Trie, a copy of "Christianismi Restitutio" with marginal notes in the handwriting of Servetus, and more than twenty letters which he had addressed to Calvin, were subjected to examination. The charge of heresy was established beyond all question. Servetus was convicted upon his own testimony. By unanimous agreement the Cardinal and Archbishop proceeded to arrest Michael de Villaneuve, physician, and Balthazar Arnollet, publisher, and to require of them an account of their faith, as well as an answer to the charges and information laid against them.

When dinner had been finished, the Archbishop returned to Vienne in company with his grand vicar, and communicated to the vice bailiff the purpose of the Cardinal. In order to prevent a discovery of the plan, it was determined that Servetus and Arnollet should be arrested simultaneously and consigned to different prisons. About six o'clock the grand vicar approached the house of Arnollet and directed him to produce the New Testament which he had printed. The publisher appeared and was committed to the prison of the archbishopric. At the very same time the vice bailiff (or deputies), proceeded to the home of Maugiron, whom Servetus was attending in his professional capacity, and, advising him that there were several sick and wounded prisoners in the Royal Palace, requested him to come to their relief. Servetus replied that his duty as a physician, as well as his desire to minister to suffering, induced him to comply. They then proceeded to the Royal Prison, and while Servetus was devoting himself to his patients, the deputy dispatched a messenger to the grand vicar requesting his presence. As soon as he arrived, Servetus was notified that certain charges and information, communicated to the Seigneur Cardinal of Tournon, had been laid against him, and that he was to remain a prisoner in the Palace until the charges were answered, or different instructions were received in reference to his case. He was then committed to Antoine Bouin, provost or jailer, with a special injunction to guard him rigidly, at the same time according him the consideration due to his rank and his professional character. He was allowed to retain his servant, Benoit Perrin, aged fifteen, who had been five years in his employ. ("Memoirs," Abbé D'Artigny, p. 100.)

On the next day, April 5th, the inquisitor, Mathiew D'Ory, came to assume charge of the proceedings involved in the examination of Servetus. The examination continued on the 6th, and soon after Servetus sent Perrin to the monastery of St. Pierre to ask for 300 Ducats which was due him at the Cote St. André, and the Grand Prieur, to whom the request had been presented, produced and paid him the amount. Almost immediately afterward, the inquisitor prohibited Servetus from conversing with anyone save by special permission, and enjoined the severest vigilance upon the jailer to whose charge he had been committed. This rigorous treatment was an involuntary tribute to the generous and philanthropic spirit which was characteristic of Servetus and had won him troops of friends in every sphere of society. He had healed the vice baliff's only daughter of a dangerous malady; he had also restored the son of the governor, Louis de Maugiron, and during the desolating plague of 1543, he had devoted himself to the relief of the stricken. Naturally, the inquisitor feared that he might escape, and the result proved that his apprehensions were not unfounded.

On April 7th Servetus arose at four A. M. and asked the jailer who was directing some work in his vineyard for the use of the garden key. Mr. Bouin did not suspect his purpose, as Servetus very adroitly had robed himself in his night-cap and dressing-gown, which concealed his accustomed clothing, his hat being hidden by the gown. He complied with his request, gave him the key, and proceeded with his superintendence of the workmen. As soon as they had advanced a sufficient distance from him to justify his taking such a bold and daring measure, Servetus left his black velvet bonnet and furred dressing-gown at the base of a tree, leaped from the terrace of the garden to the roof below, and from thence to the yard of the palace. He reached the gate of the bridge which spans the Rhone, a landmark of the Roman occupation of Gaul, extending from

Pichat Square to the tower of Philip VI of Valois opposite, at Saunte Colomb, and hastened in the direction of Givois and Lyons, as was testified by a country girl whom he met on his route. Fortunately for Servetus, she was not examined until three days after she had encountered him. More than two hours passed before his escape was discovered. The wife of the jailer was the first to be informed, and in her despair, she gave way to every form of wild and frenzied action, such as tearing the hair, beating her servants, her children, the prisoners whom she chanced to meet, even incurring the danger of running over the adjoining roofs in her frantic eagerness to recapture the fugitive. She had been suddenly transformed into a maniac.

Every possible effort was made by those in authority to recapture Servetus. The gates of the city were closed and guarded during the night of that day and the day following. After proclamation by the trumpet, a thorough and rigid search was made of all houses, including those in St. Colombe, the magistrates of Lyons and other neighboring cities in which it was possible that Servetus might have taken refuge were at once notified. They even investigated the financial condition of the escaped prisoner by application to the banks of Vienne; an inventory of his papers, furniture, and all his personal effects was placed in the hands of the local authorities (Abbé. D'Artigny, "Memoirs," p. 112). On the second of May, the inquisitor brought to light in a remote and lonely house the two presses which had been used for the printing of "Christianismi Restitutio," and ascertained from Thomas Straton, one of the employees, that he had forwarded on January 13th, by order of Servetus, five packages of books to Pierre Merrin, a type founder, living in Lyons near Notre Dame de Comfort. The five packages were returned to Vienne on the 10th of May. The inquisitor prepared a synopsis of the principal errors contained in the work, with a view to emphasizing the effect of his censure or judgment by commenting upon passages detached or isolated from their context, an effective mode of distorting their meaning and perverting the teaching of the author. By the coming of June, the investigation had been completed and the following verdict was formally rendered, the text having been literally reproduced from the archives of the old Archbishopric in Vienne:

"We, having seen the documents of said heresies, also the letters and writings of said Villaneuve, addressed to Mr. John Calvin, preacher at Geneva, and avowed by the said Villaneuve (the said Villaneuve admitting the fact), his answers, confessions, and denials, the answers, and other procedures, Balthazar the printer agreeing, certain packages and printed books under the title "Christianismi Restitutio," witnesses being examined to ascertain if the said Villaneuve had written and printed the said book at his expense, the reports of D.D.'s and other notable persons concerning the errors contained in the said book and letters, which errors and heresies, are, moreover, made evident when read, documents concerning escapes from prisons, and proceedings to arrest the said Villaneuve; the collected testimony, the final conclusions of the attorney of the King Dauphin, and everything considered, we have declared that the said faults have been duly obtained, in profit of which we have estopped and estop the said Villaneuve from every plea and defense; we have declared and do declare him guilty of and convicted for the offenses and crimes attributed to him, for reparation of which we have condemned him and condemn him to pay a fine of one thousand livres currency to the King Dauphin; and to be, immediately after his arrest, taken on a tumbril with his books the next day from the market of the Royal Gate through the most populous and frequented streets and places as far as the market of this city, and then to the place called Chaneve, and now St. Martin Square, there to be burned alive over a slow fire until his body is burned to ashes. *Nevertheless, this sentence shall be executed upon his effigy, and with it shall burn the said books.*

"And since we have sentenced him and sentence him to pay expenses and costs, of which we reserve the assessment, we declare all his property confiscated for the benefit of the heir, the said costs and damages claimed on this property being first paid. De la Court, bailiff and Royal Judge, Giotel, Cavier, Rutod . . . . assessors.

"The said sentence being delivered in open court before the said attorney of the King, we, the aforesaid, sitting in court in the Royal Palace in Vienne, the 17th of June, 1553, in the presence of Philbert Gollin . . . . and several other practitioners of Vienne witnessing with the undersigned clerk of the Court, Chasalis."

On the same day, June 17th, the sentence was executed in effigy, as is attested by the official report of Francis Barode who carried it into effect, his statement having been prepared by the clerk of the court.

Our narrative now hastens to its climax. Servetus had escaped from the jaws of the Roman lion merely to be committed to the tender mercies of the relentless autocrat who dominated the civil, as well as the ecclesiastical polity of Geneva. He had fled from Scylla only to be cast upon Charybdis. It may be assumed without intolerance of spirit or extravagance of language that the last state was at least not better than the first. He was arrested in Geneva on the 13th of August, while in attendance upon a religious service, and once in the hands of Calvin, the martyr's crown was a foregone result. His apparent purpose in passing through

Geneva was to effect a return into Italy which, it may be supposed, still retained for him the charm that marked his early years (1523–1530) in the service of Charles V. There too, he had absorbed those heterodox conceptions of the Trinity which heralded the way to the autumnal tragedy on the slopes of Champel.

The trial of Servetus, as conducted under the all-potent direction of Calvin, was a far more complex and prolonged judicial process than might be inferred from the ordinary accounts which have been transmitted to later ages. It extended from August 22nd unto October 25th, when sentence of death was pronounced against him. Nor was the autocrat and theocrat of Geneva suffered to bring his victim to the stake without vehement resistance from the party of the Libertines under the leadership of Perrin, which withstood with tenacious energy the intolerant sway and all-pervasive tyranny of Calvin, and embodied in its ranks the finest flowers of Genevan social culture, as well as the purest expression of its chivalric ideals. Encouraged and stimulated by the coöperation of so powerful a party, Servetus, who in the earlier stages of the trial had borne himself with dignity and moderation, assumed a violent and denunciatory attitude with regard to his invincible antagonist. Yet it availed not, and despite all the efforts of Perrin, the sentence of death at the stake, pronounced on October 25th, was carried into effect at eleven A. M. on October the 27th, 1553.

The story of the long drawn agony of Servetus in the flames, the rescue of his book, condemned to perish with him, his refusal to disavow his belief, even in the eye of death, his invocation with his fast fleeting breath of the mercy and compassion of Christ have all been wrought into the historic consciousness of the ages that came after.

He nothing common did, or mean,
Upon that memorable scene,
Nor called the gods with vulgar spite
To vindicate his helpless right.

Yet in no sense can the discoverer of the lesser circulation assume rank among the martyrs to the spirit of scientific discovery. His physiological teaching seems to have formed no part of the combined movement, Catholic and Protestant, that brought him to the stake. The demon of theological speculation had entered his soul and the demon of theological intolerance pursued him with its unique and unresting malignity until there remained no trace of Servetus in the world, save scattered ashes, a name from that time famous, and a mournful memory. In assailing the doctrine of the Trinity inculcated with invincible devotion by the whole Christian communion, he had left himself no sanctuary or asylum; his attitude was one of hopeless irreclaimable isolation. To impugn the orthodox creed in this special phase of belief constituted an offense against both the civil and ecclesiastical codes which involved grave consequences and placed the offender in a category not unlike that assigned to an anarchist or a dynamiter as contemplated from the viewpoint of our modern civilization. Saducees and Pharisees, the society of Jesuits and the theocratic forces marshaled by Calvin made common cause against their common enemy. The Catholics burned Servetus in effigy at Vienne; Calvin burned him in reality at Geneva. One was the preluding rehearsal, the other the ghastly tragedy.

In addition to his heretical attitude with reference to the Trinity, Servetus had been imbued with the teachings of the Anabaptists; he strenuously repudiated the doctrine of infant baptism and was involved in the odium attaching to this widespread sect whose name was almost a symbol for licentiousness and disorder. Against the combined forces of Trinitarian orthodoxy, the attempts of the Libertines to pluck Servetus from the grasp of Calvin recoiled as vain and impotent.

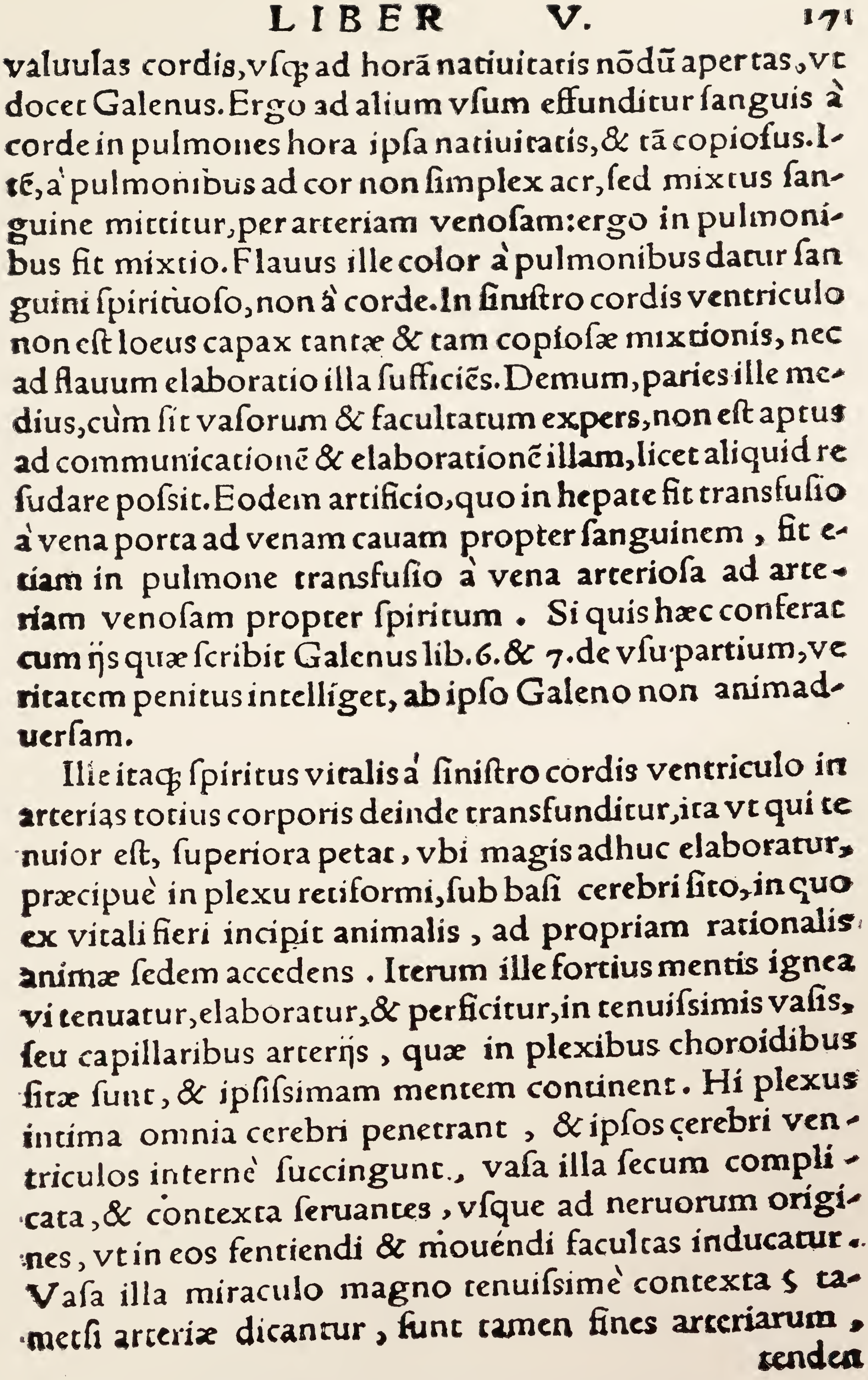
A determining element in sealing the doom of Servetus was the critical condition of the Protestant reformation during the years contemporary with the final period of his activity, or, to express it in other terms, the period which marked the middle of the sixteenth century. The outlook for Protestantism was dreary, it seemed even hopeless. The Marian, or Roman Catholic reaction in England dates from 1533; the Society of Jesus was undoing the work of the Reformers with seemingly resistless energy; Geneva was confronted on the one side by the Valois monarchy; to the North was the empire of Charles V, and Luther had been dead for seven years. The isolation of Calvin was almost complete, "a pillar steadfast in the storm," the last refuge and stronghold of the cause of Protestantism. The escape of Servetus would have implied a fatal weakness on the part of Calvin and his colleagues. The issue was not personal, not local—it was all embracing in character, and in the result was involved the very life of the Protestant reformation.

That his scientific or physiological teachings were not introduced as an element of antagonism to Servetus reveals one of the most suggestive phases of his strangely dramatic and almost unique career. The question seems never to have been presented as containing another form of heresy. The theological aspect of the controversy was the exclusive question, for the oracle of Geneva, Calvin, despite his austerity of nature and inflexibly dogmatic temperament, was endowed with a sense of literary and scholarly appreciation that has won for him no inconspicuous rank in the records of linguistic evolution during the sixteenth century. He was not indifferent to the charm of science, or hostile to its development. In marked contrast to the spirit which asserted itself at the time that William Harvey first announced his discovery (1615) he would not have condemned Servetus for his theory of the lesser circulation. Harvey incurred financial loss by the promulgation of his novel views, and it is said that they were not accepted by any physician beyond the age of forty. When they were given to the world in their matured form (1628) they appear to have gained favorable regard by slow degrees, and in 1633 when Giles Fletcher published his "Purple Island," the "thousand brooks," representing veins and arteries, they were described in accordance with the ancient doctrine, in apparent ignorance of the discovery of Harvey.

The charge of plagiarism which has been suggested with regard to the scientific teaching of Servetus has been investigated by Hemmeter, *Johns Hopkins Hospital Bulletin,* Vol. 26, 1915. His mind seems to have been marked by rare originality and creative power. A vein of mysticism and a touch of poetry reveal their influence in his style; the sensibility of his Spanish nature at times shines through the dialectic dreams and reveries of the work that attained its logical climax at the spot now designated by the monumental granite block (dedicated November 1st, 1903) which constitutes the Expiatory Monument.

The strongly developed anti-dogmatic tendency of Servetus suggested many parallels and analogies in varying ages and under diverse historic conditions. Perhaps no one of them is presented in stronger light than Archbishop Whately, whose attitude in reference to the dogmas formulated by theologians exhibits a striking resemblance to that enunciated by the antagonist of the Swiss reformer. The execution of Servetus was regarded by the contemporary theological world as a vindication of truth and a triumph of righteousness. The entire Protestant communion approved the deed; the gentle Melanchthon was especially emphatic in his expression of concurrence. In no sense did Calvin regret or repent his action. On the contrary, he acquired new strength and influence as the champion and vindicator of the Reformation against heresy. The Church of Rome could not longer exercise the sole prerogative of inflicting vengeance upon the enemies of the Gospel. Scandalized, as Gibbon naturally was more than two centuries later, by the burning of Servetus, it none the less remains an invincible truth that Calvin concretely represented the dominant religious ideals and the purest-political aspirations attained by his own era.

It is a notable historical fact that the penalty of death for denying the doctrine of the Trinity was inflicted in Protestant England during the reign of James I (1611), long after Calvin and Servetus had passed



valuulas cordis, vſq; ad horã natiuitatis nõdũ apertas, vt docet Galenus. Ergo ad alium vſum effunditur ſanguis à corde in pulmones hora ipſa natiuitatis, & tã copioſus. Itẽ, à pulmonibus ad cor non ſimplex aer, ſed mixtus ſanguine mittitur, per arteriam venoſam: ergo in pulmonibus fit mixtio. Flauus ille color à pulmonibus datur ſanguini ſpirituoſo, non à corde. In ſiniſtro cordis ventriculo non eſt locus capax tantæ & tam copioſæ mixtionis, nec ad flauum elaboratio illa ſufficiẽs. Demum, paries ille medius, cùm ſit vaſorum & facultatum expers, non eſt aptus ad communicationẽ & elaborationẽ illam, licet aliquid reſudare poſsit. Eodem artificio, quo in hepate fit transfuſio à vena porta ad venam cauam propter ſanguinem, fit etiam in pulmone transfuſio à vena arterioſa ad arteriam venoſam propter ſpiritum. Si quis hæc conferat cum ijs quæ ſcribit Galenus lib. 6. & 7. de vſu partium, veritatem penitus intelliget, ab ipſo Galeno non animaduerſam.

Ille itaq; ſpiritus vitalis à ſiniſtro cordis ventriculo in arterias totius corporis deinde transfunditur, ita vt qui tenuior eſt, ſuperiora petat, vbi magis adhuc elaboratur, præcipuè in plexu retiformi, ſub baſi cerebri ſito, in quo ex vitali fieri incipit animalis, ad propriam rationalis animæ ſedem accedens. Iterum ille fortius mentis ignea vi tenuatur, elaboratur, & perficitur, in tenuiſsimis vaſis, ſeu capillaribus arterijs, quæ in plexibus choroidibus ſitæ ſunt, & ipſiſsimam mentem continent. Hi plexus intima omnia cerebri penetrant, & ipſos cerebri ventriculos internè ſuccingunt, vaſa illa ſecum complicata, & contexta ſeruantes, vſque ad neruorum origines, vt in eos ſentiendi & mouendi facultas inducatur. Vaſa illa miraculo magno tenuiſsimè contexta, tametſi arteriæ dicantur, ſunt tamen fines arteriarum,

tenden

Servetus on the Pulmonary Circulation

to their account, and that while the statutes which defined its character became gradually void and inoperative with the growth of a tolerant spirit, they were not formally repealed until a time that is strangely and suggestively near to our own era, with its tendency toward anti-dogmatism, if not toward indifferentism and agnosticism—the negation of all definite or positive belief.

The Expiatory Monument, erected by the combined efforts of the Reformed Churches of France and Switzerland and dedicated with impressive ceremonies Sunday, November 1, 1903, occupies a position perhaps unique in the records of the Christian religion, if not in the annals of the world. Monuments have been reared by societies or representatives of ecclesiastical organizations in order to embody in abiding form their expression of repentance and contrition, elicited by the consciousness of grievous wrong or atrocious persecution inflicted upon opposing faiths, or antagonizing systems and beliefs in the sphere of philosophy or in the province of political polemicism. The long deferred confession came from the successors or representatives of the creeds which were involved in the act in its original intolerance. The monument at Champel is one of complete isolation. It has no prototype or counterpart in all records. It was created by the coöperative efforts of those whose religious ancestors had wrought the deed of shame, by loyal successors of Calvin, devotedly attached to the cardinal teachings of the Reformation of which he was the vital force, and which, in its critical moments, he plucked almost from the grasp of despair. The granite block is not merely distinguished from every other monument of which the ages hold record, it stands aloof even from the other monuments erected to perpetuate the genius and the achievements of Servetus. By no means the least of the claims to honor and renown which are the inalienable prerogative of the medical profession is the simple reminder that this phenomenal tribute of the modern spirit had its inspiration and its outcome in the life and labors of a physician. The motto of the city of Geneva, "Post Tenebras Lux," seems to reflect an undreamed of radiance of meaning, as we contemplate the massive granite hard by the Church of St. Peter in which Calvin was accustomed to preach and where his memory is still held in reverence, perhaps more discerning and rational in its nature, as the world with resistless gravitation sweeps into the younger day.

Doctrina sed vim promovet insitam,
Rectique cultus pectora roborant;
Utcunque defecere mores,
Indecorant bene nata culpae.

HORACE.

LITERATURE ON THE HISTORY OF THE DISCOVERY OF THE CIRCULATION OF THE BLOOD.

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XXI

ANTOINE-LAURENT LAVOISIER

The History of the Physiology of Respiration and Metabolism [1]

INTRODUCTION

THERE are three ways of presenting the life and activities of great men.

The first is to give a simple narrative of the man's birth, antecedents, training, social relations, personal, specific, and distinctive talents, and, in a severely objective manner, his achievements. This mode ignores the influence the individual has exerted on the human developments of his time and the progress of Science.

The second is to endeavor to fit his work accurately into the historic mosaic of Science preceding and succeeding him. This is more difficult to perform for, whilst the first requires only a patient recording of already existing data, the second constitutes a research problem that will be solved with varying success according to the fullness of the knowledge of the one that undertakes it and his ability to apply the historic method.

The third is to elucidate a psychography, not a biography of the eminent man and thus aid in making a contribution to what Galton designates as the "dynamics of civilization." [2]

I shall in the following study of the life and work of Lavoisier endeavor to combine the second and third methods.

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[1] Address before the Society of Medical History of Johns Hopkins University. *Janus,* 1921

[2] Francis Galton: "Genius and Heredity."

GENEALOGY OF THE VILLERS-COTTERETS

Antoine Lavoisier, a groom in the king's stables at Villers-Cotterets, married to Anna Castelle or Bastellet, died in 1620.

Antoine Lavoisier, the Elder, a groom in the king's stables, post master at Villers-Cotterets, married to Marguerite Gosset, died before 1637. Five children.

Antoine, the Younger, born about 1606, town crier, married to Madeleine Dubois June 9, 1630, died November 30, 1691.

N††† François Désirée Nicolas

Nicolas

Merchant, born about 1641, married July 28, 1673 to Barbe Lagonée died February 27, 1701. Three children.

Antoine, bailiwick's attorney at Villers-Cotterets, born March 22, 1678, married January 24, 1705 to Jeanne Waioquier.[3]

Nicolas-Hyacinthe born February 7, 1682, died Sept. 18, 1743, at Villers-Cotterets; married a second time on Sept. 24, 1726 to Marie-Anne Niquet. Three daughters.

Martin died in 1734 without posterity.

Jean-Antoine, "a Rich Merchant" Born January 16, 1715, married May 28, 1742 to Emile Punctis, Died at Bourget Nov. 16, 1775.

Marie-Anne, married to Jean Lentz.

Marie-Charlotte married to Jean-Nicolas Papillon.

Antoinette-Françoise, married to Charles-Antoine Parisis.

Antoine-Laurent, born August 26, 1743 died May 8, 1794.

Marie-Marguerite-Emilie born in 1745, died July 5, 1760.

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[3] In 1743 he was named hammer-keeper of the private rangership of forests of Villers-Cotterets; at that time he had quit his post of attorney. Besides Jean-Antoine he had three other sons who died without leaving any descendants.

PERSONAL HISTORY

Antoine Laurent Lavoisier, the son of a rich merchant, was born August 26, 1743, in Paris. Little can be ascertained concerning the amount of culture and the intellectual status of his parents. Alphonse de Candolle in his "History of Science and of Scientific Men" gives no reference to the genealogy. De Candolle, a Member of the Academy of Sciences, Paris, would have been in a position to ascertain the truth on this point. Galton and Wilhelm Ostwald, who endeavored to trace the evolution of genius, make no reference to this side of the ancestry of Lavoisier. There is a remark in Hoefer's "Etude sur Lavoisier" that his parents and grandparents had been educated persons. He was educated at the Collège Nazarin, his studies being directed towards the study of Law. He soon developed a preference for Natural Science. He was instructed in Astronomy by la Caille, in Chemistry by Rouelle, and in Botany by Bernard de Jussieu.

The earliest of Lavoisier's inquiries was an "Analysis of Gypsum," presented to the Academy of Sciences in 1765, and published in the collection of *Mémoires de divers Savants,* 1768. His next work of importance was a paper on "The Practical Illumination of the Streets of Paris," for which a prize had been offered by the Chief of Police, M. de Sartine. This prize was not awarded to Lavoisier, but his suggestions were of such importance that the king directed that a gold medal be bestowed upon the young author at the public sitting of the Academy in April, 1766. Two years later, at the age of twenty-five, he was admitted to the French Academy of Sciences.

He sought and received appointment as Fermier Général, a position unique in the French government of that day. The closest approximation to it appears to be a sub-officer in the Department of Agriculture, controlling mainly the revenues (tax on salts, state sales of tobacco, etc.).

In this position he showed himself to be a skillful administrator. In 1776 Tourgot placed him at the head of the Régie des Salpêtres, where he introduced many improvements into the manufacture of gunpowder. From 1778 to 1785 he gave attention to agriculture and contributed many valuable suggestions to the science of husbandry. In 1787 he was elected to the Provincial Assembly of Orléans. In 1788 he became one of the Trustees of the Bank of Discount and in 1789 he was an Assistant Deputy to the Constituent Assembly. In 1790 he was a member on the Commission on Weights and Measures and developed great interest in the preparation of a new Decimal System. In 1791, as one of the Commissioners of the Treasury, he published an essay *De la Richesse Nationale de France,* in which he presented a plan for the collection of taxes. It is said by the Minister of Public Instruction (who published all of his works from 1864 to 1893), that this essay was a forerunner of a complete treatise on this important subject, and entitles him to a high rank among political economists.

In the *Œuvres de Lavoisier publiées par les soins de son Excellence, le Ministre de l'Instruction Publique, 1864–1893,* incontrovertible evidence of great gift of exposition, power of logic and unparalleled acuteness of scientific thought are manifested. These works embody the principles by which chemical science was to be revived. His greatest work is his *Traité élémentaire de Chimie* 2 vols. 1789, in which the basis of modern chemistry is laid. In 1777 he published in the *Mémoirs of the Academy of Sciences* an essay *du Principe constitutif de la Chaleur* which marks the transition of his attention to the application of chemistry to physiology. Some of his more specific experimentations on physiologic chemistry will be explained later. By this time the revolutionary movement against Louis XVI and Marie Antoinette was in full progress. He was collecting all his writings with the ultimate view of remodeling them into a single work, when the course of revolutionary events brought him to a premature end. Dupin, a member of the Convention, having on May 2nd, 1794, introduced an act of accusation against all the farmers of the public revenue (Fermier Général), Lavoisier delivered himself up and was imprisoned. On the 6th, he was involved in the general sentence of death against the corporation to which he belonged, and two days later he was guillotined with twenty-seven others. His father-in-law, Jacques Paulze, was the third to be executed, and he was the fourth. After the execution, the great Lagrange said to Delambre, "Il ne leur a fallu qu'un moment pour faire tomber cette tête, et cent années ne suffiront pas pour en reproduire un semblable" (Éloge de Lagrange par Delambre, *Memoires de l'Institut,* 1812, p. XIV). His essays were collected and published by his widow (who afterward married Count Rumford), under the title of *Mémoires de Physique et de Chimie,* in two volumes. A complete edition of his works has been published under the supervision of the Minister of Instruction at the expense of the Government. (4 vols., Paris, 1864–1868).[4]

[4] "Lavoisier, d'après sa correspondence, ses manuscrits, ses papiers de famille et d'autres documents, inédits (1888).—"Lavoisier der Begründer der Chemie."—"L'Empirisme et la Science-Esquisse Historique sur Lavoisier. Archives Néerland, de sc. exactes, etc. La Haye, 1871, vi, 473–483."—For Portrait, see, (The) "Hundred Greatest Men." vol. vi, London, 1880. Also Purland, T. Collection II. Brougham (Henry, Lord Brougham): "Lives of Philosophers," see chapter on Lavoisier.

PHYSIOLOGICAL EXPERIMENTS AND DISCOVERIES

Historical comments upon scientific researches are subject to manifold errors. The language of science has undergone many changes in the course of time, so that it is often difficult to grasp the real ideas of an author, even though he writes in our own mother tongue. The more distant his views may be from ours, the more readily are we tempted to form erroneous conceptions of his assertions. Indefinite announcements have often been considered as discoveries, which were actually only made later. Lack of exact scientific knowledge occasionally produces erroneous ideas, which may engraft themselves as truths, by repetition; whereas, if such false ideas were not permitted to take hold of us, our thoughts would be led in another, a safer direction.

No one can draw away from the influence which intellectual activity exerts upon seemingly wholly different fields of knowledge of the day. Sensational discoveries are at once applied to other branches of science, and like newly issued coins, no matter how limited in value, are overestimated until further investigation shows their inferiority.

All this is true of our own time, and if this is so, we may conclude that much which is regarded as genuine coin to-day will be cast aside as spurious by those who come after us. We have a right to hope, however, that as methods of investigation improve, and the true estimation of knowledge acquired increases, there will be less need for elimination in the future. It is precisely to this matter of advance in the methods of experimentation that I wish to call attention.

I will not take you back into remote times, only a distance of one hundred and thirty years, and review the ideas which engaged the minds of men concerning the remarkable phenomena of nature. The great question of the difference between the organic and the inorganic world has from time immemorial been uppermost in the mind of the scientist.

The status of the natural sciences about the middle of the 18th century was by no means a low one. Bacon in a most exemplary manner had fixed the fundamental principles of inductive research; and prominent investigators, such as Galileo, Kepler, and Newton, by observation and experiment, had established the most important fundamental laws of physics. In the science of man, the advances in anatomy by Vesalius were enhanced by the discovery of the circulation of the blood by Wm. Harvey, and placed upon a firm basis by the work of Haller. Chemistry enjoyed special growth about this time, and we will go into this with greater detail, since the activity of Lavoisier is most intimately associated therewith.

In my biography of Haller (Johns Hopkins Hospital Bulletin, Vol. xix, Mar., 1908) see chapter xiv, I referred occasionally to one who stood at the head of science of that epoch—Georg Ernst Stahl—one of the foremost minds of his time, whose views controlled not only chemistry, but general medicine as well. Stahl's influence rested upon the fact that he understood how to gather together into one general viewpoint the various natural sciences of his time. We may at this day reject as false his so-called *phlogiston* and *anima* theories, but we must not forget that both had a great value because the sum total of the knowledge then existing was embraced as a unity in Stahl's doctrines which thereby accomplished what we now expect from each single theory.

When Lavoisier began his career, the *phlogiston* theory was universally recognized and he too was fully in accord with it for he included the hypothetical heat substance and light substance in his table of chemical elements (see Wilhelm Ostwald: *Die Forderung des Tages,* p. 18). Looking upon his researches from their beginning, we can trace step by step, how he at first hesitatingly, then with ever increasing certainty, began to doubt, and with convincing passion for new truths, began to criticize and finally demonstrate the untenableness of the hypotheses of Stahl.

Lavoisier did not bring about this advance by his own exclusive efforts but by the adroit use of the discoveries of his contemporaries. He is, however, clearly entitled to the honor of having applied originally the measures adopted later in chemical processes and therefore he really is to be regarded as the founder of modern chemistry. Borutteau, in Neuburger and Pagel's "Handbuch der Geschichte d. Medizin," calls Lavoisier the *Vater der modernen Chemie.*

It is not my intention to review in general all the work of this great man. His achievements in chemistry proper have been detailed sufficiently often, and for lack of time I must refer to the excellent statements of Grimaux, Neuburger and Pagel, Kopp, Ladenburg, E. V. Meyer, Würtz, Lord Brougham, and others. One more point I wish to dwell upon, namely: Lavoisier's demonstration of the established fact that in chemical processes the amount of matter present is neither increased nor diminished. This fact, first demonstrated by him in a number of experiments, has since become an established law, the law of immutability of the matter present. This law applies not only to processes pertaining to inorganic, but also to organic substances and to all interchanges between these and the outside world. Clear as this is to us now, many statements appeared in the text-books during that century contradicting these theories. According to Vauquelin for example, calcium is generated in the body of the chicken, because he thought he had discovered that more of this substance was excreted than had been ingested by this animal.

The prominent position which Lavoisier holds in the history of physiology is due to his series of researches in special physiologic chemistry, overthrowing the then existing *vitalism* of Stahl as he had in his chemical investigations destroyed the *phlogiston* doctrine.

The subject of fire and heat had attracted the attention of naturalists from time immemorial. Stahl (1660–1734) explained these phenomena by affirming that "heat matter" was contained in all combustible bodies and was made to appear when such bodies were burned. He called this heat matter *phlogiston.* Lavoisier, however, showed that the burning of combustible substances,—coal for example—depended upon the oxygen contained in the air, an element recently discovered by Priestley and simultaneously by Scheele; also that animal respiration is a process of combustion by which this O combines with the other bodily constituents of the animal; and that by this union with O, the expired air contains CO_2 and H_2O, as already previously demonstrated by Black. True to his theories, and not satisfied with his general proofs of these processes, he and Seguin made numerous examinations of the expiratory air of man and animals, and thus added largely to the furtherance of our knowledge of the phenomena of life, examinations since then often repeated by almost identical methods.

When we consider that Lavoisier was the originator of the practical methods, even of organic elementary analyses, in use at the present time, and that by these he demonstrated the composition of organic substances as made up of C, H, O, and the occasional presence of N, we can realize that he laid the foundation of physiological chemistry in such completeness that his successors could build additions to this structure but for many years could not make any material changes. We have, however, not exhausted the subject of the value of Lavoisier's discoveries. A far greater contribution will have to be considered—the explanation of animal heat.

Physiologists of all times have endeavored to expound the phenomenon that a portion of the animal world,—all mammals and birds,—during life, possess a temperature far exceeding that of their surrounding conditions, and unchangeable under the most divers circumstances. Some assumed that this body heat is inborn in the animals, with about as much reason as that of the animists, who attributed it to a product of the soul or to the life-spirit, to which all the mysterious problems of life were assigned An exception to this was made by the so-called Iatro-mathematicians, who accounted for body heat by the friction of the blood circulating in the vessels, or, compared it to heat generated during putrefaction and fermentation;—which came very near to the truth without leading to further conclusions, as one understood the cause of heat formation under those conditions. After Lavoisier, however, correctly interpreted the process of combustion and recognized the similarity of the physiologic chemistry of respiration to combustion, he was naturally led to view the heat formation of animals from the same aspect.

The adherents of the *phlogiston* theory looked upon fire or heat as a matter, or element, like their other "elements," earth, water and air. Later when combustible H gas was discovered, many claimed that this was *phlogiston* itself, or air highly charged with it, whilst on the other hand, O was thought to be *dephlogisticated* air. These views took such a firm hold upon the chemists of that time, that even the well known fact of increase in weight during calcination, which Lavoisier showed to be due to absorption of O, did not convince them contrariwise, but they rather added the new hypothesis that *phlogiston* possesed a "negative weight."

CO_2 was discovered by Black in 1757; N by Rutherford in 1772; O by Priestley in 1774, Scheele in 1775, Lavoisier in 1775, Hirn in 1781.

When, however, Lavoisier had become thoroughly convinced of the untenability of the *phlogiston* theory, he could not but draw into his investigations the heat that was generated during all processes of combustion. Like other naturalists he still supposed this heat was some form of matter, although unweighable:—an imponderable substance, such as magnetism, electricity, and the like. Although this conception has been given up, the method of estimating heat, first demonstrated by Laplace and later utilized by Lavoisier to measure the heat produced by animals, still holds good to-day. And, since they compared the heat given off with the expired CO_2, they believed from their experiments that animal heat was a result of combustion going on in the body.

For the first time, a firm foundation was laid for the understanding of the fundamental phenomena of animal life. The animal body is in the main composed of matter consisting of C, H, O, and N. These components may, however, take up a larger amount of O than they originally contain; therefore, they unite with the O transmitted by respiration, whereby CO_2, H_2O and certain nitrogenous bodies are formed and are expelled. By this combustion, heat is generated. Now since a portion of the body substance is removed by excretion, the animal loses in weight. This loss is replenished by the food substances, which are composed of the same elements as the animal body itself, so that with proper intake it will continue its existence for a longer period of time. Life processes, in many respects, may be likened to the burning of a lamp; in fact, the materials fed to the latter are identical in composition with the foodstuffs.

These are the principal features of the theories advanced by Lavoisier as a result of his physiological investigation, which are of such striking simplicity and so amply verified by his experiments, that one would think that the physiologists of his time would have eagerly seized them in order to emerge from the difficulties of understanding the phenomena of life. This was by no means the case, and by study of the literature of that period, we will endeavor to ascertain the reasons therefor.

Although the reign of the *phlogiston* theory fell under the blows given by the chemical discoveries of Lavoisier, and all reputable chemists sided with the *antiphlogiston* theory (as it was then termed in contradiction), still neither physiologists nor philosophers, and least of all the chemists, in reflecting upon the phenomena of life, could rid themselves of the opinion that these phenomena were of an entirely different order from those of inorganic nature, and although similar in character in some details, must rise from some special power or cause of their own. Lavoisier's chemical theory of respiration and heat production gave no answer to the difficult question of what life really is, and no satisfactory answer to the question, what the hypothetic cause of animal heat was, whether, according to the hypothesis of Paracelsus it might be called *archeus,* or according to Stahl, might be designated as the *anima,* the soul, or vital force. The thinkers of the time inclined to the same views upon which the Alchemists based their theories. Like those who searched for a matter, the *quinta essentia,* which should convert all metals into gold, the possessor of which would become wise and immortal, so did these investigators search for a term or formula, by which all enigmas would be solved and all secrets disclosed. And since no such formula was discovered, shorter terms at least, were adopted for the restricted field of phenomena. As to the phenomena of life in particular, some attributed them to vital force itself, others to the *nisus formativus* of Blumenbach, and yet others to the *Irritability* theory of Haller. The most generally adopted view was that of Stahl, who looked upon the characteristic of life substance as the power of resistence against putrefaction.[5]

In the fortifying and dissemination of these views much was done by a man whose achievements in other fields of scientific labor deserve our fullest recognition, Xavier Bichat. He is the creator of general anatomy, from which our present histology or the finer study of the tissues developed. As a true adherent to the *vitalistic* school, chiefly upheld by the University of Montpelier, he extensively spread this study in his works and for a long time held unlimited sway. His definition of life, "La vie est l'ensemble des fonctions qui résistent à la mort" ("life consists in the sum of the functions which resist death") is fundamentally nothing but a transcription of Stahl's theory. Bichat's great influence, especially in his own country, is to be attributed more to his elegant and attractive manner of discourse, than to the inherent value of his teachings.

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[5] "Vita nihil aliud est formaliter, quam conservatio corporis in mixtione quadam corruptibili, sed sine omne corruptionis actualis eventu."

According to Bichat, we have to distinguish between two kinds of bodies, animate and inanimate, and two series of phenomena, physical and physiological. The first are produced by physical causes, weight, electricity, etc., the other by physiological, extensibility, contractibility, and irritability. The first act according to unchangeable laws, the latter do not. It is therefore impossible to determine the physiological phenomena by the same methods of physical investigation and he speaks with some contempt of such efforts, clearly alluding to Lavoisier, without, however, mentioning his name.

To this inward impulse for knowledge and the search for an uppermost principle, two especial contemporaneous phenomena contributed, which again are closely interwoven,—*natural philosophy* and *galvanism*. Natural philosophy, in the form given to it by Schelling (System of 1797), had exerted the greatest influence in the development of the natural sciences, especially in Germany. This influence has been much dwelt upon and has met with severe condemnation, which its later excrescences justly deserved. A complete estimation of natural philosophy as conceived by Schelling cannot here be undertaken and only the effect of these theories on the conception of the process of life will be pointed out. Schelling places the highest law of nature upon a trinity: everything springs from opposite activities, united together by a third—I) the expanding, II) the retarding activity and III) the weight—which are to be looked upon as the equivalent to matter, whereas this latter represents the product of the three activities. Nature as a product of the active substance proper is represented as inorganic, organic, and cosmic. The activity of nature is retarded in the first, productivity is continued in the second, whilst in the cosmic the co-existence of the first two is brought about. In *inorganic* nature, the trinity is exhibited as magnetism, electricity, and chemism; in the *organic*, as sensibility, irritability, and reproduction; in the cosmos, as light, weight (gravitation), and accelerating and retarding force. All organization proceeds from light, which corresponds to world soul, and tends to the production of intelligence. This, however, is attained only in man, the *microcosm,* who also encompasses everything contained in the *macrocosm,* and therefore can produce everything in the form of thought. The laws of nature must therefore be regulated by the laws of consciousness, and the one may be deduced from the other.[6]

The broad and sympathetic interpretation of John T. Merz can make even the obscure speculations of Schelling appear interesting to a modern physiologist.

In this system, of which no adequate conception can be made from these preceding sentences, analogies play an important rôle. They must often take the place of proof. The influence of galvanism with its polarities is unmistakable. This very fact induced physiologists of that time to take sides with that system. Galvani's discovery of the interesting effects of the current upon muscles and nerves had made a powerful impression. In galvanism the key was sought and found for the phenomena in nerve and muscle activity and later that of life phenomena. Thus, for example, Prochaska, the Viennese physiologist, in the preface to the renewed edition of his text-book in 1820, states that *"life process and galvanism rest on the same foundations,"* and boasts that the new edition was worked out according to this principle, as is seen from the title on p. 47 of the chapter on this subject, *"Life as a matter of fact proceeds from the laws of electrical processes."* The number of treatises on the subject of galvanism which appeared at that time is too great to enumerate. I will mention only one, because it is interesting on account of its author—Alex. v. Humboldt's work in two volumes, on irritability of muscle and nerve fiber.

Looking over the text-books of that time, we find everywhere traces of these influences and I will refer to one in particular because it is typical. Georg Friedr. Hildebrandt, professor of physics and chemistry at Erlangen, is best known by his text-book on anatomy, which appeared in 1789–1792, and was revised by E. H. Weber in 1830–1832. He is also author of text-books on chemistry and natural sciences, and his work on physiology, first published in 1796, was re-published by his son-in-law, Hohnbaum, in its 6th Edition from manuscripts he had left at the time of his death. Hildebrandt was a sober, industrious worker with astonishingly broad knowledge. He constantly emphasized the importance of experience as against construction from general though partially convincing hypotheses. In spite of this (especially in his later editions) "polar matter" and *"Grundkräfte"* ("fundamental forces") play an important rôle with him. All matter is grounded in the union of the dual expansible and attractive force. These may also become disassociated, in which case the superfluous will act also upon distant bodies. By this are generated magnetism, electricity, chemical processes, and the higher stage, life. For this reason both forces act during life; the *expansible* more so, which appears as light in its free state, but in the living body as life-*turgor* [7]), by which development and growth are brought about. The *attractive* force, however, is not missing, for from it we have the chemical and mechanical processes.

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[6] See Schelling's view of nature in "History of European Thought in the 19th Century," by John Theodore Merz, vol. iii, p. 556, etc.

All this may impress one as a play of words or speculation. The same is felt by the author, for he continues immediately after his exposition, as follows: "However, we must not conclude after these explanations that we have lifted the veil of the secret of life, which covers that of the entire creation. All is hypothesis that our physiologists have to offer "as to the problem of life and what the older physiologists said about the same, although more recent investigations have penetrated deeper into the mysteries of nature than was possible with the former." The "deeper penetration" refers to Schelling's natural philosophy as pictured in the mind of Hildebrandt. However, on the whole, he was not inclined to be speculative, and had he appeared in the middle of the 19th century, he would have been looked upon as a rank "materialist."

According to him, nature yields only matter, coarse and fine; by the latter he means imponderable, and it has been so designated from that time to the present. By admixture only of these, all phenomena, even life, are produced, not as Reil thought, by mixture and form, because the latter is contained in the former, being generated by it.

Reil plays the same rôle for vitalism in Germany as Bichat does in France. He founded the *Archives for Physiology,* the first volume appearing in 1796 and containing an extensive treatise on the life energy —"Lebenskraft." According to Reil, there are two series of phenomena, (1) matter and (2) conceptions (opinions). The phenomena of the living body, in so far as they are not due to the conceptions or opinions, have their foundation in animal matter, in the mixture of matter and form, that is, chemical combination. He holds that "energy" is the relation of these phenomena to the properties of matter by which they are generated; consequently, "vital energy" results from material circumstances but is not perceptible to the senses owing to the incomplete state of chemistry and lack of knowledge as to the nature of the so-called *imponderables.* Therefore, every organ, every tissue, possesses its own vital power due to

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[7] I gather the impression from Hildebrandt's book that he uses the word *turgor* (from turgescere, to swell) as modern physiologists use the word "tonus" or tonicity.

Lavoisier

its chemic composition and form. In his endeavor to more closely confirm those properties, Reil then also resorts to defining life phenomena as "potential galvanic processes."

The impotent result of all these speculations, especially their outgrowths, which natural philosophy carried on, was that no special investigation for facts was made in single and individual fields of research but it was aimed to trace everything from the "uppermost principle" by deduction and intuition. This was the reason for naturalists of better type to entirely turn away from these theories. In justice, however, we must admit that philosophic ideas had a stimulating effect on many branches of our sciences, especially embryology and comparative anatomy. The fundamental phenomenon of animal life, however—namely Lavoisier's discovery of continuous oxidation of its component parts—and the metabolism accompanying it, remained unsolved. Even such an erudite man as Herman Lotze, physician and philosopher, could not arrive at a clear understanding; as late as 1851, in his *General Physiology* he makes an unsuccessful attempt to explain it teleologically, by the assumption that the body by its continual changes in its component parts is rendered more able to resist outside disturbances. Likewise, the eminent Johannes Müller, who embraced so masterfully the entire physiologic knowledge, as it was understood up to his time, could not arrive at a correct conclusion regarding this fundamental question.

Lotze was one of the first to scientifically attack vitalism, without, however, making a deep impression. He strove to justify the teleological viewpoint in a philosophical way. It appears possible that this viewpoint can be directly deduced from the premises of the old natural philosophy but his arguments are defective. As a fact there undoubtedly is a definite conformity to purpose and to arrangement in organic nature; however, it is not restricted to organic nature but belongs to nature in its entirety. To understand the details it is often necessary to ask what purpose or object is fulfilled by this or that part of the whole organization. We must, however, be very careful not to overlook the fact that we are introducing a conception thereby, which can only have a meaning from the standpoint of an individual who acts with a precise and conscious purpose: objects of nature, however, present themselves to our investigation as concrete objects. It is not necessary to further expound here Darwin's theory as to how living things develop, conformable to purpose. (See Chap. XXX).

Chemists are justified in feeling an inspiring pride in the contributions their historic fellow-workers have made to physiology, for the next impetus to revivifying physiology came from J. Liebig.

After having materially improved upon the elementary organic analysis created by Lavoisier, he applied his vast knowledge of chemistry to the study of conditions of nutrition in animals and plants. What he taught in this field is in part less the result of experimental investigation than deductive conclusion from basic chemic principles. Much of this has been found to be erroneous. Nevertheless he is entitled to the credit of having spurred on the scientific investigation of metabolism and to have again pointed out to physiologists the important significance of Lavoisier's fundamental doctrines.

Before proceeding further, we must go back to the end of the 18th century and consider the life process in plants, especially their nutrition. As early as 1779, Priestley discovered that plants were capable of converting the atmosphere of confined spaces, which had proven fatal to animals, in such a way that the air again became respirable. In the same year Ingen-Housz showed that only the green parts of the plant were capable of doing this, and then only in the light; that they absorb CO_2 and give off O; that contrariwise, in the dark all plants, and those not of green color, always take up O and give off CO_2 just like animals. In the further course of his research he established his theory of plant nutrition. He showed that the carbon which the plant stores up during its growth could not come from the soil, but came from the carbonic acid of the atmosphere, the oxygen of which is liberated whilst the carbon combines with the elements contained in the water (and in part with N) taken up by the roots. His discoveries were in the main confirmed by Senebier, who, however, erroneously supposed that carbonic acid was absorbed by the roots. Th. de Saussure studied these processes more accurately, and demonstrated that larger amounts of CO_2 act favorably upon the plant only in strong light (atmospheric air contains but a small quantity of CO_2, only 3 to 4 parts in 10,000); that the increase in the weight of the plant is greater than the amount of carbon taken up, because water and salts, the latter in slight quantity only, are absorbed from the soil, that respiration is attended by the decomposition of CO_2 even in light; that the plant does not absorb N from the air but from the soil, which was later positively confirmed by Boussingault. If we add to this that Maret in 1834 showed that fungi, which are lacking in green coloring matter (chlorophyl) take up O and give off CO_2, we will have summarized what Liebig found already established when he entered the field of investigation of plant nutrition.

In spite of these achievements the practical agriculturists, as well as the theorists, clung to the theory that the so-called *Humus* was necessary for plant development and that the plant derived its whole sustenance and carbon from it. Liebig had completely set aside this "humus" (soil) theory and thereby gave to agriculture a rational principle of fertilizing. For us, it is of greater importance to firmly settle the so-called circulation of matter in organic nature. The plant takes CO_2 from the air, N and H_2O from the soil, and forms organic bodies from these substances, which with the addition of a small amount of salts makes up the greater portion of the plant. Plants serve as a nourishment to animals, directly to herbivora and indirectly to such carnivora as thrive upon herbivora. In these, the C is in part again converted into CO_2, and H into H_2O; whilst another portion of these bodies is excreted in combination with N, in a form which in turn readily passes over into NH_4 and as such is again taken up by the plant. The same products result from putrefaction in both animal and vegetable bodies. Therefore the fundamental law of *conservation of matter* discovered by Lavoisier on a small scale for chemical processes has been shown to take place identically in the larger interchanges of matter transformations throughout the earth.

In spite of the emphasizing of the chemical standpoint in the consideration of life phenomena, Liebig held that these depended upon a special vital power, which, however, did not set aside chemical influences, but directed the same and held them in check. This general view held by all writers of that time may be traced back to the apparent difficulty in understanding how the living body maintains its composition, whereas immediately after death it becomes dissociated by decomposition or putrefaction. In reality this difficulty does not exist, since the living body is constantly being attacked and partially consumed but with this difference, that during life the substances lost are at once replaced.

Whilst up to this time the chemical view was exclusively held with regard to these discoveries, physical views came to the foreground in the middle of the last century, incited by Helmholtz through his epoch-making publication, *On the Conservation of Energy*. From this viewpoint du Bois-Reymond in his celebrated introduction to his *Researches on Animal Electricity* explained the untenability of the doctrine of the *vital force* and emphasized particularly that the phenomena of life cannot be differentiated from the inorganic world and that it is the province of science to make plain both series of phenomena upon identical principles in so far as it is scientifically possible.

We have seen above in what manner Laplace and Lavoisier attempted to explain that animal heat was solely produced by slow combustion going on within the body. But their experiments and calculations were not exact enough to settle such an important question. Dulong and Despretz in 1822 sought to determine the matter by new experiments urged on by the prize offered by the Academy of Paris. They arrived at very incomplete results for according to Dulong only 75 per cent and to Despertz 80 per cent of animal heat is generated by combustion. How the balance of 20 and 25 per cent was produced, remained a complete mystery. So that it is not to be wondered at, that in the many writings on physiology it was always suggested that animal heat was something especial, that it was caused by the nervous system, or by vital force, and that therefore, no constant relationship was necessary between the heat produced and that of the material burned.

Still later, it was shown that neither the experiments of Dulong nor of Despretz nor their method of calculations offer a binding conclusion and that we have sufficiently concordant estimations in the results of experiments and calculations of caloric values of the nourishment taken, provided we extend the experiments over longer periods and make sure that the animals remain in equilibrium of nitrogen and carbon, i. e., are in a condition in which an amount of material will be actually oxidized in the organism that is equivalent to the food taken in.

Combustion goes on constantly in all animals. Substances containing C and H enter into them and free O is taken in with respiration, whilst both are given off in the form of CO_2 and H_2O. Heat is thereby generated in all animals, even in the so-called cold-blooded, which are usually not warmer than their environment, because they give up their body heat more readily. Plants also form heat from the O, combining with C and H. In many blossoms, for example, the *Aroides,* in germinating seed and in other similar substances, the formation of heat may be so great as to exceed that which is lost.

Animals not only produce heat but also produce mechanical work. In a large measure this is accomplished by the muscles. This act takes place through the energy of chemical combinations. It has been shown that the process of oxidation is actively increased in the working muscles. Working persons excrete much more CO_2 than those who are inactive. The animal machine which we call muscle, is in this respect exactly like a steam engine, a part of the free energy given off appearing in the form of free heat, only that the muscle works under more favorable conditions than the best steam engine. Whereas in the latter only about 10 per cent of chemical energy can be utilized for work, this proportion may under circumstances reach 25 per cent in the muscle. Count Rumford claimed that a pound of hay given to a horse will yield far greater utilization effect than when the same is used for fuel in a steam engine.

I have personal doubts as to the feasibility of such a comparative caloric test with the imperfect apparatus available in Rumford's day.

The mechanical energy generated by combustion in the animal, when not utilized outwardly for work, is again converted into heat and does therefore not come into consideration in calorimetric investigations, where the animal is confined in a calorimeter. If however, the experiment is conducted in such a way that some amount of the result is taken up in work, a correspondingly smaller portion of heat is set free, according to the above theory. Hirn, the physicist, has attempted to prove experimentally and estimate this mechanical heat-equivalent from the relation between heat and work, but his experiments are not sufficiently exact to claim validity in comparison with the values of the heat equivalents obtained by other methods.

In the United States, Atwater and Benedict have solved this problem more successfully. For the modern physiology of this subject, see Howell, Physiology p. 963, sixth edition.

Animals obtain Carbohydrate for nutriment from plants and give off the same in combination with O as CO_2. Plants take up this CO_2, split up the same and excrete O. Whilst energy is set free during the formation of CO_2, this must be utilized in the dissociation of the same. This energy is manifestly derived from the sun, since the splitting up of CO_2 can take place only in light. There is no doubt that the great heat of the sun represents an enormous storage of energy. A portion of this reaches the earth by radiation and so far as it concerns the green plants it is stored up in the manner described. This not only serves to sustain animal life, but must also serve to maintain body temperature when parts of plants or coals, etc. are burned in a stove? With regard to coal and similar derivatives of plant life used for heating, it is well known that they are the result or effect of the sun's energy, which in part has reached this earth many thousands of years ago (See the "Origin and Evolution of Life" by Henry F. Osborne).

All life then, animal and vegetable, comes from the sun. But whilst we may be justified in assuming a cycle of events, complete in itself for the organic world, we cannot explain the transformations of energy in the same manner. The energy given off in the form of heat by animals radiates into space in the same manner as the energy from the sun, which has served the purpose of heating the earth's surface. But whether animal heat thus radiated into space is returned to the sun is beyond circulation. If, therefore, there are no other special sources besides those of the sun by which we could replenish our supply of energy, and about which we know nothing, the day may presumably come when no more energy will reach the earth. Then all life would cease; and the hour hand of eternity will have performed one revolution.

We have arrived at the goal of our travels. Whilst the path was rough and difficult, we have reached a height from which an extensive view over a vast and rich field is presented to us. This path has been largely planned and laid out by Lavoisier, so that but one, it is true the boldest achievement, was needed to reach the climax, and that was by keen research to confirm the law of the conservation of energy.

From this height it is impossible to consider details more accurately. If we step down and carefully examine the field of biological research, we will recognize the fruits of the labors of so many workers. No opportunity is afforded to mention all. We would find everywhere many actively engaged in gathering in new fruits, sowing seed for future harvests or collecting crops preparatory for future cultivation. We would understand how the aids of chemistry, physics, and the astonishing advances in microscopical technics have cleared up the views of life processes. Above all things our attention would be drawn to a fact of which Lavoisier and his contemporaries had no inkling: the perception of the generation of living beings from elementary organisms or cells. This knowledge, first advanced by Schwann in 1839, has made tremendous progress. Histology and experimental physiology have continued the examinations of cell structure, as the laboratories of the finer processes of life.

However, let us rejoice in our present possession of truth which will serve as a starting point for new discoveries.

Lavoisier's work has helped greatly to put us in possession of these basal truths. Chemists have long ago agreed that Lavoisier is the founder of modern chemistry. It must be admitted, from the foregoing, that physiology too looks upon him as a pathfinder and owes him thanks for some of the most treasured acquisitions. But he also had predecessors who pointed out the way to proceed. As early as 1681, John Mayow recognized that only a portion of the atmospheric air was capable of sustaining respiration and combustion, and that this portion must be contained in saltpetre, for which reason he called it "Spiritus nitro-aereus"; Black in 1757 named the expired air (our CO_2) "fixed air." (See article on Albrecht von Haller, section on Respiration by John C. Hemmeter, in *Johns Hopkins Hospital Bulletin,* March, 1908).

Going back still further, Leonardo da Vinci, a man of comprehensive and marvelous mind, who combined the genius of an artist with the talent of the scientist, recognized that fire consumed air and that animals could not live in air which could not support the flame. We must also note that before Lavoisier, Priestley (in 1774) produced pure O, and showed that it was absorbed by the blood and that dark blood was rendered bright red by it and that this absorption may also take place through the membranes, an important factor in understanding pulmonary respiration. Lavoisier has been criticized for not being fair in acknowledging the merits of his predecessors (Brougham). This may be true, but one thing is certain, it was he who first established the doctrine of respiration and of formation of heat in animals and presented it in such form, that all his successors were enabled to add many single details, though they were not able to change the main fundamental principles.

ESTIMATION BY FELLOW-SCIENTISTS

In 1774 the substances which we now designate as gases, as far as any of them were known at all, were simply called "airs." From the time of Aristotle's four elements—earth, water, fire, and air—new views had been advanced bringing out the chemical elements gradually as they are known to modern science, but there had been only one "air," the atmosphere which we breathe. It is largely due to the English school, Watt, Black, Cavendish, Mayow, and Priestley, that new "airs" were beginning to be recognized. On August first, 1774, Priestley exposed red lead, or *Minium,* in a closed vessel to the sun's rays concentrated by a lens. He found that an aëriform body was evolved, permanently elastic, and that this air had the peculiar property of increasing exceedingly the intensity of the flame. This he called "Dephlogisticated air," upon the principle that the matter of heat and light (the "phlogiston" of Stahl) were abstracted from it by "the return of the calx to its metallic state," which the "phlogiston" was supposed by that theory to effect. "This air had a greater avidity for 'phlogiston' and seized it from the inflammable bodies it came in contact with." He afterwards discovered that its absorption by the lungs in the process of respiration gave the red color to the blood. It was proved to act through the substance of a thin bladder-(diffusion). He obtained the same gas from *Mercurius calcinatus per se.* There are many reasons for believing that Priestley, who was steeped in the "Phlogiston Theory" and who clung to it tenaciously till the end of his life, did not recognize the new thing which he had discovered as a new gas or air different from the rest of the atmosphere but as a part of the atmosphere brought into a new condition (M. Foster). It should be noted that in 1668, Mayow had obtained the evidence that antimonium burnt by the sun's rays collected by a lens increased considerably in weight. "Now we can hardly conceive," says Mayow, "that the increase of weight of antimonium arises from anything else than from 'Igneo-aërial' particles inserted into it during calcination."

John Mayow was in reality merely repeating the views of Hooke whose immediate successor he was. Hooke pointed out the similarity of actions produced by nitre or saltpetre and by air, and announced a theory of combustion, stating that it was effected by that constituent of the air which is contained in the nitre. (Micrographia, 1665.)

There is indeed no absolute truth and man's progress in search of truth is a zig-zag and, sometimes, a retrograde path. Here Hooke and Mayow are reaching a point in 1668 which Lavoisier re-found a hundred years afterward. One might almost theorize that there is a time and place for everything, even including a new truth, and Foster remarks that if a discovery is made before its time, it withers up barren without progeny as did Mayow's. Stahl's "Phlogiston Theory" in 1697 seemed to drag the whole world of thought backwards, for Mayow was on the right path thirty years before this historic error. Priestley, in 1774, was dealing with the igneo-aërial substance of Mayow but he did not discover it because he did not ascertain the true nature of the substance, which he had prepared. What he did discover was, that the air, which he had prepared, was that part of the common air which supported combustion and life.

We are now ready to proceed to the magnificent defense of the claims of Priestley as given by Henry, Lord Brougham, in his attacks on Lavoisier. ("Lives of Men of Letters and Science." Henry, Lord Brougham, 1847, Vol. II.).

Brougham speaks (page 245) as follows: "Thus we find that in at least eight several papers, between the years 1772 and 1780, which discuss the effects produced by the absorption and evolution of Oxygen gas, not the least hint is given by Lavoisier of his own claim, though in five of those papers he mentions Priestley as having given the gas a name, and would therefore acknowledge him as the discoverer, without claiming any partnership for himself. This must be confessed to be a very strong circumstance according to all the rules of evidence and principles of decision, etc., etc. It was not until the year 1782 that his claim for the first time appeared. In a paper read in November of that year on the means of increasing heat by the use of Oxygen he says (page 458), 'Cet air que M. Priestley a découvert à peu-près en même temps que moi, et je crois même *avant moi*' and reminds the Academy that he had announced this discovery at Easter, 1775, as having been conducted with M. Trudaine in Montigny's laboratory some months before. In the memoir already cited, he distinctly informs us that these experiments were not made until February, 1775. Therefore it is to this period that he refers his supposed discovery, not to any part, however late, in 1774. Priestley, who is represented as being a person of the most scrupulous veracity and wholly incapable of giving any false coloring to the facts which he related respecting his own discoveries, states in a work published in 1800 on 'Phlogiston':— 'The case was this: having made the discovery (of what was later called Oxygen) some time before I was in Paris, in the year 1774, I mentioned it at the table of M. Lavoisier when most of the philosophic people of the city were present, saying it was a kind of air in which a candle burned much better than in common air but I had not then given it any name. At this, all the company, and M. and Mme. Lavoisier as much as any, *expressed surprise.* I told them I had gotten it from *precipitate per se* and also *red lead.* Speaking French very imperfectly and being little acquainted with the terms of chemistry, I said *plombe rouge,* which was not understood till M. Macquer said I must mean *Minium.* M. Scheele's discovery was certainly independent of mine though, I believe, not made quite so early.' " (Priestley made the discovery of Oxygen in 1774, Scheele in 1775. Brougham asserts, p. 309, that Lavoisier *never* made it).

In Foster's Lecture on the "History of Physiology" we find the evidence that Priestley understood the new "air" which he had found, not as a new thing, not as a new gas or new "air" different from the rest of the atmosphere, but as a part of the atmosphere brought into a new condition. When he, in 1774, prepared the "igneo-aërial" substance he did not discover it in the sense that he discovered the true nature of the substance which he prepared. What he did discover was that the "air" he had prepared was part of the common air that supported combustion and light, and his great satisfaction was that by this discovery he could explain his results on the basis of the "Phlogiston Theory"; in doing this, he had to explain away the Carbon Dioxide formed during respiration, called, as it was in those days, "fixed air," and discovered by Black. Foster states, "From what I have said it is clear that he had formed in his mind an image of the respiratory process which, so far as oxygen is concerned, we with our present knowledge may call wonderfully exact, save that it was, in a sense, *completely upside down, an image of the truth, but an inverted image.* When we say 'took,' he said 'give,' and vice versa, and this so persistently *throughout the whole business* that anyone who attempts, as I have just done, to describe respiration in Priestley's terms, will find that he has to be very careful at each step lest he represent him as saying exactly the opposite of what he did say. It is so difficult for us, as it was so easy for him, to think of 'oxidation' as a *'giving up'* and not as a *'taking in.'* "

Little wonder, then, that the distinguished English physiologist, establishes an evaluation of the relative merits of Priestley's and Lavoisier's discoveries that is different from Brougham's and states (page 245) that, whether Lavoisier attained his results wholly himself or not, he, and he alone, not Priestley, reached the true *meaning* of the result. He, and he alone, really discovered Oxygen. We might support this by a statement of another brilliant British scientist for, says Huxley, "Lavoisier first showed, by the most conclusive experiments, what was really the composition of the atmospheric air, 1777."

Brougham also charges Lavoisier with neglecting to give due credit to Watt and Cavendish for priority in the discovery of the composition of water; but in a supplement (page 507 l. c.) entitled, "Notes to the Lives of Cavendish, Watt, and Black." Brougham even charges the illustrious M. Cuvier with being unjust to these men and that he did not read any of their works; also, that Lavoisier did not give any credit to Black for the discovery of "fixed air," CO_2. He characterizes Cuvier's eulogy of Cavendish as confused and inconsistent; but this may be based upon the singular way in which the French Academy publishes its Memoirs, for the articles which they contain are never published in less than three and sometimes four years after the year to which they nominally relate. Lord Brougham, in this article, gives evidence of the most penetrating search into historical detail. He is a man of broad experience with an amazing memory, conservative critical judgment, and whilst defending the merits of his own countrymen he has a warm, sane heart in appreciation of the achievements of men of other nationalities and, in the end of his article on "Lavoisier," he gives a résumé of the great merits of Lavoisier's special discoveries and also of his errors.

Before enumerating them I should like to remind you of a striking assertion of the great German chemist, Justus von Liebig, "Letters on Chemistry, No. 3": "Lavoisier discovered no new body, no new property, no natural phenomenon previously unknown, but all the facts established by him were the necessary consequences of the labors of those who preceded him. His immortal glory consists in this, that he infused into the body of science a new spirit; but all the members of that body were already in existence and rightly joined together."

From the statements of the severe foreign critic, Lord Brougham, who endeavors to use his versatile talents to obtain the most rigid kind of justice, it is easy to recognize that Liebig goes too far in this estimate; for, even if we do not accept the conclusions of the other English savants, Foster and Huxley, more equipped by experience and experiment than Brougham to judge correctly the achievements of Lavoisier, we still have Brougham's own assurances that we must credit to Lavoisier: *first,* that he put upon a solid foundation the doctrine of calcination, justly termed by him, oxidation—a discovery which will forever stand the test of every inquiry; *second,* the important blow thus given to the theory of "phlogiston"; *third,* the ascertaining of the chemical composition of "FIXED AIR"; that it is a combination of Oxygen gas with Carbon; *fourth,* the analogy and identity of the diamond with Carbon; *fifth,* the composition of sulphuric and phosphoric acids; *sixth*—there is no doubt that the composition of the atmosphere was first proved by him; *seventh,* the two great discoveries, that of Oxygen, and that of the composition of water to which we have already referred.

Concerning the errors which Brougham enumerates, the only one of consequence is the third (page 273), namely the statement of Lavoisier that all acids contain Oxygen. A more regrettable and far-reaching error, which Brougham does not mention, was a physiological one, namely, that the oxidation of Carbon and Hydrogen supplied by the food took place within the lungs, in the tubes of the lungs, as an oxidation of a *hydrocarbonous fluid* (?) secreted into the tubes. Now, as Lavoisier was not an anatomist and not even a physiologist, and in his other writings does not venture into physiological hypotheses, we may assume that in the article on the "Transpiration of Animals," which he published in 1790 in conjunction with the physiologist Seguin, Lavoisier was led into an erroneous hypothesis by his more distinctly physiological comrade.

ABSTRACTS OF IMPORTANT PUBLICATIONS OF LAVOISIER

EXPERIMENTS ON RESPIRATION IN ANIMALS AND THE CHANGES WHICH TAKE PLACE IN THE AIR PASSING THROUGH THE LUNGS (1777). OEUVRES II, 174–183.

Of all the phenomena of animal life, none are more striking and none arouse the attention of the physicist and physiologist more than those which accompany respiration. Heated mercury absorbs about ⅙ its volume from the atmospheric air by calcination; the remaining air cannot sustain respiration and combustion. On heating the mercurial precipitate we obtain a gas, which when mixed with the rest of atmospheric air, again resembles the latter. A sparrow when confined in an air mass of 31 cubic inches dies in 55 minutes; the volume of air is only slightly diminished, about $\frac{1}{60}$. The residue cannot serve respiration or combustion and renders lime water cloudy. Caustic potash decreases the volume ⅙, loses its alkalesence, effervesces with acids and crystallizes, briefly, it has become united to "fixed air." What remains after this absorption behaves just like the air in which Hg became calcined; if the respirable portion of ordinary air be now added we again have the latter. Therefore only two things are possible, either respiration changes the "respirable air" into "fixed air" (which Lavoisier calls *acide crayeux,* because it may be developed by treating chalk with acids) or an interchange takes place. [He inclined to the latter view because "respirable air" changes the blood to a red color, like the metals (Hg, Pb, Fe) in calcination; but be believed that both occur].

An Evidence of Lavoisier's Inclination to the Phlogiston Theory

N°. 4, P. 537. ON THE COMBINATION OF FIRE MATERIAL (LA MATIÈRE DU FEU) WITH VAPOROUS FLUIDS AND THE FORMATION OF AERIFORM ELASTIC FLUIDS (1777. OEUVRES II, 212–224).

The author is of the opinion that there exists a very fine substance which he calls "matter of fire, of heat, and of light" (matière du feu—de la chaleur et de la lumière) which permeates all bodies, forming with them an equivalent weight, not readily penetrable to each, and that this fluid exists in part free and in others (material bodies) in combination. The supposition is not new and its admissibility is proved by the conformity of the phenomena of which he treats and the fact that it explains all experiences of physics and chemistry. In aqueous solutions in which a chemical combination is effected—for example in adding an alkali to an acid solution to form a neutral salt—a double rôle is played in that a portion of the same enters into the combination, the other portion taking up the salt contained in the solution, keeping the particles of the salt apart, so that each part of the fluid contains an equal portion of the salt, so with fire material, which permeates each body, a portion of which is in combination and the other free, keeping apart the particles of the body. This "free" part is designated as heat. There is, of course, no means of estimating the quantity but this may be inferred by the expansion of the body. In bringing together different substances, if chemical dissociation or union takes place, it depends upon whether or not the new formed substances required as much *fire material* as the original; in the later case heat must either be set free, which is noticed in the result, or it is abstracted from the surrounding bodies. Since all bodies take up *fire matter,* measurements of same cannot be determined exactly. All vessels also are lined with pores through which *fire matter* passes in, so that it cannot be measured like a fluid or gas shut off. Cooling sets in upon evaporation as shown by Richmann, Mairan, Cullen, and Baumé, and vapors are therefore produced by the union of fluids with *fire matter.* Descriptions of experiments which he made conjointly with Laplace on the evaporation of fluids under the cell of the air pump, and refutation of the claims of the heat given off from lime stone and effervescing alkalies by addition of acids, are also given.

Mémoirs sur la Combustion en général (1774).
Oeuvres II, 225–233.

N°. 7, P. 538. MEMOIRS ON HEAT (LAVOISIER AND LAPLACE (1780). OEUVRES II, 283–285).

This work is in 4 parts. In the first the terms "free heat" "heat capacity" and "specific heat" are defined.

The authors do not decide between the two hypotheses on the nature of heat. One holds that it is an imponderable fluid, whilst the other thinks it is due to the oscillation of matter. They claim, what may be true of both views, the unchangeableness of free heat by simple mixture of the bodies, whereas in chemical processes it may be increased as well as diminished. At any rate we may formulate the rule that every change of heat, whether real or apparent, occurring in any alteration of the bodliy system, must appear in inverted order when that body assumes its original state. They designate *"heat unit"* as the amount of heat necessary to raise 1 lb. of water 1 degree on the scale of 80 degrees; they term "heat capacity" or "specific heat" the relative heat units required to raise equal masses of different substances to an equivalent number of degrees. Conditions may vary in different temperatures, but it may be inferred that they are sufficiently constant within the limits of from 0 to 80 degrees. Since the admixture method is not sufficient to determine the specific heat, they show how it may be measured by means of ice, and describe the theory of the development of the ice calorimeter and the apparatus employed.

In the second part the value of specific heat is considered with reference to water for a number of substances; further the heat which is generated by mixture of H_2SO_4 and H_2O; Ca with H_2O; Ca with KNO_3; the combustion of various substances and the heat developed, in a guinea-pig.

The third part consists of hypothetic considerations foreign to our subject.

In the fourth part the heat from combustion of coal and the amount of CO_2 is again estimated and compared to that produced in a guinea-pig and the air expelled. The comparison of value as to conformity is sufficient to warrant the conclusion that respiration is a form of slow combustion, similar to the burning of coal. The heat generated by the lungs is taken up by the blood and distributed throughout the body; and like Crawford, the authors believe that the difference of heat capacity between arterial and venous blood also contributes to this. From all this they hold that the maintenance of an equal temperature in animals, with constant loss going on, is in great part due to the generation of heat by the union of the inspired "pure air" with the bases of the "fixed air" furnished by the blood.

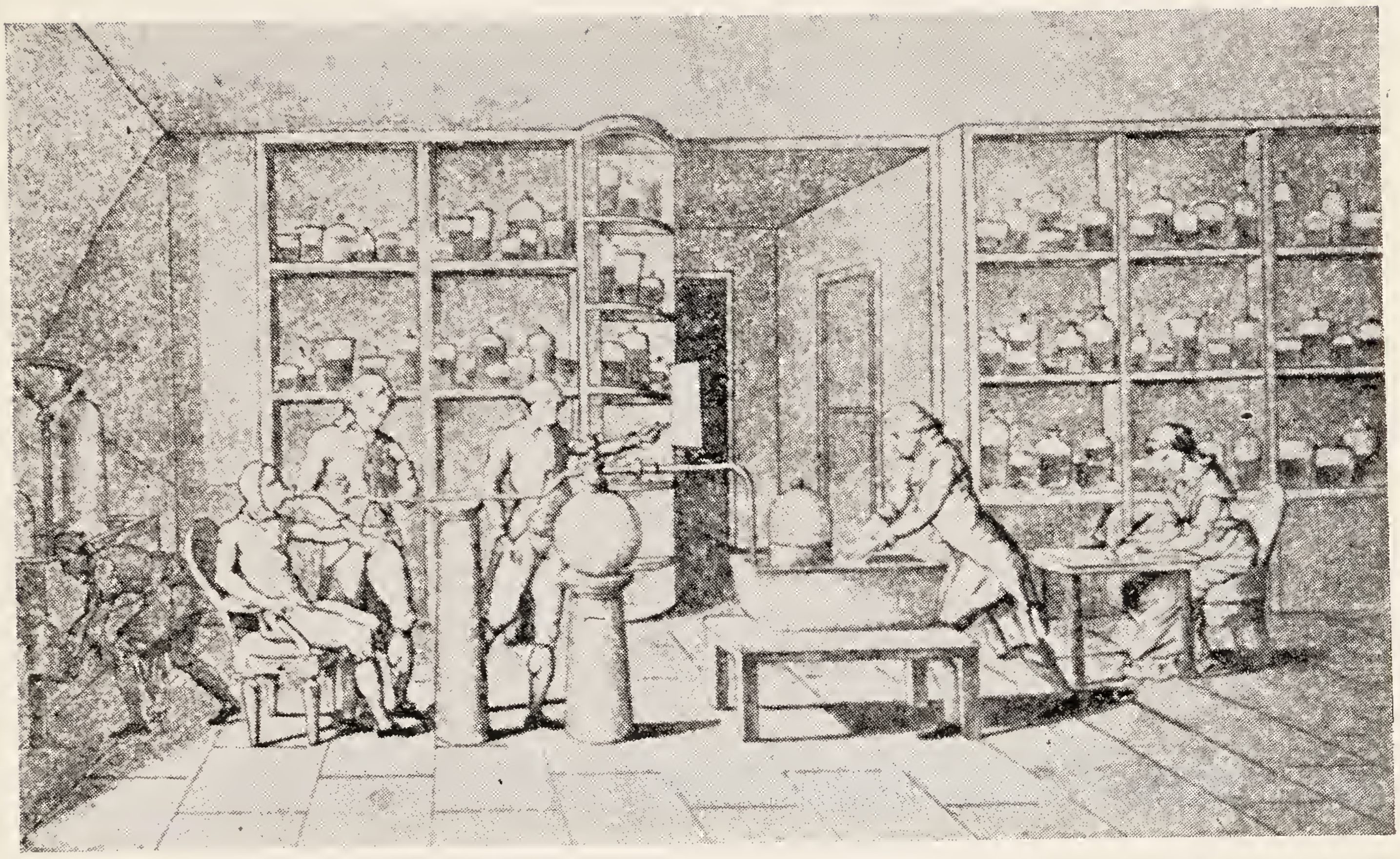
N^{os}. 11 AND 12; PP. 539 AND 540. FIRST MEMOIR ON RESPIRATION IN ANIMALS (1790). OEUVRES II, 688–703. PREMIER MÉMOIRE SUR LA RESPIRATION DES ANIMAUX

FIRST MEMOIRS ON TRANSPIRATION IN ANIMALS (1790). OEUVRES II, 704–714. PREMIER MÉMOIRE SUR LA TRANSPIRATION DES ANIMAUX

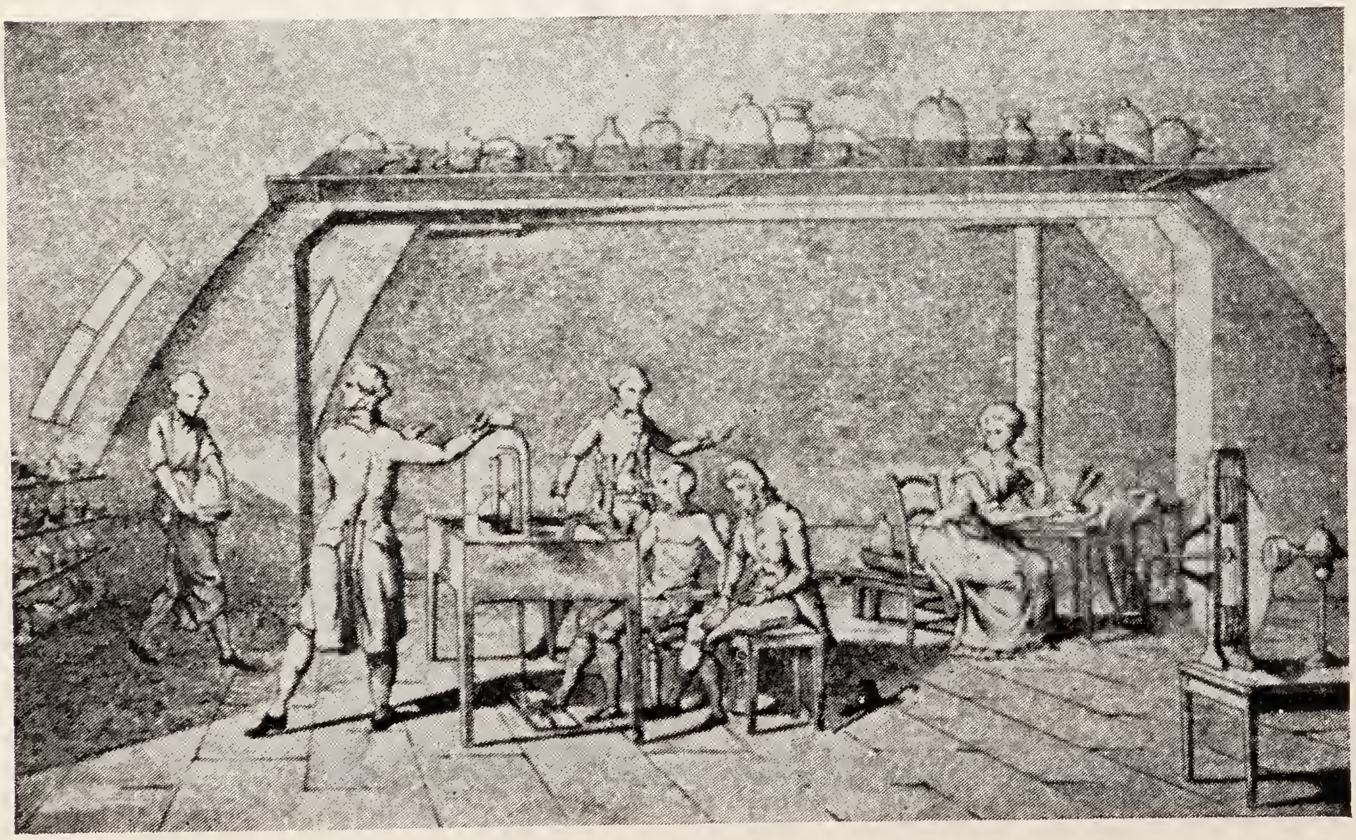
These two works of Lavoisier and Seguin, give a more exact description of their former experiments on respiration and animal heat. In the experiments of Laplace and Lavoisier the amount of heat produced in animals was found to be somewhat greater than the CO_2 given off. To account for this Lavoisier (1785) gave his opinion that probably some H besides C was burned during respiration. Respiration goes on in pure O and in a mixture of O and N under various conditions precisely in the same manner as in atmospheric air. N is neither absorbed nor excreted, but can be replaced by other indifferent gases. The amount of O taken up is greater in a lower temperature and is increased during digestion and muscle activity. The bodily temperature changes but little in the latter, but the pulse is accelerated in almost direct proportion to the amount of work yielded, so that mental exertions, not measurable, such as recitation, composition, etc., may be determined thereby in a mechanical way. The average amount of O used by a man in 24 hours is estimated as 2 lbs. 1 oz. 1 dr.; the C excreted as 10 oz. 4 dr; and the H (calculated as indirect from the superabundant O) as 1 oz. 5 dr. 51 gr. The article closes with observations on the nourishment of the working classes, regulation of heat and disturbances of weight equivalents.

For the examination of cutaneous transpiration they used an air-tight covering for the entire body, respiration being conducted by means of a tube in mouth. They differentiate between pulmonary respiration and transpiration. They surmise that carbonaceous and hydrogenous fluid is "transpired" in the blood of the lungs and then burned. With the CO_2 and H_2O thus formed the water is evaporated as excreted by the blood. The latter is lung transpiration water, the former lung respiration water. To estimate these separately, water is calculated from the O and C, with the proviso that all CO_2 originates in the lungs or the blood whilst circulating in the vessels. The sum total of respiration and transpiration is formed by weighing before and after the experiment, and the part taken by the lungs alone, by weighing with the apparatus before and immediately at the end of the experiment,—the difference then showing the part played by the skin.

The authors are not deceived by the uncertainty of this hypothesis.



I. Lavoisier's Laboratory



II. Lavoisier's Laboratory

A résumé of all the published works of Lavoisier was given by Seguin in 1814.

MADAME LAVOISIER

Lavoisier's family life is of singular interest. He was married in 1771, while traveling in Switzerland. His wife was a daughter of M. Paulze, a Contractor of the Finances under the old régime of Louis XV. She is described as having been very handsome, rich, clever in conversation, and interested in science. Lavoisier had reason to suppose that she would make a desirable wife. It is said that there was a great disparity in their ages, for she was fourteen years old when she married the distinguished chemist and he was twenty-eight. If there were any matrimonial difficulties, they did not leak out, for she assisted him faithfully in the laboratory in translating, and in drawing the illustrations for his great work "Traité de Chimie."

Prof. Dr. E. Korschelt and Prof. Dr. G. Linck's *Handwörterbuch der Naturwissenschaften,*[8] BIX s. 706 shows the interior of Lavoisier's laboratory during an experiment on the Respiratory Gaseous Interchange. Three assistants are shown, and a human subject seated in a chair respiring through an apparatus for the collection and estimation of gases. Lavoisier himself is dressed in a long frock coat with a wig on his head, and is seen busy at an immersion trough. At a table to the right a lady is pictured busily taking down the notes of the proceedings. Prof. B. Schöndorff, the writer of this article, has taken this picture from the work of Grimaux on Lavoisier. The fine illustration in Grimaux bears the inscription that it is the production of Madame Lavoisier herself; and that the lady to the right is Madame Lavoisier, drawn by herself.

During the years which followed her husband's death, Madame Lavoisier was preoccupied with numerous affairs of a personal nature. She had to supervise the restitution of the confiscated property, to settle the estate of her father with her sister-in-law Mme. Christian Paulze; and her husband's with the latter's heirs, Augez de Villers, Augez de la Voye's children, and the mesdames Parisis, Papillon, and Lents the daughters of Nicholas-Hyacinthe Lavoisier. This finished, she hastened to complete the task undertaken by her husband and arrested by his death.

In 1792 Lavoisier had conceived the idea of publishing his principal data, and of collecting the works of the other scientists who had contributed to making modern chemistry a greater science. This work was to be compiled in eight volumes. The first imprint was made at the end of April 1793. Lavoisier, with the help of Seguin, did the proof-reading during his detention. When he was executed the first two volumes were almost entirely printed as well as a few pages of the fourth. The fourth year (1796), Mme. Lavoisier had a meeting with Seguin in order that these works might appear before the public and asked him to edit a preface, but she required that it should brand the men guilty of Lavoisier's death. Seguin refused for, "To denounce those who had condemned him or who let him perish does not strike my fancy and, on the other hand, would diminish the horror that the event has left in all hearts. There are certain things more difficult to tell than to have told by the consciences of those to whom they were presented. Oftentimes they acquire more force from these witnesses. If you do not agree with me, you may have your own views printed; in your own words they would acquire a more convincing tone proceeding directly from you." (Letter from Seguin dated July 18th, 1796.)

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[8] Vol. ix, p. 706, Jena, 1913, Publ. Gustav Fischer.

At the same time, he sent with this letter a sample of a preface which could never have pleased Mme. Lavoisier, for he attributed to himself an equal part with Lavoisier in the project of the publication of the works, saying, "The plan *we* had thought about, the design *we* were meditating," etc.

Mme. Lavoisier gave up the project temporarily and took it up a few years later. She edited an introduction herself in which she explained the conditions under which Lavoisier had undertaken this work. We publish it in its entirety:

"In 1792, Lavoisier conceived the idea of making a collection of all his works that had been read at the Academy during the twenty preceding years. In a way, it was the making of modern chemical history.

"To make this history more interesting and more complete, he had proposed to add the works of those who, having adopted his system, had made experiments in his behalf.

"This collection was to number about eight volumes.

"Europe knows why they were never completed.

"Almost all of the first, all of the second, a few pages of the fourth, have been finished.

"Many scientists have desired to see those published. We hesitated a long time. It is difficult not to feel a certain fear when it comes to publishing the unfinished works of a man who justly enjoys a great reputation. It is when we have lost him that friendship must become impartial and show only what might add to the glory of a cherished and honored name.

"We would have persisted; and their fragments would never have appeared if they had not contained (page 78 of the second volume), one of Lavoisier's ideas which claims, from the accompanying proofs, that he was the founder of the new chemical theory.

"Thus it becomes a duty to confirm the views which scientists have expressed in regard to this question.

"We beg their indulgence for the mistakes that may have been made in some other part of this collection.

"They will surely grant this request when they learn that most of the proofs were reviewed in the author's last moments, and that, although he did not ignore the fact that his murder was premeditated, M. Lavoisier, calm and courageous, compiling a work which he thought would be useful to science, gave a memorable example of the serenity which intellectual purity and virtue can produce in the most unfortunate hours."

The two volumes of the "Treatise on Chemistry," published in 1805, were not placed on sale, but given to all of the eminent men of this period. Cuvier thanked Mme. Lavoisier in the following words for her gift of the work to the class in Sciences at the Institute:

"Madame, the class requests me to thank you for the work which you have so kindly given them, precious to all as well as to each one. Allow me to offer the testimony of my gratitude. All the friends of Science are indebted to you for your heroic determination to publish this collection, and to publish it as it had been originally planned, a sad memorial of your loss as well as theirs. These incomplete volumes, these unfinished utterances, make a profound impression. What truth, it seems, we were about to learn in a book which began with so many facts so beautiful and so new; and yet how we feel the horror of the crime renewed in all its force, a crime which has afflicted humanity perhaps for centuries."

At that time Mme. Lavoisier resided in a spacious mansion on Aujon-Saint-Honoré Street, and occupied its parlors where the most illustrious scientists met, as before her husband's death. Among these were Delambre, Cuvier, Prony, Lagrange, Laplace, Berthollet, Arago, Biot, Humboldt, etc.

Madame Lavoisier later acquired an unenviable reputation through her second marriage, which was to Benjamin Thompson who became Count Rumford, an American physicist born in Woburn, Massachusetts, in 1753; one of the learned men who visited her home. This was Benjamin Thompson's second marriage. His first marriage was in 1772 to the widow of Colonel Benj. Rolfe, a daughter of Rev. Mr. Walker, of Concord. Great man though Benjamin Thompson was, he must have been possessed of views concerning marriage which could hardly be considered equitable. Pictet (quoted by Edwin E. Slosson in "Leading American Men of Science," edited by David Starr Jordan, page 14) reports his sentiments as follows: "My ideas were not yet fixed; one project succeeded another and perhaps I should have acquired a habit of indecision and inconstancy, perhaps I should have been poor and unhappy all my life, if a woman had not loved me—if she had not given me a subsistence, a home and an independent fortune. I married, or rather was married, at the age of nineteen. I espoused the widow of a Col. Rolfe, daughter of the Rev. Mr. Walker, a highly respectable minister and one of the first settlers of Concord."

There are evidences that this first wife of Rumford exercised the privileges of a princess towards a lover of low degree. She died in 1792 leaving one daughter, Sarah, some times spoken of as Countess of Rumford. Dr. Young of the Royal Institution of London says that Benjamin Thompson was mild in his manner; in disposition, however, he was authoritative and dictatorial.

Though born in America, Thompson had not espoused the cause of his fellow-citizens. Loyalist subject, he fought against American Independence in the ranks of the British army, became Lieutenant-Colonel, and was knighted in England. In 1790 he served in Bavaria as Aide-de-Camp to the Elector, Charles-Georges, who named him Superintendent of the Police. In the discharge of his duties, he displayed his administrative and scientific qualities, reorganized the Bavarian army and suppressed begging by establishing workshops. At the same time he was performing wonderful experiments with heat and light, inventing steam-heating and, on perceiving the escape of heat produced by the boring of the cannon, had indicated the transformation of motion into heat.

Named Count of Rumford by the Elector, Charles-Georges, he withdrew from the Bavarian army upon the latter's death and came to live in France. He was intelligent, had a stately bearing, with a great reputation as a philanthropist and scientific man. He proposed to Mme. Lavoisier who, hoping to find a few happy days with a man as devoted to the progress of sciences and to the happiness of humanity as was Lavoisier, consented to marry him. The marriage was celebrated October 22nd, 1805. But very soon differences of character and opinions appeared. Finally, after four years of strife and recrimination, they agreed to separate in 1809.

These two human beings, great souls as they undoubtedly were, were too independent; they lacked mutual adaptation and differed decidedly in their convictions and tastes. Madame Lavoisier Rumford has been accused of being fond of lavish entertainments and elaborate dinners;

while the Count ate little, drank less and detested idle conversation. De Candolle presents an interesting analysis of their temperaments. He states that Rumford (meaning Benjamin Thompson) was cold, calm, obstinate, egotistic, prodigiously occupied with material elements of life, and the very smallest details of inventions. He wanted his chimneys, lamps, coffeepots, windows made after a certain pattern. He contradicted his wife a thousand times a day about the household management. Madame Rumford was a woman of resolute, willful character; her spirit was high, her soul strong, and her personality masculine. In one of his letters to his daughter, Sarah, Benjamin Thompson (Count Rumford) narrates a scene from their married life which will explain why they were separated:

"A large party had been invited which I neither liked nor approved of; and invited for the sole purpose of vexing me. Our house being in the center of the garden, walled around with iron gates, I put on my hat, walked down to the porter's lodge, and gave him orders on his peril to let no one in. Besides, I took away the keys. Madame came down; and when the company arrived she talked with them, she on one side and they on the other side of the brick wall. After that, she goes and pours boiling water on some of my flowers."

It is necessary to bear in mind that although Benjamin Thompson had been married twenty years to his first wife, Sarah Walker Rolfe, he had little training as a husband, for he had left her in America when he went to England three years after their marriage, thus being separated from her seventeen years;—so completely that Sally Thompson, who was later transformed into the Countess of Rumford, knew her father only from a portrait of him. At any rate it must be plain that the failure of Madame Lavoisier's second marriage was not exclusively her fault, as might be inferred from De Candolle's description of her personality. A French lady of culture, who had lived twenty-three years with a genius like Lavoisier, and who could not live four years with Benjamin Thompson, would probably have her own story to tell, explanative of the marital unhappiness with Count Rumford. His dictatorial manner becomes evident from his own statement of how he humiliated his wife by giving the porter orders *on his own peril,* not to let in a large party that had been invited by her, besides taking away the keys. Four years of such life were enough. They parted and lived happily ever after.

"Since that time, and for twenty-seven years," says Guizot, who called occasionally at Mme. de Rumford's, "no event, we might say not another incident, disturbed Mme. de Rumford in her noble and agreeable mode of living. She belonged in an especial sense to the friends and to the society which she received with a rather singular blending of *rudeness and politeness.*"

"Always a very good companion ("de très bonne compagnie") *of great world experience, in spite of her brusqueness of language and fantasy of authority.*" (Guizot, La Comtesse de Rumford—"Melanges Biographiques.")

"Mme. Lavoisier's mansion was closed only at her death."

"Besides the scientific men whom we have mentioned, there also met Theodore de Lameth, Lally-Tollendal, le duc de Flaisance, le duc de Cadore, M. de Barante, le duc de Broglie, M. et Mme. de Reuinsat, Lade Edgeworth, Lord Holland, le duc d'Albufera, &c., &c."

Mme. Lavoisier died suddenly February 10th, 1836. Born in 1758, she became, therefore, 78 years of age.

She kept her circle of distinguished friends about her until the day of her death, and one of the most brilliant of the eighteenth century salons of Paris perished with her.

During the social meetings of the International Medical Congress of Paris, in 1900, I had the pleasure of meeting Dieulafoi, Hayem, Lannelongue, and the physiologist Gaulthier, and also was fortunate enough to be invited to attend the unveiling of the monument to Lavoisier near the Madeleine Cathedral.

At the dinners by various eminent clinicians, the conversation turned to Lavoisier and his wife. There was an elderly clinician who gave evidence of intimate knowledge of the family and of their physicians. His information was that the experience of witnessing the execution of both her father (M. Paulze) and her husband on the same day had had a profoundly injurious effect on Madame Lavoisier, which may have more than accounted for the "Singular intermingling of rudeness and politeness (brusquerie de language)" naïvely referred to by Guizot (La Comtesse de Rumford). Yet it is hardly to be believed that illustrious men like Cuvier, Berthollet, Laplace, Lagrange, Arago, Biot, Humboldt, and others could have found a woman interesting, unless she combined exceptional intellectual gifts with fine feminine grace and moral force.

It matters not whether Lavoisier utilized the work of others or not; his was a great constructive intellect that turned away from speculative theories and built and made evident, carefully tested foundations for future science. Among the pioneers of modern civilization there is no more honored name than his.

For the exact psychographic estimation of his mind-processes we must

Lavoisier's Statue

wait until another, such as he, shall arise, and out of the chaos of present-day psychological research well tested experimental foundations for a future science of Thought, will be born. Already we seem to see the dawn of the light, shining through the chaos of obscuration, and we have the laboratory beginnings of experimental opportunity. The new Lavoisier will surely be born, a man or woman of hereditarily healthy brain-cell, country-bred perhaps, whose ancestors have been quiet, successful, men of business detail; who, freed from money cares and encouraged by sympathetic fellow-workers, shall carry out to its conclusion each basic experiment which he undertakes, building a solid road to scientific success.

One rises from a perusal of what is known of Antoine-Laurent Lavoisier with an intense desire to know more of his real self, and a sense of disappointment that no record has been made of his mind-processes which solved so simply the problems that had baffled human effort for ages. It is one of the purposes of the present cluster of biographies to quicken the desire of scientific men for the coming of an era of experimental psychography, the establishment in many great centers of laboratories where the truths of human mind-action shall be laid as solidly as those of modern chemistry and physiology. A great storehouse of useful data will be accumulated if biographers will, in view of the near development of psychography, lay new stress hereafter, on the reactions of scientific men to the facts they are investigating, and their methods of attacking problems.

The conclusion of the whole matter, is that according to older psychographic methods of classification Lavoisier must be considered an objective investigator, a *Klassiker,* of Ostwald's scheme, one of the "Anschauenden" of Goethe's third group. How he would be listed in the laboratory records of the more exact psychography of the near future will probably never be determined, for among the tragedies of the world's history is the lack of studies and statistics of the mental processes of its great thinkers.

We can trace the origin of the method of his thought with some accuracy, perhaps, in his heredity and by the circumstances of his life. As shown by the Genealogical tree offered, he came of village ancestry, a race of Lavoisiers who for many generations had been officials in their ancestral home place. This implies a certain practical grasp of affairs which made them efficient in complicated details. They had what is known as "faculty." They seem to have risen from lower to more important positions of public responsibility, until the father of our scientist launched into trade and proved a money maker.

These traits come out strongly in Lavoisier. He takes up science, which seems unpractical, yet after hard work at it and little gain but glory, shows his practical side by marrying a rich wife. Having married her, however, and through her father gotten a satisfactory income of his own, he goes into the practical development of the affairs committed to him in a way, to perfect and systematize the whole subject of chemistry and build it for all time to come.

As he who took hold of Chemistry and Physiology and put these into order, so in his research on gunpowder, his work in husbandry, his monetary decimal system, his treatise on taxation, he is exactly the same type of classical genius, no matter what the nature of his problem happens to be.

The new German series of Autobiographies of eminent Medical thinkers entitled: *Die Medizin der Gegenwart in Selbstdarstellungen,* edited by Prof. Louis Radcliffe Grote Leipzig, F. Meinert Verlag, makes an auspicious beginning for the self study of scientific workers.

XXII

PASTEUR

NO other master mind that has worked in medical science has received the widespread, almost unanimous approval, loyalty, and affection not only from his own countrymen, but the entire world, as Louis Pasteur. He was born on December 27th, 1822, at Dôle, the son of a poor tanner who had, however, an admirable record as a soldier under Napoleon I, and in 1814 had received the Cross of the Legion of Honor. Later, the father, Jean Joseph Pasteur, moved to Marnoz, where he established a tannery, but he did not succeed in business there and moved to Arbois, where he again started in his occupation as a tanner. His little son, Louis Pasteur, first was sent to "Ecole Primaire," attached to the College of Arbois. René Vallery-Radot in his "Life of Pasteur," gives a fascinating description of the childhood days and early instruction of Louis Pasteur, and how he earned the epithet "The Artist" by his Arboisian friends because he showed talent in drawing.

I have not been able to trace historically the etymology of the name Pasteur, but presumably it is derived from the Latin "Pastorius." For the land in which the early ancestors of the great physical chemist and biologist lived is in that district of Northern Gaul, which Julius Cæsar describes in the first part of his work "De Bello Gallico" and here his memorable battle against the German king "Ariovistus" was fought. No doubt the Romans gave the name "Pastor" to the possessors and guardians of the herds in that much tortured district which has not enjoyed continuous peace since the time that Cæsar introduced this race to history.

When I say that Pasteur is the most widely acclaimed master of medicine at the present time, I do not wish to exclude Alexander von Humboldt, but Humboldt was not a medical man, nor did his telling work exert itself specifically in medicine. But to Pasteur were given such honors as never have been accorded to a scientific man before or after him. For example, a movement was set on foot for a popular subscription to establish an institute, not only for the treatment of hydrophobia but also for the scientific study of means to control other infectious diseases, as anthrax, diphtheria, tuberculosis, etc. It was said that the extraordinary enthusiasm which accompanied the foundation of this great institution has not been equaled in our times. Considerable sums were subscribed by foreign countries whilst rich contributions poured in from every part of France. The most obscure little towns and villages organized fêtes to send their little gifts. At the time of the opening ceremony, November 14, 1888, 3,586,680 francs had been subscribed, but there are more than thirty Pasteur institutes at the present day, established throughout the civilized world. A district in Canada, bordering upon the state of Maine, has been named in his honor, and a town in Algeria, in the department of "Constantine," has been named "Pasteur." Alexander II, Czar of the Russians, and Pedro II, Emperor of Brazil, visited Pasteur in his Institute. The honors to Alexander von Humboldt in his own country were not equally great, but then he as a climatologist, geographer, and author of the monumental work "Cosmos," was not engaged in lines of research that could make such a direct appeal to humanity. As an encourager to young scientific men he was unequaled; in the United States a city in Kansas and another in Tennessee bear the name of Humboldt as well as a river in Nevada and a lake in the same State.

There are three outstanding features in the formative periods of Pasteur's life. The first was a profound religious conviction, which appears to have been inherited and fortified by continued deep thinking upon the history and development of religion. He not only retained his religious convictions during his entire scientific endeavors but his concepts of God became more and more sublime and at the same time more real to him as his intellect grew in every direction which the human mind is capable of. The second remarkable feature of his early education was that he had the good fortune to come under most efficient and sympathetic teachers. From all that I can gather concerning M. Romanet, former Head Master of Arbois College, M. Dumas of the Sorbonne in Paris, and J. J. Biot, they must have been of the type of Humboldt, Johannes Müller, W. K. Brooks, Mark Hopkins, and Kielmeyer, the teacher of Johannes Müller, Goethe, and the great comparative anatomist Cuvier. This was an extraordinarily rare good fortune to befall Pasteur. In order to appreciate the harm that can be done by teachers who, though possessing a most comprehensive knowledge of their subject, lack the gift of exposition and also the will to enter into the psychology of their pupils, I refer to the various essays in Ostwald's "Grosse Männer," and to Sarton's "Biography of Evariste Galois" (Scientific Monthly, Oct. 1921, p. 365). The harm done by incompetent teachers, temperamentally and psychologically unfit to be educators, is far greater than the general public has any knowledge of. It is not only a great detriment to the education of the young in any system of public schools, partly under political control, but it is noticeable even in universities where it occurs repeatedly that a man is engaged as a teacher simply because he has completed a piece of original research work and obtained his doctor's degree.

The mass method of teaching pupils and students without any effort at individualization, and without any adaptation to the personality of the student, does more harm than good. Departments of Education should aim to develop the personal side of instruction and the psychologic determination of the specific and characteristic mental qualities of the pupil.

The third characteristic feature of Pasteur's education did not reveal itself until he began his investigation on Crystallography, at least not before the year 1857. This feature does not appear so evident in his education as it does in the succinct stages of his original investigations, which were contrary to his own personal inclinations. It seemed as if something external to himself compelled him to work in fields which, as he himself had formerly stated, were not as congenial to him as other spheres of research might have been. It will be remembered that at the "Ecole Normale," Besancon, he met Charles Chappuis, a philosophic student who became his intimate friend. Filial piety, brotherly solicitude, friendly confidences, all of these early human joys mark the friendship between these two youths and many insights into the psychology of Pasteur are gained from the latter's letters to Chappuis.

In January, 1841, Pasteur had received an offer from M. Repecaud to become Preparation Master at the Royal College of Besançon. Here he received remuneration and experience as a teacher, but his desire was to go to Paris to an "Ecole Normale," and in his preparation for this, a prolonged study of mathematics was necessary. Here we find for the first time that this subject did not appeal to him very strongly. He wrote in April, 1841, to Chappuis, "One ends by having nothing but figures, formulæ and geometrical forms before one's eyes" . . . "On Thursday I went out and read a charming story, which much to my astonishment, made me weep. I had not done such a thing for years. Such is Life." In 1843 he was admitted fourth on the list to the "Ecole Normale," but before that, in the year 1842, in his examinations before the Dijon Faculty, he passed less brilliantly than he had done for the Baccalaureat des Lettres; in Chemistry he was put down as only "mediocre." On August 26th, 1842, he had been declared admissible to the examinations for the "Ecole Normale," but he was only the fifteenth out of twenty-two candidates. He considered this too low a place and resolved to try again the following year, when, as we have stated, he was admitted as fourth on the list. His rating as "mediocre" in chemistry will strike the historian, who takes an interest in the methods of education applied to a genius during his formative period, as a very important factor: either the teacher was not efficient or Pasteur had not yet awakened to the great rôle which chemistry was to play in his future life.

Pasteur was in the beginning, and always, a physical chemist, and the first important work of his life was done in stereo-chemistry. This name is applied to the use of a solid diagram for representing the constitution of chemical compounds with which it is concerned. In 1874 LeBel and van't Hoff simultaneously and independently had pointed out that all known compounds, which manifest optical isomerism contain at least one asymmetrical central atom. In most known cases this asymmetrical atom is a carbon atom, but a few cases are now known in which it is a nitrogen atom. Stereo-chemistry is in reality nothing but a thought construction, one of those things that Plato describes as an idea (See Chapter IV) but it was capable of being expanded into a general theory of a constitution of compounds of carbon. The first successful attempt of a theory of this sort was made by van't Hoff in 1878. Tartaric acid is one of the outstanding examples of stereo-chemistry, because of its structure and also because it was upon this acid and its modifications that Pasteur performed the classical experiments which led to the development of methods that are now used in the separation of optical isomeres.

The student of history, unless he is himself capable of becoming enthused at the daring attacks of the human mind upon unknown fields in chemistry, cannot appreciate the great importance of the discovery of Pasteur in making the differentiation of paratartaric or racemic acid into right handed tartaric acid and left handed tartaric acid, which possess equal and opposite rotatory powers, and which neutralize each other when these two acids, on being dissolved in water, combine spontaneously in equal quantities.

This most momentous event in the history of chemistry, which was to open a new world to the chemical mind, is so important that I shall quote the stirring interview with Biot at the Collège de France, where Biot lived (from page 41, edition of 1920, of Vallery Radot's book):

> Biot began by fetching some paratartaric acid. "I have most carefully studied it," he said to Pasteur; "It is absolutely neutral in the presence of polarized light." Some distrust was visible in his gestures and audible in his voice. "I shall bring you everything that is necessary," continued the old man, fetching doses of soda and ammonia. He wanted the salt prepared before his eyes. After pouring the liquid into a crystallizer, Biot took it into a corner of his room to be quite sure that no one would touch it. "I shall let you know

when you are to come back," he said to Pasteur when taking leave of him. Forty-eight hours later some crystals, very small at first, began to form; when there was a sufficient number of them, Pasteur was recalled. Still in Biot's presence, Pasteur withdrew, one by one, the finest crystals and wiped off the mother-liquor adhering to them. He then pointed out to Biot the opposition of their hemihedral character, and divided them into two groups—left and right. "So you affirm," said Biot, "that your righthand crystals will deviate to the right the plane of polarization, and your lefthand ones will deviate it to the left?" "Yes," said Pasteur. "Well, let me do the rest." Biot himself prepared the solutions and then sent again for Pasteur. Biot first placed in the apparatus the solution which should deviate to the left. Having satisfied himself that this deviation actually took place, he took Pasteur's arm and said to him these words, often deservedly quoted: "My dear boy, I have loved Science so much during my life, that this touches my very heart." "It was indeed evident," said Pasteur himself in recalling this interview, "that the strongest light had then been thrown on the cause of the phenomenon of rotatory polarization and hemihedral crystals; a new class of isomeric substances was discovered; the unexpected and until then unexampled constitution of the racemic or paratartaric acid was revealed; in one word a great and unforeseen road was opened to science." Biot now constituted himself the sponsor in scientific matters of his new young friend, and undertook to report upon Pasteur's paper entitled *Researches on the relations which may exist between crystalline form, chemical composition, and the direction of rotatory power*—destined for the Académie des Sciences.

In 1816, Louis Joseph Guy-Lussac, demonstrated that a crystal of potassium-alum, brought into a solution of ammonium-alum, enlarges without changing its form, thus a crystalline body could be formed from superimposed, stratified, heterogeneous particles. He expresses himself in the following manner: "Without doubt the molecules of different kinds of alum possess the same form and identical forces. It is, therefore, a matter of indifference for the growth of a crystal, whether one or another molecule is deposited."

In 1819 Eilhard Mitscherlich discovered isomorphism and declared the occurrence of isomorphic crystals in various bodies and substances was due to an analogous chemical composition in the seemingly different substances. He demonstrated that this phenomenon can be experimentally produced in artificial combination, and that it depends on the physical circumstances under which crystallization occurs.

Mitscherlich, one of the greatest chemists of his time, announced that the tartrates, the paratartrates of sodium and ammonium had the identical composition, the same crystalline form, the same angles in the crystalline condition, the same double refraction and specific weight, and, consequently, the identical inclination of optical axes. Notwithstanding all of these points of similarity, the tartrate, after being dissolved in water, causes the plane of polarized light to rotate whilst the paratartrate does not. Pasteur conceived the idea that all of the chemical and physical conditions of these two substances were not identical, and could not be identical in face of the fact that their action on polarized light was so diagonally opposite. It was this discovery by Pasteur that the paratartrates as prepared by Mitscherlich, contained two different groups of crystals, one of which turned the plane of polarization to the right, the other to the left, that was so epoch making. These two effects neutralized each other when the two different groups of crystals were in the same solution, and apparently, therefore, the paratartrates had no influence on the polarized beam of light.

Pasteur observed that the paratartrates were composed of crystals that were dissymmetrical, that is the image reflected in the mirror could not be superimposed on the crystal itself. This idea Pasteur makes clear by the mirrored image of the hand. The image of the right hand in the mirror cannot be superposed on the hand of which it is a reflection. But the image of the right hand can be superposed on the actual left hand. (As a matter of fact this is not exactly true; the image of the right hand in a mirror can in reality not be superposed on the image of the actual left hand, except that the left hand be turned around so that when the inside of the right hand is reflected in the mirror the outside of the left hand has to be turned towards the mirror in order to effect the superimposition. There is in reality no possible way by which the image of the one right hand as reflected in a mirror could be conceivable as exactly corresponding to the real left hand, corresponding parts occupying corresponding places, unless this left hand is turned around, and then it would not correspond exactly though a superimposition is conceivable, the inside of the reflected right being thought to cover the inside of the left hand).

Pasteur's great discovery became the foundation of the new science of stereo-chemistry, the study of the physico-chemical arrangement of atoms inside the molecule. Pasteur later demonstrated that all mineral substances have a superposable image and are, therefore, not dissymmetrical. All products of living cells, however, are dissymmetrical: that is, all these substances turn the plane of polarized light. He also pointed out that dissymmetry in some way probably indicates an essential manifestation of life energy, and that, therefore, this is a fundamental distinction between organic and inorganic substances, perhaps the only one that has up to the present time stood the test of scientific investigation.

Vallery-Radot, Pasteur's son-in-law, is quoted as having received from Pasteur the instruction that the cause of so great a difference can only be attributed to the molecular forces which operate in the inorganic world, and which are brought to play every day in our laboratories, that these are forces of the symmetrical order; while the forces which are present and active at the moment when a grain sprouts, or an egg develops, and when under the influence of the sun the green matter of the leaves decomposes the carbonic acid of the air and utilizes in diverse ways the carbon of this acid, the hydrogen of the water, and the oxygen of these two products are of the dissymmetrical order, probably depending on some of the grand dissymmetrical cosmic phenomena of our universe.

Duclaux, one of Pasteur's successors as Director of the Pasteur Institute, and a great authority on biological chemistry, wrote a work which might be called "The Mind of the Master, Pasteur" (*L'Histoire d'un esprit,* Paris, 1896). Concerning dissymmetry, he expands Pasteur's conceptions and makes them a little plainer to us. Duclaux says, "A living cell appears to us then, as a laboratory of dissymmetrical forces, a bit of dissymmetrical protoplasm acting under the influence of the sun—that is to say, under the influence of exterior dissymmetrical forces. It presides over actions of very different kinds. It can manufacture, in its turn, new dissymmetrical substances which add to or take away from its energy. It can, for instance, utilize one of the elements of a paratartrate without touching another. It can manufacture crystalline sugar at one moment and consume it at another, laying by stores for itself today and using them tomorrow. In a word, the living cell presents a marvelous plasticity which exerts itself without the slightest disturbance of minimal deviations of forces due to dissymmetrical influence. Ah, if spontaneous generation were only possible. If we could only create living matter, raise up in the midst of inactive mineral material a living cell, then it would be easy for us to understand something more of vital manifestations and to comprehend better the mystery of dissymmetry."

Even at the present day (1926) I feel that we can not yet completely comprehend the vastness of this conception. In my opinion it even extends into the realms of human psychology and into the abysmal depths of the philosophy of the universe. The average cold-blooded mind of the historical student will not sense the meaning of Pasteur's regret that he was not permitted to perfect his researches in Crystallography. This is because of the inability of the average mind to follow up the meaning of a great discovery and all its possible applications to the limit which human understanding permits. Great as were Pasteur's discoveries in other fields, which I shall presently enumerate, I can comprehend that he would have produced even greater results if he had not entered those domains of research which yielded the more pragmatic results, scientific progress and great therapeutic blessings that he gave to the world. Admirable beyond all possibility of human expression as these gifts are and blessed be his name in all Eternity for these gifts to our race, yet to one who understands his colossal insight into the universe of the molecule, they are but part of the great revelations that this stupendous genius would have been capable of giving to the world. At the age of 70 years he said of himself, "If I have a regret it is that I did not follow that route (meaning crystallography and studies of molecule and atom), less rude it seems to me and which would have led to wonderful discovery. A sudden turn threw me into the study of fermentation; fermentation set me at diseases, but I am still inconsolable to think that I have never had the time to go back to my old studies." (Quoted from Tarbell.) What more evidence do we want that Pasteur, whose psychic antennæ were in touch with the infinite as much as ever any human prophet had been, felt convinced that he could penetrate into the unknown further, to the advantage and progress of his race if he had been left to follow his original inclination. He even makes the gigantic effort of applying his studies on crystallography to a conception of the universe. He says, "The Universe is a dissymmetrical whole. I am inclined to think that life, as manifested to us, must be a function of the dissymmetry of the universe or of the consequences that follow in its train. The universe is dissymmetrical; for, conceiving the group of bodies which compose the solar system with their proper movement, before a mirror, we would obtain in the mirror an image not superposable on the reality. Even the motion of solar light is dissymmetrical. A luminous ray never strikes in a straight line. Terrestrial magnetism, the opposition which exists between the north and the south poles of a magnet, the opposition presented to us by positive and negative electricity, are all the resultants of dissymmetrical actions and motion."

When we consider this magnificent flight of the imagination we must remember that it was the imagination of one of the most logically scientific men of the nineteenth century, and in view of the modern conceptions of light contained in the explanations of Einstein, the sentence, "Even the motion of solar light is dissymmetrical, a luminous ray never strikes in a straight line," suggests that Pasteur's idea and Einstein's idea of light may have been identical.

In his effort to find distinctions between living and non-living matter he was led to the study of fermentation. Thomas Willis in the seventeenth century first conceived fermentation as a chemico-mechanic act, and explained that it consisted in communicating the inner chemical movements of the ferment to the fermentable substance. All the physicians and chemists of the eighteenth century expressed themselves in a similar manner, even Mitscherlich and Berzelius, the latter giving the name "catalytic" to this chemico-dynamic effect of ferments. Liebig, 1839, still adhered to this conception and many English physicians assumed that the disease producers were substances in a state of decomposition that had entered the human body and acted as ferments on the blood.

In 1837 Theodor Schwann and Cagniard-Latour discovered the organic nature of yeast independently of each other. Schwann concluded that the yeast cells took the fermentable substance as a medium for their multiplication and growth, and extracted from it the necessary material for this purpose. The substances that remained after this, constituted the material for the products of fermentation. Schwann was the first to place the concept of fermentation on a biological basis, or rather explained it as a form of parasitism in which the yeast cell lived upon the fermentable substance.

An epoch-making polemic arose between the adherents of the two views which was decided in 1857 by Pasteur's investigation. According to his view we must distinguish between two forms of excitants to fermentation: first, the formed or organized ferments, the effect of which is physiologic; second, the unformed animal or mechanical substances (enzyme), the effect of which is purely chemical. The polemic between Liebig and Pasteur resulted in a chemical literature, which from the standpoint of a historian of science, is a classic period in this subject, but even among his own countrymen Pasteur's investigation met with violent opposition (which is discussed by Vallery-Radot on pages 241 and 242 of his work). In one of his addresses before the Academy of Medicine in Paris, Pasteur concluded his speech by an unexpected attack on the pompous etiquette of the Academy's usual manner of procedure. He urged his colleagues to remain within the bounds of scientific discussion instead of making flowery speeches, and the address of M. Poggiale, in which there was considerable sarcasm, provoked him to actual wrath. So we may class Pasteur among those great martyrs of the human race who did not escape the ingratitude of their contemporaries, and who found his hard work opposed by irony and disingenuousness. The heroic self-sacrifice of Pasteur in consuming himself in the service of humanity, in spite of the provoking and unappreciative attitude of his contemporaries, reminds one of a portrait of the Dutch Professor Tulp or Tulpius, by Rembrandt, where the anatomist is portrayed as pointing to a burning candle, the inscription under the picture being *ALIIS INSERVIENDO CONSUMOR.*

In an address by Roux in 1899 at the inauguration of the Pasteur Institute of Lille, he referred to Pasteur's work on Alcoholic Fermentation and its Relation to the Evolution of Bacteriology. In this address the adherents to the enforcement of the prohibition laws in the United States will find little comfort, for this great work, which so furthered our knowledge of bacteriology, is asserted by Roux to have been prompted by a sympathy for the brewers, vintners, and distillers (in a desire to improve both the quality and quantity of their products). Roux described (*Revue Scientific,* 1899, No. 27) how Pasteur, on being appointed first Dean of the Lille Faculty of Sciences, hoped to attract sympathy to the new Faculty by benefiting the alcohol industry of the neighborhood, and how his researches not only revolutionized the theory of fermentation, but enabled the brewer, vintner, and distiller to improve both the quantity and quality of his products. The speaker then showed how Pasteur in explaining fermentation, also explained zymotic diseases. His labors have resulted in the discovery: that while some ferments, such as diastase, the digestive enzymes and the toxins of cholera, diphtheria, and tetanus, are readily separated from the living cells which produce them, others, such as the alcoholic ferment of yeast and the toxin of the tubercle bacillus can only be isolated with the greatest difficulty. It is, for example, only recently that E. Buchner, has succeeded in separating alcoholase (zymase) from yeast by crushing the latter with fine sand, adding infusorial earth, and subjecting the paste to a pressure of 500 atmospheres. From the expressed juice he obtained a powder which splits up sugar almost instantaneously into alcohol and carbon dioxide. Passing to immunity and phagocystosis, phenomena depending on "the science of antitoxins which Behring has revealed to us," Dr. Roux declared that chemistry has annexed bacteriology and will not give it up. The isolation of the alcohol ferment has not overthrown the Pasteurian theorem that "Fermentation is correlative with the life of the yeast plant." To do this we must manufacture alcoholase synthetically and that this is possible is indicated by M. Bertrand's discovery that gluconate of manganese will act as an oxidizing ferment to the juice of the lac tree in the same way as the natural ferment "Laccase." The speaker concluded by insisting upon the practical value of his work to be carried on in the new institute. In a large Prussian color factory he had been taken through a well-appointed laboratory in which more than fifty chemists were at work. "These," said his guide, "are not employed by the firm, but are young doctors from the universities who want to extend their researches. We provide material gratuitously and they make what investigations they please. Their object matters little to us; so long as science progresses we shall always profit by it."

The next great question to be investigated by Pasteur was that of the spontaneous origin of life. It is a question of a biogenesis, meaning Life without pre-existing life, or from inorganic matter alone. This entire thought development may be considered under three historical epochs: first, the period from Aristotle, 325 B. C., to the experiments of Redi in 1668; secondly, from the experiments of Redi to those of Schulze and Schwann in 1836 and 1837; thirdly, the most modern phase extending from the investigations of Pouchet in 1859 to the time of Pasteur, 1862. It serves no scientific or historic purpose to dwell upon the grotesque views of the first period when it was argued that frogs and toads could arise from the mud of ponds and streams through the vivifying action of the sun's rays. The whole subject is interestingly presented in William A. Locy's book "Biology and its Makers" (from page 276 to page 291). It was not until Pasteur, by the most incontrovertible experiment proved that spontaneous generation was a delusion, that one of the practical applications of the outcome of this very long phase in the evolution of a great idea was announced, namely, the system of asepsis first developed by the English surgeon Lister, to wit, that wounds during surgical operation are infected by microbes either in the air or clinging to instruments, or the skin of the patient or the operator, and that by rendering all appliances sterile and by using antiseptic dressings to prevent completely the entrance of bacteria into wounds, was the only way to insure healing without formation of pus.

One of the most epoch-making discoveries of Pasteur which was generally misunderstood, was that of the existence of living things that did not require oxygen for their life. At least it was proven that they could exist in an atmosphere that was devoid of oxygen. They were called, by Pasteur, "Anærobic Bacteria." Later it was discovered, however, that these anærobic germs developed CO_2 from the oxygen which exists in the organic substances upon which they thrive. They, therefore, generate exothermic reactions, that is, such reactions in which energy becomes free in the form of heat. Therefore, the anærobics respire, although in a different way from the remaining living things. It, therefore, cannot be said that they do not breathe; they do use oxygen, but take it out of its combination, whilst the vast majority of living things respire free oxygen. Those which are of the simplest type use the oxygen, directly; those which have a circulation use it indirectly.

There is a great temptation to enter more fully into the details of the various scientific investigations of Pasteur, but in the works that I have already referred to they are given so exhaustively that it would be useless repetition to enter upon them minutely in this chapter. What is more important in this historic study is to point out the manner in which this great mind was lead from one investigation to the other, apparently against his own will, for I have already quoted his own words that he would have much preferred to restrict his investigations to physics and chemistry. We have already spoken of his researches in crystallography, molecular dissymmetry, fermentation, spontaneous generation, and in the quotation from Roux's address at Lille in 1899, I included his pragmatic efforts into the cause of fermentation in relation to the brewing and wine industry.

Before proceeding to his work on the germ theory of disease, it would be an enlightening transition to explain his investigations on the so-called "silkworm disease," which I will quote again from René Vallery-Radot's book on the "Life of Pasteur":

> Pasteur's first investigations of the disease concerned a mysterious malady that affected the silkworm and was ruining the silk industry of France. This disease was first noted to be serious about 1850. When a colony of silkworms were attacked it was useless to try to do anything with them. The only thing for the silk farmers to do was to import from a distance the eggs of an unaffected race of worms. These became infected after several generations and consequently untainted eggs had to be brought from a distance once more. Soon this plague of the silk worm invaded most of the silk growing countries of Europe, and in 1864 the only races of silkworms not infected were those of China and Japan. Great suffering had taken place in many departments of France by the failure of the silk industry, and the most careful investigation failed to reveal any method of combating the disease. Acute observers had been at work and some very suggestive observations on the affected worms had been made, but no solution for the prevention of the disease could be found, or was as far off as ever. In 1863 the French minister of Agriculture formally agreed to pay 500,000 francs (about $100,000) to an Italian investigator who claimed to have discovered a remedy for the disease, if his remedy proved successful. The offer was to no purpose and in 1865 the weight of cocoons of silk had fallen to 4,000,000 kilos. It had formerly been nearly 30,000,000 kilos. This involved a yearly loss of 100,000,000 francs or about $20,000,000.
>
> Pasteur showed that the failure of the silkworm was not due to any one disease, but to two diseases—"pébrine" and "flacherie." These diseases are communicated to the eggs of the worms, so that the young begin life handicapped by the diseases. The crawling of the worms over the leaves and stems makes these liable to communicate the diseases. To prevent the disease it is absolutely necessary to procure healthy eggs and never allow them to come in contact with anything that may have been touched by the diseased worms. If, at the egg-laying period any worms show signs of disease, their eggs are to be rejected. These simple suggestions were the result of rigid experimental demonstration of the spread of the disease from one worm to another, including the demonstration of the microbic causes of the two diseases. These measures of precaution proved most effective, but their introduction was met with opposition. The strain of the work and the worry of controversy brought Pasteur to the brink of the grave by a paralytic stroke. From this stroke he never entirely recovered and was somewhat lame following it. After the

severest symptoms had passed off, he was given the opportunity to make a crucial test of warding off the silkworm diseases at the villa of the French Prince Imperial. The products secured from the silkworms on the estate had, for years, not sufficed to pay for the fresh supplies of eggs obtained from a distance. Pasteur was given full charge of the silk industry on the estate and the sales from the silkworm products at the end of the year gave a net profit of 26,000,000 francs (over $5,000,000). This decisive demonstration effectually ended all opposition.

Numerous biographers have thought that when Pasteur entered upon the applications of his discoveries for the prevention and cure of diseases, and made experiments concerning the pure cultures of disease producing microbes, aiming to attenuate these cultures to a diminished degree of virulence, thereby creating a vaccinating form of the virus, capable of producing a mild attack of the disease, that then he entered upon his chief services to humanity. This conclusion rests upon the dramatic effect of an artificially secured immunity following the injection of this attenuated virus. Great and highly humanitarian as this result of Pasteur's work has been, and from which I do not wish in the slightest to detract, yet I believe that he could have attained to even greater blessings, could he have continued as he himself wished to do—his studies into the molecular constitution of matter. I have the notion that he would have revealed the nature of that undefinable quality of human beings which is called "the constitution." He would have revealed it in a chemical perspective, and this is as important a factor in pathogenesis as is the infecting organism itself. Such an insight into the constitution of the human being has, to the present day, not been revealed to us, for the constitution is the sum of all of those qualities by which the living organism reacts to changes in the environment, as well as to changes within itself and we shall never reach a complete prophylaxis until the knowledge of the constitution has reached a degree of perfection that is at least equal to that of bacterial infection and artificial immunity.

If Pasteur had resumed his studies on dissymmetry and the molecular constitution of matter by opening up the routes to understanding experimental immunity after others had learnt sufficient from him, these future expansions on the knowledge of prevention of diseases would have followed as natural evolutions of the incentives and fruitful ideas that Pasteur had given. (Work of Roux, Behring, Ehrlich, Bordet.) Following Pasteur in the study of acquired or developed immunity to bacterial toxins, we find the most ambitious introduction of purely chemical theories In this field the labors of Pfeiffer, Buchner, Bordet, Ehrlich, and others are preëminent, and in all cases the chemical idea appears as an essential factor. This is peculiarly true of the so-called "side chain theory" of Ehrlich, which still holds the widest interest. Years ago Pasteur introduced the notion of molecular asymmetry into chemical science and pointed out in effect the importance of the conception of configuration in dealing with certain problems. In 1894 during the progress of his famous investigations on the bodies of the sugar group, Emil Fischer published some remarkable papers on the behavior of certain enzymes in the fermentation of sugars, in which he pointed out that in order to work as ferments the enzymes must possess a certain stereo-chemical structure, bearing a definite relation to the stereo-chemical structure of the sugar. Without this relation, fermentation cannot take place. In order to make his meaning plain Fischer employed a figure which has since become famous. He said, in speaking of certain glucosides: "Enzyme and glucoside must fit into each other as a key into a lock in order that the one may be able to exert a chemical action on the other." In one of these papers Fischer suggests that the idea of related molecular configuration of enzyme and fermentable body may prove of value in physiological investigation as well as in chemistry. We have apparently in this prediction of Fischer, made ten years before Ehrlich's theory the basis of the Ehrlich hypothesis.

Without going into minute details, the Ehrlich notion of bacterial or toxin action on the cells of the body, and immunity from the same is briefly this: Bacteria, animal cells, and toxins are all complex aggregations of more or less complex molecules. The latter have certain configurations dependent on the presence of side chains or side groups, to borrow an expression from organic chemistry. These side chains are directly or indirectly the points of attack or defense in the action of the several bodies on each other. In order that a substance may behave as a poison or toxin to cells of the body, both cells and toxins must, therefore, possess certain reciprocal molecular configurations. It has been suggested by Ehrlich that it is through the presence of these side groups that the cells absorb their necessary nutriment and elaborate new structures from it. Some of the side chains may be constructed to combine with fats, some with carbohydrates and some with proteins, but in the presence of toxins or bacteria with the right kind of side chains, combination with these may take place instead. Certain phenomena seem to indicate that this combination is not a direct one, at any rate not always direct, and the conception of an intermediary body or linking complex has been developed. This intermediary body must itself possess two groups with special configurations; one of them fits it to combine with the cell, while the other brings about the combination with the toxic molecule. (Amboceptor.)

The medical historian, J. J. Walsh, who was in Paris during Pasteur's funeral and has a personal knowledge of the Pasteur Institute, because he had taken a course there, gives a graphic and at the same time pathetic description of the public honors and a manifestation of sentiment that were nation wide when Pasteur died in 1895. (See Makers of Modern Medicine by James J. Walsh, article on Pasteur and Modern Medicine, 1895.)

Christian A. Herter in an address on *The Influence of Pasteur in Medical Science,* delivered before the Medical Society of Johns Hopkins University in 1904, stated: "Had Pasteur died in 1868 instead of twenty-seven years later he would have left a name immortal in the annals of science. Others, in some degree, would have developed his ideas. Already inspired by the researches in fermentation, Lister would have continued to develop those life-saving surgical methods which will forever be associated with his name. But we may well question whether investigations in biology and medicine would not have been, for a time at least, conducted along less fruitful paths. Who shall say how soon the great principle of experimental immunity from pathogenic bacteria, the central jewel in the diadem of Pasteur's achievements would have been brought to life?"

My own convictions concerning this question is that great as this achievement is, and no one will hesitate to declare it to be one of the first magnitude, we will have to attach great importance to Pasteur's own views in the matter as expressed in his own words "that he had great regret that he could not follow out the route of his researches in crystallography and physical chemistry." As physicians we cannot see the full meaning of the Master's intentions. This is due to two reasons: one is, that no one but the Master himself could judge of the great vision that his own mind was capable of and we are unable to follow his vision; secondly, as physicians it is pardonable for us to see in experimental immunity just as Christian Herter did "the central jewel in the diadem of Pasteur," the reason for our admiration concerning experimental immunity being that it has given immediate and practical results, and saved many thousands of human lives. We are unable to see as the Master saw that a knowledge of molecular constitution would have led to still greater revelations to those disclosures that are necessary for a knowledge of human constitution. By a curious coincidence I found in a little book on Philosophical Logic (Bernard Bosanquet's "Implications and Linear Inference") a reference to Harvey as an illustration of the author's contention that inference is based on the implications in a system. As I understand the author, the doctrine is not that there is absolutely no such thing as "linear inference," from rule to case, or from case to case, or case to rule (the traditional doctrine about Syllogism-Deduction and Induction respectively) but that such a process is taken up and included in the really significant procedure in deduction, which is made on the basis of the more or less adequately apprehended character of a systematic connection of details, so that, in principle, the alternative becomes "this or nothing": i. e., we must either accept this, or abandon the whole system in which it forms a part. Bosanquet refers to Dr. Pye Smith's article on "Harvey" in the Encyclopædia Britannica as setting forth with special clearness eight points (which Harvey is said to have proved) and remarked that all of these points might have been proved severally, either by direct observation or by inference from others, or enough of them to suggest a system, and that a critical one; the actual circulation (through the capillaries which Harvey never saw), was then a matter of inference. If Bosanquet's logical doctrine is maintained it points, it seems to me, ultimately to a systematic connection of facts, and propositions about facts, in the universe—which would throw light on the psychological doctrine which I suggested in my article on "The Discovery of the Circulation of the Blood," and which is expressed in the quotation from Chapman: "The spiritual development of humanity is also a growth that is governed by life law and that a discovery, although if it was not made by him to whose name it is wont to be attached, yet at the appointed time would have seen the light of the world."

In the historic phase of the evolution of the idea of a blood circulation, —the final developments cover a period from Galen to Harvey from 125 A. D. to Malphigis' discovery of the capillaries and observation of the blood moving through the capillaries in 1661.

All the facts which in the process of discovery have been accumulated would thus tend to throw light on one another, and so also tend to develop insight into the system and as "THE SYSTEM IS PRIOR TO THE DISCOVERY, THE INSIGHT MAY WELL BE REGARDED AS THE SELF-REVELATION OF THE SYSTEM." It would perhaps be too much to say that at a certain point the discovery is inevitable but it at least is intelligible.

When we are dealing with problems of the human mind which, at the present time, cannot be approached by objective experimental methods, but only by the mind itself, we must at once make clear to ourselves that just as, in ordinary objective, biologic experiments, many of them are only tentative, others must necessarily fail, because the way of progress is exceedingly rarely in a straight line, as Pasteur himself has very correctly observed. Therefore, in thought experiments we cannot avoid failures, and must often be satisfied with tentative results. In other words, our efforts in solving the problems of thought by thought must frequently fail. I state this in order that the physicians and surgeons in reading this shall not get a discouraging idea of the philosophy of thought or of the thought method of delving into problems. In the preceding it was stated that a discovery, although if it was not made by him to whose name it is wont to be attached yet at the appointed time would have seen the light of the world. The question might be asked: Why is it that so few great discoveries are made, why is it that in the long reach of human history the breaks of the human mind into the unknown, and the clear explanations of the unknown are so rare? This is of course another one of those thoughts that may not be capable of solution at the present time, but to my mind the reason of the slowness of discovery, and the pragmatic application of the rough forces of nature to the purposes of man, is due to the fact that man is himself a part of nature, and has to undergo a slow reciprocal process of adaptation, not only to the changes in his natural environment, but also to the effects of the changes which he himself has produced. For example: We are at the present time not yet completely adapted to the changes which the invention of the radio, the automobile, the airship, and the submarine and other great innovations have brought about. The alarming death rate of accidents due to the automobile are a most convincing evidence that man has not yet adapted himself to this method of transportation. I use these as an illustration only of the many thousands of changes that are going on all the time, and which call upon our central nervous system for continual training and throwing into use of new and hitherto unemployed neurones, and the new building out of association fibers between these neurones. This requires time and if the discoveries were to follow each other so rapidly and in such striking contrast to that which the human race is accustomed to, the response of the central nervous system, in effecting the process of adaptation, would be inadequate; overtaxation of the central nervous system may result in its disorganization. One can almost recognize this at the present time, namely, that the changes not only in the application of the forces of nature to man's purposes, but the changes in human government and civilization are too rapid for man's central nervous system.

There have been periods in the past history where through great human teachers, like Moses or Socrates, or Christ, the race has been cautioned to make haste slowly, as it is expressed in Eccles, Chapt. IX, verse 11, "The race is not to the swift, nor the battle to the strong, etc., but time and chance happeneth to them all." Such a great teacher is necessary at the present time, for the entire human race is living too fast, and the structure of human civilization may become top heavy and collapse upon itself, because of the disregard of some of the earliest maxims for the automatic protection of the race, at least that part of it which is doing the world's work. It is this part that requires protection because it is the advanced races that are consuming themselves to serve others. *Aliis inserviendo consumor.* By far the largest part of the human race is still in the stages of barbarism, and only the small minority is engaged in doing the work of the world.

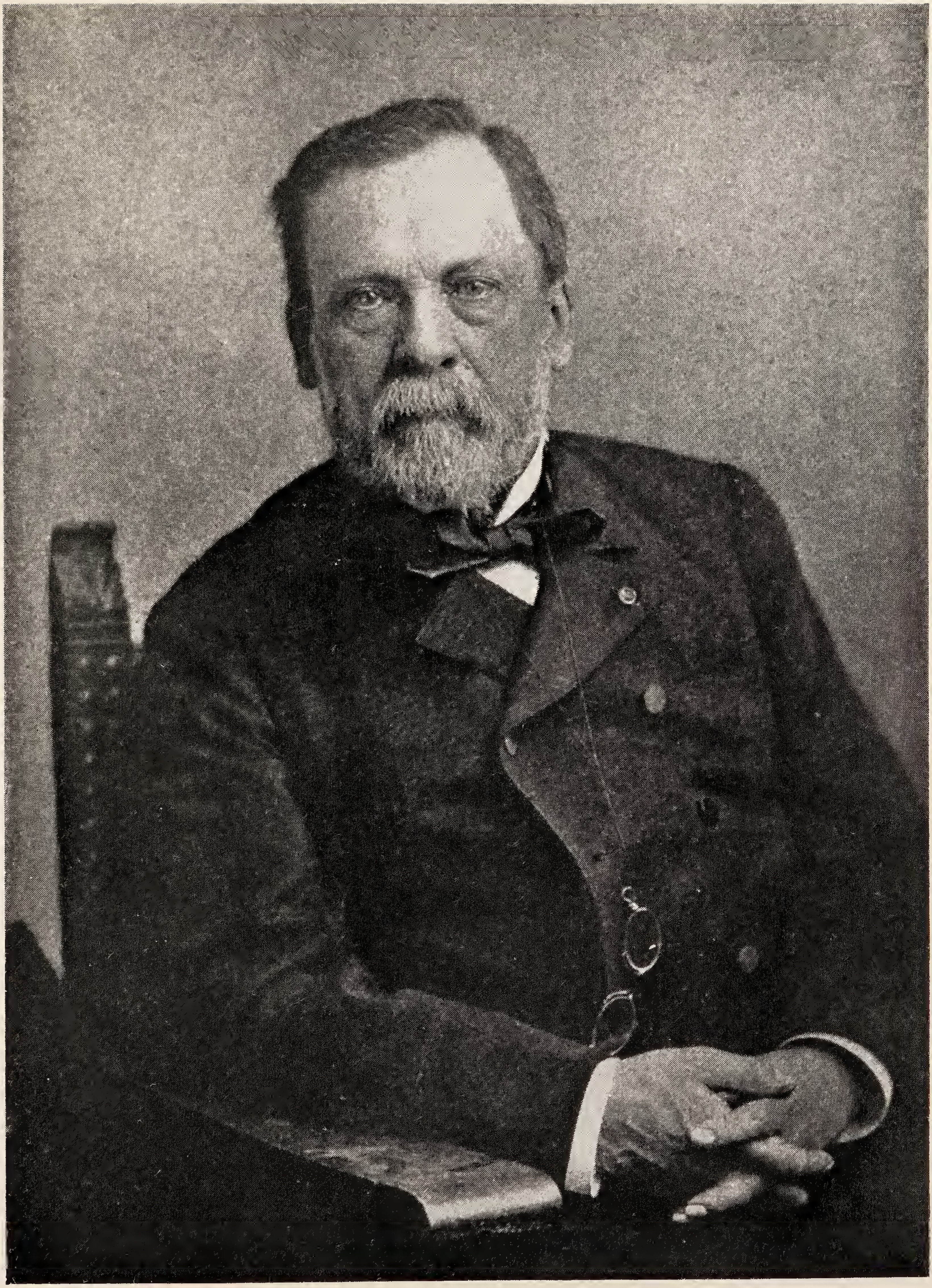
Now Pasteur was one of those who did not merely refrain from sparing himself in the ordinary sense of the term but even gave up his most cherished pursuits and lines of investigation for those that promised the most practical beneficial results to his fellowmen. He never entertained false illusions about the frantic rush toward improvement of one's material condition and accumulation of wealth. In his heart he kept, as within a mighty stronghold, unassailable by the drift of opinion of the outside world, a large reservoir of placid, serene peace. This was his most securely founded refuge. There he could retire whenever he wished and refresh himself from the toils of his scientific labors.

Pasteur's entire life and work is a most exceptional illustration of the overawing peace and repose a great man may preserve in the most trying circumstances, if he has faith in God.

In the preceding I have spoken of Bosanquet's logical doctrine and discussed whether it is identical with Plato's doctrine of ideas, discussed in Chapter IV. I take it that it is a fair interpretation of Plato's doctrine in certain of its aspects. The system *of* and the system *in* the universe which are now dimly and now more clearly revealing themselves to our intelligence, are evidently not brute data or mere particulars; far more than that, they are universals capable of being exhibited in an indefinite number of instances.

Another profound insight into the manner in which the brain of a great Master in Medicine works is the revelation that all of Pasteur's achievements in physical chemistry, biology, and experimental immunity existed as ideas before he carried out the verification by the objective experimental method. It may shock some medical colleagues to be told that Pasteur's molecular dissymmetry was an idea, according to the Platonic system; yet it seems to be so indubitably and the circulation of the blood (see Chapter XI), as eventually verified by Harvey, was also an idea according to the Platonic System. That these ideas come only to minds properly prepared by inheritance and environment is self-evident from the history given in the preceding. That there is also an insurgence from the subconscious region in which our individual lives are rooted, up into the conscious sphere is also unmistakable, particularly so with the great masters of music and biology. See Chapters IV, XXXI, and XXXII.

Pasteur died in 1895, seventy-three years old, greatly honored by the whole world. On Saturday, October 5th of that year, a national funeral



Pasteur

was conducted in the Church of Notre Dame, which was attended by the highest representatives of the State, the scientific bodies and learned Societies of France. In one of his lectures, before the Royal Institution of London (consult Tyndall on Faraday as a Discoverer, 1868; also Thompson's Michael Faraday, His Life and Work), Faraday said: "I do not name God here because I am lecturing on experimental science. But the notion and respect for God comes to my mind by ways as sure as those which lead us to physical truth." This exactly expressed the mental attitude of Pasteur.

At the end of the last century a temple marked the place (at least we could compare it to a temple) where this great man's ashes repose. Upon the walls of the chapel are inscribed the scientific triumphs of this master mind. There is also a triumphal arch in Paris on which are sculptured the names of all the battles of Napoleon I. What a discrepant comparison between the monuments to the greatest Master of Biologic Science which France has produced, and the one to the greatest General. Pasteur saved perhaps as many lives as Napoleon I destroyed, and, if we could calculate in that manner, the balance of destiny as regards progress and degeneracy is on an even beam. But before the judgment of Him whom Pasteur worshiped his life long, there can be no comparison. The chronicle of achievement recorded on the walls of the chapel where Pasteur lies buried, presents the following array:

1848	Molecular Dissymmetry
1857	Fermentation
1862	So-called Spontaneous Generation
1863	Studies in Wine
1865	Diseases of Silkworm
1871	Studies in Beer
1877	Virulent Microbic Diseases
1880	Vaccinating Viruses
1885	Prophylaxis of Rabies

"Estne Dei sedes nisi terra et pontus et aër
Et coelum et virtus? Superos quid quaerimus ultra?
Jupiter est quocunque vides, quocunque moveris."
M. Annaeus Lucanus.—

"Is there a seat of the Divinity unless it be the earth, the sea, the air, the heavens, and virtue? Why, then, do we seek the heavenly beings beyond? Jupiter is wherever you look, wherever you move." (This passage is often quoted as containing a sublime idea of the Deity, though falling from the pen of a pagan.)

The bibliography on the life of this illustrious French biologist, biochemist, bacteriologist and savant is so great that I will have to refer to the index catalogue of the Surgeon-General's Library, but it would be a neglect if I did not call attention to the handsome "Transactions concerning the National Celebration of the Centenary of the Birth of Louis Pasteur, held under the auspices of the University of Pennsylvania in Philadelphia, December 27th, 1922, containing addresses as follows: Dr. Russell H. Chittenden, on "Pasteur in Chemistry"; Dr. Vernon Kellogg, on "Pasteur and Biology"; John B. Deaver, on "Modern Surgery's Debt to Pasteur"; Dr. Hugh S. Cumming, on "Pasteur's Influence on Public Health"; and M. Jules Jusserand, on "Pasteur as a Man among Men." There was another Centenary Celebration of the birthdays of Louis Pasteur and Gregory Mendel at the George Washington University of St. Louis, on which occasion Professor Victor C. Vaughan delivered the address on "Pasteur," and Professor Herbert S. Jennings delivered the one on "Mendel."

In *Medical Life* for December 1922 we have a "Pasteur Number" with essays by Dr. Robert G. Eccles, by Dr. Victor Robinson and others. The issue is abundantly illustrated.

The finest collection of works on Pasteur which I have found is contained in the private library of Dr. Henry Barton Jacobs of Baltimore. I wish to take this occasion to express my thanks to him for the courtesy in extending the privileges of his library to me.

XXIII

HISTORY OF ALCOHOL AS A SUBSTANCE FOR HUMAN CONSUMPTION.[1]

THE word "alcohol" comes from two Arabic terms, *al,* which is the article corresponding to "the," and *kahal,* meaning a finely divided powder.

In the year 2220 before Christ, during the reign of the Chinese Emperor YÜ, the production of wine was in practice, but was only used for religious and sacrificial purposes.

Originally, alcohol was the term applied to any finely divided body, but later became applied to the spirit of wine, after it had been brought to its greatest purification. Kopp, in his "Geschichte der Chemie, 1847," IV Part. p. 271, narrates that with the Arabians and also Spaniards, the word alcohol means antimonium sulphate—and not the C_2H_6O, but that the volatile part of wine was rendered freer from H_2O by placing it in contact with tartrates (Weinsteinsalz) and all salts are called "alcali"—thence was derived the term *spiritus alcalisatus*—which was corrupted into *spiritus alcolisatus,* and this word went over into *alcool spiritus vini.*

The ancients had no intimate knowledge of alcohol as such, although they consumed wine and beer. Their distillation apparatuses were too imperfect. The distillation apparatus was first introduced by the Alexandrians.

In 950 the Arabic physician, Rhazes, described a method of preparing alcohol, but does not mention its combustibility. In 1440 Giovanni Michele Savonarola published a method of testing brandy for the amount of C_2H_6O. In 1784 Lavoisier demonstrated the composition of C_2H_6O. In 1784, Carl Wilh Scheele showed that when C_2H_6O and inorganic acids form ether, this ether can reversely be converted into the acid and C_2H_6O used. In 1796, absolute alcohol was made by Tobias Lowitz with dehydrated (freshly heated) calcium carbonate.

Advocates of alcoholic beverages frequently maintain that the use of alcohol like that of all other stimulants is due to a deep-seated necessity of the human race. Alfred Grotjahn, in the thirteenth volume of the

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[1] Read at the annual meeting of the American Therapeutic Society, June 3, 1921. *Medical Record,* August 13, 1921.

Library of Social Sciences (Bibliothek der Socialwissenschaften), which appeared in 1898, makes the assertion that the desire for alcoholic beverages has for all time been so general with all peoples that the same must be regarded as a common human heritage. Older authors speak in the same strain. Even Pappenheim asserts in his work, "Sanitätspolizei," that alcoholic beverages seem to have been evolved to meet an ever-present requirement of the human race; that we can only hope to do away with inebriety and the stronger alcoholic mixtures.

In contravention to this, Baer states in his standard work: "The human race surely has existed countless ages without using these artificial remedies. Many races exist to-day or have lived recently who developed no feeling of instinctive necessity for these things until they became cognizant of them."

I have made it my business to show that the whole alcoholic question is the product of a very slow process of evolution from a very modest beginning to its present proportions, from a slight inclination to its subsequent abuse.

From prehistoric times to which the historic period compares as a small fraction of time, no clue has come down to us in regard to the use of alcohol. Man could only consume that which nature voluntarily offered him. Very gradually did he develop his customs and his tastes through learning to adapt the forces of nature to his purposes.

As glorious as this victory of consciousness over subconsciousness really was, it brought with it many customs and habits which, being in opposition to the laws of nature, superinduced sickness, misery, and crime. As the prime offender we have the consumption of alcoholic beverages. Their first appearance is enshrouded in mystery. The first mixtures of sweet wines and other fermented beverages with much water had doubtless an agreeable effect, as well as the ensuing mild paralysis, which revealed itself as a heat sensation and a stressing of the emotional propensities of the individual.

Tolstoi maintains that the chief objective of alcohol consumption is the paralysis of the conscience. If primitive people had been aware that the aforesaid phenomena of paralysis were in fact symptoms of poisoning, and had had some knowledge of the concomitant and after effects of same, they would have been still more cautious than they really were with their discovery. Thousands of years after the first production of wine, it was used in small quantities, highly diluted, in conjunction with meals. Among the Israelites, who were the first to produce wine, drunkenness was not known to any extent.

The Israelites preserved their wines in skins and jugs. The German, French, English, Italian, Latin, and Greek words for wine are derived from the Hebrew "yain." Wine was used with meals and for devotional purposes. There was no abuse, and hence Moses saw no necessity to formulate regulations in regard to its consumption. The Semitic peoples, more particularly the Phœnicians, introduced wine to all the Mediterranean nations, of which we shall refer especially to Egypt and to Greece. To be sure, the Egyptians had concocted a drink from barley and bitter herbs and the Greeks had done the same through the fermentation of dates, figs, apples, and honey, but wine rapidly supplanted these.

It is necessary to emphasize that such a thing as drinking apart from meals, and also the existence of inns, was out of the question. The Greeks were the first to use wine not as an adjunct to meals, but at entertainments devoted to sensual enjoyment. They consumed wine greatly diluted with water, while listening to the music of the flute and kithara, while nude female slaves danced and sweet smelling odors permeated the halls. Rough, boisterous conduct at these affairs was regarded as beneath contempt, barbaric. There was no drinking in the open or at political gatherings, gymnasiums, or baths.

Greek emigrants introduced the manufacture of wine into Sicily and Italy. Wine rapidly supplanted the fermented brews of barley and honey of the Romans. Its production thrived everywhere on the peninsula, and it was diluted with sea-water and consumed at meals. At first drinking by women and young men was discountenanced; but very gradually, near the end of the imperial period, women took up drinking. The feeling was about the same as is now that regarding smoking by women.

When intemperance had become a fact, particularly with the well-to-do classes, the first measure against the use of wine was called into existence by Domitian (87–96 A. D.). Domitian forbade the development of new vineyards and ordered the demolition of one-half of those in existence. This regulation remained in force 200 years. It was annulled by the Emperor Probus (276–282 A. D.).

Rome's historians and moralists do not make any references to alcoholic abuses of the middle and lower strata of society. We can assume, therefore, that in the case of the ancient peoples, there was really no alcoholic question. This first sprung into existence when the Romans came in contact with the barbarians, just as happens to-day, when a highly civilized people introduces a narcotic to a savage people. Foreign nations, such as the Scythians, Thracians, Macedonians, Gauls, and the Germanic tribes adopted wine with avidity, drinking it unadulterated. Nearly all terms pertaining to the raising and making of wine may be recognized as Latin derivatives.

Although the Teutons, like the Egyptians, Greeks, and Romans, had a mildly intoxicating drink, derived from honey and called "Meeth," it is, nevertheless, very improbable, at any rate not demonstrable, that they produced the same in large quantities, as the art of husbandry was not developed to any extent with them.

With reference to the frequently cited passage in Tacitus' "De More Germanorum," 22, 23, that Teutonic armies suffered defeats owing to widespread drunkenness, let it be said that these occurrences were not, as is frequently assumed, the result of an inherited trait, but a phenomenon corresponding exactly to the effect of spirituous liquors on American and African indigenous tribes.

Some Teutonic tribes evidently recognized the great danger of the new drink (for which see Cæsar's "De Bello Gallico," Liber 4, Chap. 2). where the Suevi are reported to have instituted measures prohibiting the importation of wine.

After the fall of the empire the barbaric custom of drinking unadulterated wine to the point of intoxication, spread through Europe. Such was the state of affairs during the time of the racial migrations. Christianity might have been an effective check to the growing alcoholism, but unfortunately, several centuries had elapsed since Christ had first offered his doctrines to the world. In His time the drink habit was of such insignificance that there was just as little reason with Him as had been the case with Moses, to agitate against it. With the third religious founder, Mohammed, things were different. In his time the drink habit had reached such proportions that he started the greatest anti-alcohol movement known up to that time, succeeding in eradicating wine growing in all conquered regions.

A peculiar accident would have it that a Mohammedan, that is an Arabian physician, Rhazes succeeded in 950 in distilling brandy from wine, which discovery was to be devoted to healing purposes. They called the new product *al kohol,* that is, "fine extract." It was a very unfortunate incident, in so far as it encouraged to an enormous extent, the abuse of alcohol. Later on, Occidental alchemists took possession of the newly acquired knowledge. After repeated distillations, that is, rectification, a more concentrated spirituous liquor was obtained until finally Basilius Valentinus, a Benedictine monk, succeeded in producing an almost pure alcohol. The belief spread that this wonderful thing, "spiritus vini," was a cure-all remedy for diseases and also a preventive agent against epidemics, such as the pest. Rhazes did not mention the inflammability of alcohol.

Michael Schrick, in the year 1482, at Ulm, offered this precept in his work, entitled "The Burnt-out Waters," published at said time and place: "Whoever drinks one-half spoonful of burnt wine every morning, will never become sick." The price of this "burnt wine," however, was so high at that time that only the well-to-do classes could carry out the suggestion.

In the meantime, wine culture developed northward up to the limits set by the climate itself, that is Central Germany. In North Germany, after the Teutons had developed their knowledge of agriculture, there was produced an alcoholic stimulant from barley, which was called in Anglo-Saxon "bere." This was also done in northern Gaul.

In the twelfth century, John the First of Flanders and Brabant, known as Jan Primus, became such a patron of the brewer's art at Bruges, that he evolved later into Gambrinus, the legendary patron saint of the brewing industry. In the fifteenth century, the art of brewing made a marked advance when it acquired knowledge of the malting process, succeeding in making a much more palatable and stable beer than was known before, particularly in Einsbeck, Naumburg, Hamburg, and Brunswick.

With growing wealth and consequent luxury, debauchery and intemperance became continually worse, so that Emperor Maximilian the First at various diets from 1495 to 1512, took a most decided stand against the drinking habits of the well-to-do classes. About 1600 the first temperance society was founded. The members of this society had to agree not to drink more than fourteen goblets of wine a day.

A new advance in the technique of making spirituous liquors made the latter accessible to the lower social classes. It was the manufacture of brandy directly from corn. This discovery made in the sixteenth century was spread everywhere, into the smallest hamlets, by the soldiers during the Thirty Years' War.

Duke Ernst August, in 1691, expresses his great regret, in an edict, that spirituous liquors which were really intended for purposes of healing had become an instrument of debauchery, and that even in the places where they could be bought, drinking rooms existed in which such debaucheries were carried on.

The wrong assumption that spirituous liquors gave strength and health was largely responsible for the diffusion of the custom. In Hungary, in 1550, miners regularly received apportionments of alcoholic stimulants to increase their efficiency. Soon after this, the beginning of the drink tip, or "Trinkgeld," as it is called, makes its appearance, supplanting the bath tip. In the fifteenth century every village in Germany and Austria had its public bathhouse from which was developed later the barber shop. Apprentices and servants received on Saturdays and on the days preceding holidays, the bath tip. In the course of time the opinion became current that pest and syphilis, which had been imported from France, were transmitted in the bath rooms. Spirituous liquors were regarded as preventives of these dread diseases, and thus the drink tip replaced the bath tip.

As the manufacture of spirituous liquors became cheaper and cheaper, the demand for them increased and in consequence the injurious effects became worse. Perhaps it was necessary to have the whole matter reach such enormous dimensions, as to make the necessity for calling a halt seem more imperative.

In reply to such writers who care to assert that the drinking habit is really the result of an inherent instinct, I shall refer to an assertion of Darwin, to the effect that since an acquired habit becomes hereditary, the similarity between an acquired habit and an instinctive trait is so great that the one can not be distinguished from the other.

To the impartial observer who can follow the development with an unbiased eye, it would appear that this gradually expanded habit was not instinct at all. I believe, all drinking customs, to the contrary notwithstanding, that the opinion which the great Dutch savant Donders expressed in the year 1853, in his work entitled "Foodstuffs," still holds good, to wit, "If from to-day on not a drop of spirituous liquor is consumed, the instinct will become after a few generations dormant, if not entirely eradicated."

This abstention is however a moral act and requires the willing cooperation of the individual. The endeavor to enforce it by law has provoked the "contrast reaction" described in Chapters IV and XXXIII.

XXIV

LEONARDO DA VINCI AS A SCIENTIST[1]

INTRODUCTION

THE relation of a great man to his time, is, as a rule, viewed only from one perspective, namely, that of the individual as bearing upon the human thought and progress of his day. More correctly, it is a reciprocal relation, in which not only the perspective just spoken of must be considered, but also the relation and influence that the social, political, religious, and intellectual standards and conditions of his day may have had upon the great man himself. It is therefore impossible to study any human genius and obtain a correct knowledge of his power, apart from the history of his time.

Scientific history is a narration of human events given connectedly and systematically and arranged according to causal factors. In that endeavor we find the relation of cause and effect to be also most frequently reciprocal. Men make history, and history makes men. How in each instance one follows the other, and how in the same individual while he may be making history, he himself may become the object of the very historic effect that he is producing—this is the object of scrutinizing investigation into original sources bearing upon the life of any great man whom we desire to study.

A knowledge of the history of the period in which the great man lived, is, therefore, indispensable. But if we study the histories of great men that have been written at various epochs after them, especially with regard to one side of the character of any particular genius, we frequently find this character represented in a widely different manner by different historic scholars. This is explained by the fact that each investigator will view the work and influence of the great man he is studing through the spectacles of his own times. It may be realized, therefore, that while the history of any one great man may have been exhaustively written, and written by men of talent, many hundred years ago, this does not mean that a more comprehensive history, and one containing renewed interest may not later on be written by another historiographer.

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[1] *Annals of Medical History,* Spring, 1921.

Moreover, some of the contributions to human welfare and human thought, which great men have made, have required centuries for maturing and bringing about their most telling effect; in fact, the nearness of a great event to its author obscures all correct judgment. For example, we are at the present time, 1926, yet unable to discern clearly the complex international relations in Europe that brought on the great war of 1914; nor can any man living at this date predict what the effect of this war will be on human civilization. I venture to assert that all efforts to give an interpretation of these questions at the present time will, by future historians, be proved to have been only tentative, if not entirely conjectural.

The publication of the *Leonardo Da Vinci, Quaderni D'Anatomia, I–VI, Fogli della Royal Library di Windsor Anatomia e Fis vologia, Publicati da Ove* C. L. Vangensten, A. Fonahn, H Hopstock, by the Swedish house, Jacob Dybwad, Christiania, which began in 1916 and was just completed in 1919, about 425 years after these admirable anatomic studies were made by da Vinci, is an example of the clarifying effect of time on the conceptions of the merits or demerits of the work of any artist or scientific man.

LEONARDO DA VINCI, THE SCIENTIST

In those happy days when a new springtime was breaking upon old Europe, and minds were awakening everywhere from medieval slumber to new life, in the time of the rebirth of the arts and sciences, there arose in various countries learned men and artists whose many-sidedness seems hardly creditable in these days of the universally applied principle of division of labor. Especially in Italy, there were at that time several distinguished men who not only transported their own and future generations to a point of admiration by their prodigious creations in the broad field of fine arts, but who shone also at the same time by their contributions to science. As one of the most universal minds of that memorable epoch and of all time, we turn in wonder to the name of Leonardo da Vinci.

In mentioning Leonardo, we immediately call to mind "The Lord's Supper" and "Mona Lisa." At this place however, we do not propose to consider the celebrated works of art of Leonardo, nor his famous controversy with the mighty Michaelangelo, nor his powerful influence on the young Raphael; in a word, we are not touching upon his importance in the history of art, but endeavoring to get acquainted with another, less well-known, side of this highly gifted genius. For as this master knew how to enrapture with the charm of his portraits of women and men, how to exalt with his figures of the apostles their dignity, so he also was capable of setting up for himself an imperishable memorial in the history of science by his definite solution of difficult problems in physics. We now propose by means of his manuscripts to pursue the scientific activity of this versatile mind. We want to go eavesdropping on the master in his works or at his apparatus while he is tracking the mysterious behavior of the forces of nature, discovering her laws, and with skillful hand making her serviceable to his mighty will.

Leonardo da Vinci, born in 1452, out of wedlock, was the son of "Caterina," a peasant woman, and Ser Piero da Vinci, notary of the signory of Florence, who was married four times in rapid succession, and had eleven legitimate children by the latter two wives. Leonardo grew up in the loving bosom of the family, 24 years before his nine brothers and two sisters and according to the custom of the time, received his first instruction from a humanistic teacher.[1] Leonardo's early manifested predilection for drawing and modeling led to his being put under Andrea del Verocchio, alike goldsmith, sculptor, and painter, but above all a teacher. The latter fact, together with the varied circumstance of Verocchio's profession was probably a deciding factor if not in arousing, at least in fostering a native multifarious talent in this pupil. Like his master, Leonardo was at the furnace one day, at his easel the next. Art was here closely allied with craft and mental endowment alone differentiated the artist from the artisan.[2]

The conditions under which these anatomic drawings arose will be elucidated in the present article on da Vinci, which is intended as an introduction to a second study, in which I shall consider more especially his contributions to physiology and natural philosophy (Chapt. XXVI).

In those days the flourishing commercial and industrial activities in some Italian cities furnished the support to the arts and sciences. Bloody wars were often waged to get possession of some lucrative branch of industry, when the perfecting of methods of production were considered state secrets and their divulgence, treason. It is not strange, therefore, that preëminent minds took a prominent part in the improvement of mechanical appliances. We shall see how Leonardo da Vinci promoted industrial progress; but through it all let us bear in mind from first to last, that Leonardo received the necessary preparation for his varied skill in the school of Verocchio, where in daily activities, both artistic and mechanical, he grew up from boyhood to adolescence.

Nature marked its chosen son not only with unusual mental endowment, but with a captivating personality and presence. In the self-portrait of the master, at the height of manhood, we see his abundant dark hair curling down upon his broad shoulders and a huge black beard falling upon his breast, the whole framing a pale oval countenance. The dignified seriousness of the lofty brow is mitigated by a pair of soulful, friendly looking eyes. A noble figure harmonizes with the intellectual, amiable physiognomy, and his contemporaries pronounced him the handsomest man in all Italy. Blessed with unusual strength, he was a bold swimmer, an audacious rider, an indomitable fencer, and an elegant dancer. In his person, grace and force were in rare union. With the same hand that slid lightly over the strings of a lute, he bent a horseshoe like so much lead; with the same hand that traced the delicate outlines of a sweet woman's face, he restrained the wildest charger in his course. His frank nature and his agreeable intercourse unlocked for him the social circles of Florence. He captivated the women by his fascinating appearance, by his poetic and musical gifts, and not least by his untiring love of dancing; to the merry feasts of his friends he added charm by his graceful pleasantries. Thus in the midst of an exuberant life in the city of the Medici, between times of bustling festivities and times of serious application, Leonardo ripened from youth to manhood. The young man's power of will was marvelous indeed, to enable him to dash away from boisterous merrymaking to studies in mathematics and physics, the pursuit of which requires preëminently a calm and collected state of mind.

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[1] See article Leonardo da Vinci Chap. xxv this vol.: Personality and Psychography.

[2] See Goethe on Jos. Bossi's book concerning Leonardo da Vinci's painting of the Lord's Supper at Milan. Goethe's *Werke,* Grote Edit. *Zur. Kunst.,* xxx, 263.

Already at Florence, Leonardo had gathered about him a considerable circle of disciples and was active not only in following art but also in endeavoring to utilize his knowledge of mechanics. Not finding in Florence the desired field for his talents, at the age of thirty he addressed a letter to Duke Ludovico Moro at Milan. In this letter he fully expatiates on his manifold capabilities for purposes both of war and of peace, mentioning his ability:

1. To build portable bridges, safe from fire and sword and to burn and destroy hostile bridges.

2. To drain moats during a siege, to construct scaling ladders and other apparatus for this purpose.

3. To destroy any tower or fortification not built on rock.

4. To make light transportable cannon for ejecting burning masses, the smoke of which would cause terror, destruction, and confusion among the enemy, (smoke screens).

5. To dig subterranean tunnels to inaccessible places noiselessly, even under rivers.

6. To construct secure and unassailable covered wagons for invading

the enemy's lines with guns and not to be impeded by ever so dense a mass, and behind which the infantry can follow unimperiled. (Tanks.)

7. To cast guns, mortars, and hurling engines, etc.

8. To supplant cannons, where these are impracticable, with other, until now unknown, projecting weapons.

9. To construct fire- and gun-proof ships; finally, powders and inflammables.

10. To compete, in times of peace, with any one in architecture for the erection of public and private buildings and in the building of canals; to execute statutes in marble, bronze, and clay; execute paintings, etc.

In line with the turbulent times, Leonardo put his abilities as military engineer into the foreground. This letter led, however, to Duke Moro's engaging him for the creation of an equestrian statue of Francesco Sforza. This statue was later destroyed by the invading French.

In Milan, Leonardo founded an academy which, as the foremost school of the sciences and fine arts, enjoyed a high reputation even beyond the confines of Italy, and was considered a shining prototype for all similar institutions of Europe in later years. As teacher in this school, Leonardo outlined a number of writings which shared the sad fate of most of the master's works of art. But the sparse fragments at present stored at Milan and at Paris comprise such an astonishing number of valuable observations and such important results of scientific speculation that we are led to believe that we have before us an encyclopedia of the arts and sciences of that time rather than the hasty notes of one single individual. These sheets covered with letters and drawings form, as it were, a diary of his mind; they contain jottings of his ideas as they occurred to him: now the sketch of a charming woman's head and on its margin a sonnet; then, the plan of a church, and beside it an algebraic calculation; at another time the sketch of a machine, accompanied by remarks on the courses of the stars and the structure of the earth.

A part of these writings betrays plainly that they were destined as a syllabus for his lectures in the academy. Another part certainly originated in the tumult of war, when he was on a journey through Umbria and the Romagna in the service of Cæsar Borgia. In his leisure times he used to systematize these various observations and ideas according to content and, as is manifest from numerous references in the extant manuscripts, they consisted of complete treatises subdivided into chapters on such subjects as painting, hydraulic engineering, the motion, impact, and friction of bodies, machinery, and comparative anatomy.

Leonardo's peculiarity of writing from right to left (mirror writing), as he was said to be left-handed, gave his pages, at first glance, the appearance of being covered with illegible hieroglyphics, and this circumstance, together with the lack of understanding of the first possessors as to the value of the material, may have contributed to the fact that their contents became known very slowly and imperfectly. Personally, I believe that Leonardo da Vinci was not simply left-handed—as some of his biographers assert—but ambidextrous. This can be judged from some of the characteristics in da Vinci's sketches in Milan and the Windsor Castle collection. I shall consider the evidence of my view later.

The best known of Leonardo's writings is his treatise on painting. From this we learn that in his artistic creations, he did not follow unconsciously the intuition of his genius; but went to work rather after mature reflection and careful study. According to his opinion, that artist deserves the highest commendation who, as nearly as possible, can present an object on the flat surface of his canvas so that it appears in relief and in perspective. "Only the ignorant man lays the main stress on beauty of color." Accordingly, he aims to initiate his pupils into the secrets of perspective which he calls the steering gear of painting. Even to-day we cannot explain to the beginner the concept of linear perspective more clearly than this is covered in the words of Leonardo: "Take a pane of glass, fix it stationary in vertical position between the eye and the object you wish to draw; step back from the glass two-thirds of your arm's length, hold your head still without moving, close one eye, and draw everything you see through the glass." According to this direction a picture is indeed obtained in the simplest manner and drawn correctly according to the rules of linear perspective. For the realistic representation of an object, however, correct linear proportions will not suffice alone, but color must be graduated according to distance. The remoteness of an object, determines the layer of air that will be interposed between it and the observer; the more remote the object is, the more the colors lose their individuality and the outlines their sharpness and distinctness. Besides establishing the principles of linear perspective, Leonardo was the first to conceive similar principles for air perspective; and Correggio, the much lauded master of *chiaroscuro,* justly calls him his teacher.

How easily Leonardo passes from art to a contemplation of nature in general is proved by the adjoining remark on the influence of intermediary air on color: "The blue color of the sky is likewise due to the altitude of the illuminated layer of air that intervenes between the darkness beyond and the earth. Along the horizon the blue appears lighter in shade than overhead, where the line of vision passes through the smaller masses of air permeated with dense moisture." The influence of yellow artificial light did not escape him. He also knew the phenomenon of contrast, for he expressly emphasizes the fact that every color shows to better advantage at the juncture with an opposite shade, that black appears darker when it adjoints a light color and conversely. He lays great stress on the proper distribution of light and shade, and finally cites the reason why a picture executed as nearly perfectly as possible, according to all the rules of linear and air perspective, and according to illumination and color, can never present the objects in as perfect relief as they appear to our eyes, for, in nature we see an object with two eyes of which each occupies a different position in space. We, therefore, receive two impressions of any object which we fuse mentally into a stereoptic entity.

There had been nothing new in optics since Claudius Ptolemy instituted his investigations on refraction, so Leonardo began his studies in perspective where optics had been left for nearly fifteen hundred years. The term "perspective" must not lead us astray, for the writers of the Middle Ages, from Roger Bacon (1214–1292?) to the time of Johannes Müller, of Königsberg, according to precedence from the ancients, designated by this term all investigations on light in general, whence the term perspective was to them synonymous with optics. Accordingly, in his perspective, Leonardo frequently enters the field of optics in general. Long before Cardan (1530) and Porta (1558) he discovered the camera obscura. The ancients explained vision as the contact, with the objects seen, of rays of light emanating from the eye; but Leonardo deduced the inversion of the image and its diminution from the paths of the rays of light, as is done to-day, and in his ingenious way did not hesitate to utilize this discovery of the camera obscura in explaining the functions of the eye. To convince his pupils of the correctness of his view, he even constructed an artificial eye in accordance with his knowledge of anatomy. This important discovery justifies a division of the honors for having founded physiological optics between the great master and Kepler (1571–1630) who furnished the mathematical proof one hundred years later.

When art gradually stripped the cramping fetters of traditional forms and on all sides a striving toward being true to nature gained the ascendancy, those creative artists that wished to infuse life-like truth into their forms felt it peremptory to know thoroughly the human body, not only according to its external outlines, but also according to its internal organization. The status of anatomy in the Middle Ages was deplorable. The Arabs, to whom the medieval physician owed his meagre scientific knowledge, were forbidden by their religion to open cadavers. The abstracts and commentaries of the Greeks and of Galenus (131–200), to which physicians confined themselves and which had been rendered into barbarous Latin by the so-called Arabists or medieval translators of the medical writings of the Arabs, had to meet the anatomical needs of the physicans for, in the Occident, the opening of cadavers was also for long periods, but not entirely, prohibited during the Middle Ages. Mondino de' Luzzi of Bologna (1326), whose book on anatomy, copied in part literally from the canon of Avicenna, an Arabic physician (980–1036), was in almost exclusive use in all the medical schools of Italy for more than two hundred years, mentions as something quite remarkable that he had dissected three cadavers.[3]

Under these circumstances we are not astonished to find a lack of illustrations true to nature, so important an aid to the student of anatomy. The first attempts were the work of Johannes de Ketham, a German physician, residing in Italy toward the end of the fifteenth century. These illustrations transmitted the traditional errors, and from one such drawing by Magnus Hundt (1501) we are astonished at the bold conceptions of the forms and positions of the human organs which were current among the physicians at the beginning of the sixteenth century.

Accordingly, there existed no such thing as anatomy. But Leonardo was quite the man to create this science. He found a congenial spirit in the person of Marco Antonio della Torre who, as a young man, had founded a school of medicine at Pisa. Not satisfied with the traditional error of Mondino and the animal anatomy of Galenus, Marco Antonio apprenticed himself to nature and dissected numerous cadavers. A series of magnificent anatomical drawings from Leonardo's own hand, the fruits of common study, are an everlasting monument to these two gifted men as creators of illustrative anatomy.*

These drawings, which had disappeared for a long while, have lately been brought to light. A file of Leonardo's manuscripts came into the possession of Charles I of England, probably through Count Arnold, who resided at the court of Ferdinand II as ambassador in 1636. This file, together with the sketches of Hans Holbein (1493–1554), had been stored in a special closet in Kensington Palace. Here it was discovered in the eighteenth century by Dalton, the librarian of George III. The volume contains 235 leaves with 779 drawings executed in various ways with black and red crayon on blue, brown, and red paper. Besides portraits and caricatures, on riding, fencing, and tournament subjects, and illustrations in optics, mechanics, and hydraulics, there are numerous fine crow-quill illustrations of bones, muscles, blood vessels, viscera, brain, eye, and ear executed with utmost fidelity to nature and with the most artistic finish. These illustrations in some respects, especially in the finesse of execution, surpass those famous drawings in Vesalius's anatomy (1538) so long ascribed to Titian. J. Hunter, the most eminent anatomist and physiologist of the eighteenth century, speaks of them in the following terms: *"I consider Leonardo as the best anatomist and physiologist of his time; his teacher and he were the first to know how to rouse the spirit of the study of anatomy."* At Hunter's instigation, Chamberlaine, when publishing some of Leonardo's drawings, included several plates with anatomical illustrations. Knox, also an excellent English anatomist, who later on studied the originals, even declared that the representations which Leonardo made of the valves of the heart, and especially of the semilunar valves of the aorta and of their varying positions, could only be founded on a correct conception of their functions. Accordingly, it is assumed that Leonardo may have known the circulation of the blood one hundred years before Harvey, an idea that has received the approval of Dr. Arnold C. Klebs.[4]

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[3] For a most illuminating article on the beginnings of *Sculpture and Painting as Modes of Anatomical Illustration,* see the contribution by Drs. Fielding H. Garrison and Edward C. Streeter in *Annals of Medical History,* ii, 305.

* Sudhoff regards M. de la Torre as L. da Vinci's teacher in anatomy (Gesch. d. Med. p. 280).

Moreover, Leonardo did not content himself with a knowledge of the defunct body, but extended his investigations also to the physiologic functions of several organs. The results of these studies are preserved in part in his treatise on painting, in which he considers not only the relative conditions of the body at rest but also the conditions of its movements, both voluntary and involuntary. He begins with the flexions and extensions of the several members and explains walking quite in the sense of modern physiology, as a constant falling forwards. Then he expatiates on the influence of occupations on the carriage of man in explaining which muscles are active in lifting, in carrying, in supporting a load, in raising an arm for throwing, or for swinging a club, and how the body remains at equilibrium in the positions variously conditioned by these muscular activities. Leonardo might well have been the first to institute investigations on the statics and mechanics of the human body. He directed his pupils to study, as did the ancients, the various movements of the human body in the fencing room, and he even wrote a scientific treatise on the art of fencing. To grasp correctly the involuntary movements, however, such as are expressive of pain, of joy, of fright, and of other emotions, he advises the young artist to mingle with the common people, among whom the emotions are revealed unrestrained in the facial expression and in the gestures.

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[4] "Leonardo da Vinci—His Scientific Research with Special Reference to his Investigations of the Vascular System." Arnold C. Klebs, *Boston Medical and Surgical Journal,* 1916.

When engaged upon his equestrian statue, he felt impelled to extend his anatomical investigations to the horse, and his efforts to build flying machines for man induced him to delve into the anatomy of birds and their mechanism of flying. Thus he entered upon the field of comparative anatomy.

The following note gives an insight into the method he pursued in studying and teaching anatomy: "I wish to demonstrate the difference between man, the horse, and other animals. I begin with the bones and follow these up with all those muscles that attach themselves to the bones without tendon and then those that are provided at each end, or one end only with a tendon. I am going to give in full detail the anatomy of the leg as far as the hip, and show the various arrangement of muscles, veins, arteries, nerves, tendons, and the bones; the latter must first be sawed through in order to ascertain their thickness." Are we not led in these words to believe that we are attending the lecture of a professor of anatomy of to-day at the dissecting table?

But his interest also turned to the widely differentiated plant world. With the eye of an artist he investigated form, arrangement, and distribution of the leaves and the branches of trees. He sought to probe into conditions necessary to the life of plants, and to demonstrate their specific nurture. He traced back to the quality of the soil the more or less vigorous development of the bark of a tree; he knew that in wet years the annual ring was thicker than in dry years; he explained that the varying distance of the rings from the center was due to the north and south position of the tree. He even conducted experiments on feeding plants artificially through the bark.

Mysticism, which at that time still surrounded alchemy, seems to have kept him aloof from that science. But as military engineer, it behooved him to know about the making of explosives and inflammables, and as bronze founder, about the apportionments used in alloys. He did not disdain to give his pupils exact directions about mixing colors and preparing durable varnishes.

But we are more than ever astonished at Leonardo's keen ardor for pure mathematics and mechanics, which, according to our modern point of view, are so remote from all artistic interests. Numerous calculations and geometric figures on the margins of his sketching sheets attest with what predilection he toiled over the solution of abstract mathematical problems, and both algebra and geometry owe to his efforts valuable enrichment. Mechanics, the master called the paradise of mathematical science.

The magnificent Gothic cathedrals with their heaven aspiring steeples are a most brilliant testimony to the skill of the medieval architect in conquering mechanical difficulties. In many branches of industry there was no dearth of ingenious machines. As architect, Leonardo must have been familiar with the mechanical expedients of his time. With what amazement do we read how, in his youthful ardor, he offered to raise the lofty tower of the Church of St. Giovanni[5] from its foundation and to dig a commodious navigable bed for the torrential Arno. Despite similar wonderful achievements of enterprising men, the whole of medieval mechanics was pursued according to artisan tradition and nobody was concerned about scientific reasons. The understanding of the fundamental problems of mechanics and hydrostatics already solved by Archimedes (287–212 B. C.) had become extinct in the Middle Ages. The credit for having again paved the way to a rational treatment of mechanical problems, and for having stated correct views about simple machines, one hundred years before Stevinus, is incontestably due Leonardo. Penetrating deeper and deeper into the spirit of mechanics, Leonardo discovered the laws of inertia and motion and thus laid the foundation for mathematical physics which later enabled Galileo (1564–1642) and Newton (1642–1727) to investigate the motion of heavenly bodies and to extend our knowledge of phenomena from the earth to the remotest visible stars in immeasurable space.

With veritable zeal the master plunged into the practical application of his mechanical knowledge and gave his inventive mind full rein. Observing that cotton eagerly absorbed moisture, he attached equal weights of cotton and wax to a small balance in order to prognosticate the coming of wet weather; thus the first hygrometer, so important meteorologically, came into existence. Navigators, as late as the fifteenth century, estimated sailing distances by the eye and in most books on navigation the erroneous opinion has been circulated that the log was not in use until at the end of the sixteenth or the beginning of the seventeenth century. The first mention of the log, according to Humboldt, occurs in Magellan's journal in 1521. Since Leonardo in his "Codex Atlanticus" gives an account of such a distance measuring apparatus for ships, it is quite possible that navigators are indebted to his inventive genius for the instrument so important for the knowledge of ocean currents.

Of his many technical inventions may be mentioned the wheelbarrow, ascribed to Pascal (1623–1662, *brouette*) by the French; u-shaped chain links (Vaucanson, 1709–1782); the well-known and much-used spiral door hinge for automatically closing a door; the smoke-jack; looms, spinning jennies, cloth-cutting and cloth-washing machines; well-boring machines; rollers for embossing iron rods; power planers and saws; a file-cutting machine; and a stone saw (still in use at Carara). He is said to have drawn the plans for thirty mills, and sketches have come down to us of suction and force pumps, of water wheels and of hydraulic presses.

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[5] Florence.

In the "Codex Atlanticus" he mentions the important function of the air in burning and in breathing. "Wherever a flame occurs, it creates a current of air; this current serves to increase and maintain the flame. A strong current intensifies the brightness of the flame. Fire unintermittently destroys the air which feeds it, and creates a vacuum if other air cannot follow to fill the same. *As soon as the air is not in proper condition to sustain a flame, no creature on earth can live in the air any more than the flame.*" (A prediction that air contained a gas indispensable to life). See Chapter XXI.

With these facts in mind, he devised the lamp chimney, usually traced back to Philip de Girard (1804). From his knowledge of the physical properties of the air—weight, elasticity, density—emanated a swimming belt, a diving apparatus, various flying machines for man, imitating the flying of birds, and three hundred years before Lenormond's performance (1787), Leonardo[6] had drawn a parachute and given adequate dimensions for safety.

As military engineer he strengthened the walls of fortresses to meet the increased capacity of the guns, and gave complete plans for the construction of ravelins, the laying of mines, and the use of storming machines. He cast cannon of various forms and sizes, built revolving mitrailleuses (machine guns). He calculated not only the capacity of his guns but the path of the projectile, and expounded minutely the difference in effectiveness of stone and lead balls. He constructed a steam cannon, his "architonitrus," a contrivance of copper that, by means of steam with loud roaring, "hurled its projectiles afar."

Besides his treatise on painting, a work on hydraulics has come down to us. The first sections contain the theory of hydraulics and treat of the physical properties of the earth and of water, the formation of the clouds, the laws of equilibrium of liquids, the changing velocity of water in various altitudes, according to the nature and declination of the soil, and, finally, he discusses waves, whirls, and other questions concerning moving water. Long before Newton (1642–1727), Leonardo recognized the nature of wave motion, conceiving the wave as the consequence of an impact under which the water does not leave its place. There is a great similarity, as he says, between the waves of water and the waves caused by the wind across a grain field, which waves we see moving onward while the grain remains stationary. This striking example is still contained in the majority of our books on physics. To prove that the motion of the water progresses vertically and not horizontally, he cast a straw on the ripples caused by a stone falling into the water, and showed that this was raised and lowered by the waves but was not carried onward. "In the same manner," Leonardo continues, "sound waves emanate in circular motion from their origin. Where there is no air and no instrument to cause its motion, there is also no sound." In accordance with this point of view and corresponding to the present status of the science, he attempted to measure the distance of the source of sound by the time required for the sound to reach the ear.

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[6] A model of his flying machine is in the U. S. Army Medical Museum, Washington, D. C.

In the last four sections, the author arrives at the practical application of these theoretical studies, enumerating the causes for the breaking of dams, and proposing procedures for preventing and repairing. Then he speaks of the best process for draining swamps and of covering with fertile humus such areas as are despoiled of their soil by floods. He further explains the best form for a canal with references to the supply and discharge of masses of water and the effect of the latter on the bottom and walls of the canal, and demonstrates how through the form of the bed and the direction of the current, whirlpools, underwashings, and sandbanks are conditioned. The treatise closes with detailed specifications on the irrigation of arid regions, calculations on the amount of water a canal can accommodate in a given time, according to the elevation of the water supply, the rapidity of the current, the canal form, the declination and the size of the several discharge openings, besides other problems concerning the judicious distribution of water.

This replete experience the master gathered largely while building the canal of Martesana. As has been mentioned before, Leonardo, during his sojourn in Florence, intended to make the river Arno, which was impassable on account of its rapids, navigable so that it might conduct the swift stream through the swamps of Val d'Arno to the sea. With the building of the canal he associated the idea of reclaiming the barren plains of Prato and Pistoja by means of the vegetable mud to be gathered in reservoirs for the purpose. According to Pliny, the Etruscans had made use of the vegetable sediment of rivers for such purposes and a similar procedure is chronicled in the Tuscan annals of the twelfth century. But Leonardo was undoubtedly the first to lay down such a plan according to scientific principles. There is an outline of the project in Paris, and Viviani, a pupil of Galileo's, who established the connection between Florence and

Pisa two hundred years later, departed from Leonardo's original specifications to his own disadvantage. Unfortunately, it was not granted Leonardo to execute this grand piece of work in his own country. Ludovico Moro knew better how to exploit the technical commission of confining the unruly Adda to her rocky banks and of conducting her turbulent waters to the walls of Milan. After the master had prepared himself in the idyllic quietude of Vaverola by the study of older canal structures, he entered sturdily upon his arduous task and completed the difficult stretch of the Martesana canal from Brivio to Trezzo in the year 1497. Milan owes in no small part the fertility and the verdant vesture of her fields to the irrigation made possible by these water works, and Leonardo's efficient work became a source not only of imperishable glory to the land that had given him a hospitable reception, but also one of material prosperity to himself.

During the excavation of earth from his canal channels, Leonardo observed the various strata of the earth. The finding of petrified mussel shells and plants incited him to geological studies, the results of which he collected in far-reaching theories about the structure of the earth. The most erroneous opinions obtained at that time concerning the numerous petrifactions on the west slope of the Apennines and in the southern Alps. Did not Scilla as late as 1670 feel constrained to rise against the opinion that fossils originated from the wonderful influence of the stars? Leonardo recognized long before Fracastoro (1517), Bernard Pallissy (1563) and Nil Stenson (1669) that petrifactions were no miracles of nature, but the remains and attests of an era in creation long past and, as it were, in presentiment of a philosophic division of animals according to structure, he even calls conchs "animals which have their bones outside."

The process of petrifaction he presented to himself as follows:

> When turbid river water deposited its sediment in the sea upon the animals which inhabit the coast, these animals were covered with this mud and had to perish under its load from the lack of their accustomed sustenance. As the sea had gradually receded and the saline waters had drained off, the mud petrified and the shells of the molluscs remained filled with petrified mud in the place of the perished animals. Accordingly all such shells occur between two stones, the one that surrounds them, the other that they enclose. Nearly all petrified shells in the rocks of the mountains present the natural shell on the inner side, especially those that were already old enough and were preserved by their hardness; with the younger shells that were largely permeated with mucous substance, only the calcified portions became petrified. Those animals, however, that had their bones under their skin and were enveloped with mud away from the usual river bed, were immediately penetrated by this mud which displaced their muscles and viscera and left only their scattered bones.

> But if anyone should assert that petrified shells have been produced in the mountains by nature under the influence of the stars, how can he explain that such an influence could generate in the same place shells of various kinds and sizes and of different ages? And how could he explain to me the stratified hardened sand at different altitudes in the mountains? This sand was carried there from different regions by river currents and is nothing but shattered stone that has lost its corners by the continued rolling, the numerous jostlings and hurling in the current of the water which has carried it to this place. How can anyone believe that under the influence of the stars the numerous varieties of leaves were fixed and impressed in the rocks of the mountains and algæ, and sea-weeds mingled with crabs and shellfish of the sea were petrified in one mass?
>
> The sea changes the structure of the earth and the molluscs that lived in the mud of the sea testify to the mutations the earth has undergone. Large rivers always carry with them earth which they wash from their bed by friction. This erosion permits us to discern many mollusc banks enveloped in diverse beddings. The molluscs formerly lived in the same place where the seawater stood over them. In the course of time these banks were covered with mud at various depths and the molluscs gradually enclosed as the water receded. To-day these bottoms have grown to the heights of hills and mountains. The rivers eat into them and again uncover the mollusc banks.

Like Cuvier (1773–1838) Leonardo holds that valleys are erosions caused by draining water:

> When a river forms mud mounds or sand banks and then leaves them, the water that has unburdened itself of these masses shows us the manner in which mountains and valleys could gradually have been carved from the terrain that rose from the bottom of the sea nearly full and uniform.

The brilliant discoveries of the Spaniards and the Portuguese occur at the very height of Leonardo's scientific activities. With the universal interest with which the old world followed the bold voyages of the conquistadores it need hardly be mentioned that he too advanced a keen interest toward geographical research. Moreover, the learned geographer, Amerigo Vespucci, was his countryman and personal friend. Even though an unbiased critic pronounced against claiming for Leonardo the authorship of the oldest map of America now in London, nevertheless there are preserved indisputably genuine cartographic drawings by him of Europe and of several ports of Asia Minor bordering on the Mediterranean, besides topographical descriptions of the valley of Chiavenna, Valteline, and others where he had been engaged on hydraulic works. Soon after the discovery of America, referring to St. Augustine, who had denied the possibility of the antipodes, he calls attention to the fact that even highly respected authors can be in the wrong.

From the earth, Leonardo turns his searching gaze toward the starry heavens. Although since the days of the Pythagoreans there existed among the astronomers a vague intuition as to the spherical form of the earth, and conjectures gradually loomed up concerning her suspension in space, her motion, and her attraction, such points of view up to the time of Nicolas de Cuss, Copernicus, and Galileo remained the property of a select few only, and to have enunciated them publicly would have incurred danger, because of the high regard for the church, which clung to the world order of Ptolemy. Leonardo was so convinced of the spherical form of the earth as to believe that fourteen (Italian?) miles out at sea would make it manifest to the naked eye. The bluish glimmer with which the moon's disc becomes visible in the early days of a new moon, Leonardo attributes to the reflection of the sunlight from the ocean and in several instances compares the moon with the earth:

> The earth probably appears to a human being on the moon or on a star like a heavenly body. To man on earth the moon appears exactly as the earth would appear to the inhabitant of the moon. The earth is not situated in the center of the sun's orbit and still less so in the middle of the universe. It is in the midpoint of the elements that have been assigned to it or are dependent upon it.

That teachings of this sort, one hundred years before the sentencing of Galileo, drew down upon their author the opprobrium of being designated as an heretic seems incredible indeed and his enemies did not let the opportunity escape them to make life miserable for him in Milan. This may be one of the reasons which induced him, in 1499, to bid adieu to that city where for ten years he had been respected and had wrought blessings.

If Leonardo had pursued all these various branches of learning in the quiet of a student's life, we could hardly withhold our admiration. But in the midst of balls and public spectacular displays he began his treatises on painting, geometry, and mechanics. While he was busy on one of these pageants for the wedding celebration of Giovanni Galea Sforza and Catherine of Aragon, in 1499, he began his studies in optics, and under his collaboration, the famous work of his friend Paciola "De Divina Proportione" took shape.

At the marriage of Ludovico Moro with Beatrice d'Este, he gave the wedding festivities a setting on a sumptuous scale, decorated the marriage contract with miniatures, and adorned the ducal palace for the young princess. The next year (1500) there was a recurrence of festivities at the marriage of the Emperor Maximilian and Bianca Sforza. On this occasion the equestrian statue, begun ten years earlier, was set up to public view. The next year (1501) he took a trip to Pavia, in order to perfect his knowledge of anatomy with Antonio della Torre. Returning to Milan, he used the following year in the difficult construction of the Martesana canal. In the interim, he found time to build a bath in the palace garden for the youthful princess. He personally selected the red marble for the facings, the white marble for the solium (bath basin), designed the mosaic floor in mythological subjects, and drew with his own hand the hot and cold water bibbs. To this period belong his participation in the erection of the cathedral of Milan and the completion of the magnificent Lord's Supper in the Church of St. Maria della Grazie. His notes record in addition a series of artistic achievements.

The years between 1499 and 1502 he passed with short interruptions at Florence in the company of Paciola and Salai. In the summer of 1502 he was called as military engineer to the court of Cæsar Borgia, the son of Pope Alexander VI. This took him away from his easel where he had worked for four years (not steadily) at the picture of "Mona Lisa," and hurried him to the rampart of a fortress where he pointed the guns upon the line of the attacking foe. His notes show how, in the din of war, he found leisure for scientific consideration. On July 30, 1502, in Umbria, he drew a dove-cot; on August 1, in Pezzano, some agricultural implement; on August 8 he expressed surprise at the harmonious cadence of water pattering in the public fountain at Rimini; on October 11, at Cisena, he sketched the plan of a villa and described a cart of his invention and of other implements for gathering grapes; on September 1 he constructed the bridge of Cesenatico. In Piombino he speculated on the beating of the waves and in Sienna a remarkable bell gave rise to studies in acoustics.

In Sienna the honorable call of the Signory of Florence to decorate the court room reached him, and we soon find the master in contest with Michaelangelo on the famous cartoon of the Battle of Anghiari which was finished in 1507. After taking Geneva, Louis XII of France asked for the services of the master in Milan where, in the friendship of the all-powerful minister, George d'Amboise, four happy years were dedicated to science and art.

At the departure of the French in 1511, Leonardo, together with the two Melzi, Salai, Lorenzo, and Fanfajo went to Rome. Here, in addition to his painting, he was wrapt up in the construction of flying machines. Dissension with Michaelangelo drove him from Rome and, when Francis I, after the battle of Marignan, entered Milan, we find Leonardo on the side of the victorious French king. He accompanied the king to Pavia and to Bologna and served him then and thereafter in elaborating pageants for state and domestic occasions.

In the fall of 1516, already advanced in years, which seem to have made no inroads upon his physical and mental vigor, the master, accepting an invitation of Francis I, went to France. Here he was assigned a permanent residence in the little palace of Cloux (now Clos-Lucé). From this position he accompanied the king on a hunting expedition to Sologne, where he formulated the resolution to construct the canal of Romorantin for the purpose of irrigating this barren region. Hence we find in this period the draft of this canal delineated by the master's own hand to the utmost detail, even to the very flood-gates. This canal was constructed later on by Meda in conformity with Leonardo's original plans. At Cloux, where Leonardo was surrounded by his faithful friends, Salai, Villanis, and Melzi, he passed away, May 2, 1519.

This short sketch of Leonardo's activities gives an insight into a fertility of mind, a superabundance of ideas, and a multifariousness of interest that seem almost incredible to-day. But to appreciate fully the scientific significance of this mighty mind, it is essential to keep in view the condition of science at his time. Humanity could not divest itself of the medieval trend of thought. Not only was there a dearth of material, because the collection and systematization of experience and experiment had been willfully neglected, but there was also a total absence of method, as the fallacious way of thinking under the ban of scholasticism was incapable of providing proper digestion of the results of observation. By the aid of the knowledge treasured in the ancient classics and their beaten paths of philosophical thinking, society gradually awakened and emancipated itself from the fantastic conception of the Middle Ages. Italy, the virgin soil of classical culture, naturally furnished the starting point for the new movement and, although disunited within and politically powerless beyond her confines, she enjoyed during this period of transition the uncontested leadership over the rest of the Occident.

In the very nature of events, literary culture preceded artistic culture. Dante, Petrarca, and Boccacio were followed by Leonardo da Vinci, Michaelangelo and Raphael. Natural science, associated with the name of Galileo, did not join in until later.

With the literature and art of classic antiquity, ancient superstition also revived and entered into a pious pact with Christian mysticism. Up to the sixteenth century humanism claimed the best powers of that highly gifted nation and, during the most flourishing period of Italian art, the phantoms of astrology and alchemy reigned supreme in the realms of natural science. Leonardo was one of the first in the age of the renaissance to undertake a philosophic organization of the empiric knowledge of nature. With comprehensive insight he associated the most heterogenious phenomena under a common viewpoint: Upon the rippling of the water from a falling stone, he founded a theory of sound; from the effects of running water on the forms of his canals, he deducted an ingenious hypothesis on the configuration of the earth under the agency of rivers. Two favorite problems of his time, the squaring of the circle and perpetual motion he combated with scientific reasons.

Long before Bacon (1561–1626), usually pointed to as the father of natural science, Leonardo entered upon the path of induction and made experiments with the avowed purpose of discovering physical facts. And while the (English) Lord Chancellor himself remained unproductive in the field of natural science and could never fully extricate himself from the mysticism of his time, the efforts of Leonardo, who went to work with a more open mind and with superior mathematical knowledge, were crowned with enduring success. Leonardo extols experiment as the foundation of all human knowledge: *"For experiment never deceives, it is only our judgment that goes astray, when we make deductions not inherent in the former."* The reproach of not being able to quote authorities for his statements he met by saying: "It is of greater value to cite experiment, the mistress of all masters, as an author; for he who repeats the assertions of others, plumes himself with foreign feathers." In another place he avers: "There is no certainty in the sciences in which mathematics are not applicable in some form at least or which is not dependent upon it in certain respects."

While Leonardo never took any active part in politics, he did not omit to jot down certain remarks on statecraft in the spirit of his famous countryman, Machiavelli. In addition we find scattered throughout his manuscripts linguistic and poetic attempts and observations on ethics and philosophy.

We do not admire Leonardo's labors in natural science simply because they are the leisure-hour creations of a great artist; they are of momentous importance for the history of the physical contemplation of the universe. For the reasons already stated, they did not come to proper notice even among the specialists. Alexander von Humboldt in his "Cosmos" rendered the genius of Leonardo deserving appreciation when he said: *"The greatest physicist of the fifteenth century, who combined excellent mathematical knowledge with a most admirable insight into nature."*

Leonardo da Vinci was a contemporary of Columbus; he died three years after him. Meteorology occupied the glorified artist as much as hydraulics and optics. He was influential during life through his great works of art and through his inspired conversation, less so through his writings. If the physical views of Leonardo had not remained buried in his manuscripts, the field of observation which the new world offered would already have been scientifically cultivated in many directions before the great epoch of Galileo, Pascal, and Huygens. Like Francis Bacon, and a full century before him, he considered induction the only sure method in natural science. In Leonardo's own words, "We should begin with experiment and by means of this discover the reason."

When Leonardo made his appearance, a deep rooted mysticism still luxuriated in all branches of natural science; but as with mailed fist he shattered the old forms of art and substituted more perfect ones, so he also unfettered the free flight of thought and in clear, unprejudiced thinking out-stripped his contemporaries by centuries. His scientific importance, therefore, lies less in his numerous mechanical inventions in which he simply utilized in his own way what already existed, but rather in his truly inspired, modern way of thinking and deducing through which he correlated his manifold single observations into a unified whole. Since antiquity tendered him no significant advantages in ways and means, Leonardo stands entirely on his own feet in the realm of contemplating nature and in drawing conclusions from her. As in art, so also in science, Leonardo shines forth as one of the most potent autodynamic human elements of the renaissance.

To the student of conditions that produce or may lead to the production of human genius, the works of Francis Galton, Wilhelm Oswald, and De-Candolle may be presumed to be familiar. Not so the statistical and historical researches of Casper L. Redfield of Chicago, Illinois.[7] This investigator gives the evidence that intellectual superiority can neither be produced nor maintained when generations succeed each other rapidly. He also establishes a new biogenetic term, namely "birthrank" by which he means to express a man's inheritance when measured by the age of his father at the time the son was born. It is determined by taking the difference between the dates when the father and when the son was born. This figure is placed behind the name of eminent men in Redfield's collection. For example: Washington (38) means that George Washington was born when his father, Augustine Washington was 38 years old.

Redfield[8] gives the names and parts of the pedigrees of 571 eminent men, in 222 of which the pedigrees are extended for two or more generations. There are 860 individual birthranks averaging 40.70 years and 168 birthranks which are undivided because intermediate dates are unknown, but which average 40.67 years each. Thus the deduction appears justified that these great men were produced by fathers who averaged over 40 years of age at the time when their eminent sons were born.

Redfield further gives proof that intellectual superiority can neither be produced nor maintained when generations succeed each other rapidly, i. e., rapid breeding inevitably and necessarily leads to the production of inferior stock (in man and animals) and slow breeding is essential to the production of superior stock. These conclusions are based on many generations in those animals that are most profitable to mankind, namely, race horses, milk cows, and hunting dogs, but are directly applicable to man, as is at least partially born out by the author's investigations. Notice, for example, the birthranks in such well-recorded families of great men as those of Johann Sebastian Bach and the Bernouilli.

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[7] Redfield, Casper L.: *Dynamic Evolution.* New York: 1915.

[8] Redfield, Casper L., *Great Men and How They Are Produced.* Chicago, 1915.

There is, however, an important difficulty about this method of prognosticating the production of genius, namely, the scarcity of reliable family histories, which become more and more obscure the more remote the birth of the great man is from our own time. This is particularly regrettable when the particular genius was an illegitimate son, as in the case of Leonardo da Vinci. I have found it impossible to ascertain who this peasant woman, "Catarina," alleged to be his mother, really was. To this is added the intentional secretiveness and confusion that accompanies the birth of most illegitimate children and hence the willful falsification or even destruction of any record. It is also not ascertainable with exactness what the ages of Leonardo's parents were at the time of his birth.

Dr. Albert Raibmayr [9] states that artistic disposition, when carried on by the female line, may remain latent for generations and thereby become lost for biographic investigation. If the maternal side is the only representative of an extinct male talent or family, which has become so lowered in human fortunes that every family tradition is effaced, then biographical investigation is futile. Thus, it may appear that the products of a talent or genius, suddenly reappearing as the offspring of a comparatively unknown and untalented mother, appear in history as if "snowed down from heaven." He claims that the genealogy of Leonardo represents not only a mixture of social stations, but also a mixture of nations. That he was a result of national mixture, I state on the evidence of Woltmann [10] who, upon the basis of iconographic evidence, states that Leonardo was the result of a mixture of Germanic and Etruscan blood.[11]

The demonstration of the inheritance of an artistic predisposition is entirely lacking, in the records and works I have thus far quoted. It is also lacking in Felice Turo's genealogy of Leonardo da Vinci.

In Leonardo, the artistic emotional side makes up the most important

[9] Raibmayr, Albert, *Die Entwicklungsgeschichte des Talentes und Genies.*

[10] Woltmann, *Die Germanen und die Renaissance.*

[11] I append Leonardo da Vinci's Tree as far as it is given by Felice Turo in *Leonardo da Vinci e la Sua Scuola,* i. Milano, 1857.

Ser Michele da Vinci, notaro.

Ser Guido, Notaro, Rogava nel 1339.

m. Lottiera di Francesco Beccanugi
Ser Giovanni
(mori a Barcellona)

Ser Piero, squittinato per gli uffici
maggiori nel 1384.

Antonio,
agli squittini del 1433;
m. Lucia, n. 1396

Francesco
n. 1438
alla matricola della Seta, nel 1464;
m. Allessandra, n. 1444

Ser Piero, n. 1430
Notaro della Signoria, 1484;
m. 1. Albieri di Giov. Amadori (1452).
2. Francesca di Ser Giuliano Lanfredini (1465), n. 1450.
3. Lucrezia di Guglielmo Cortigi.
4. Margherita, n. 1458.

Giovanni, Bartolomeo, Guglielmo, Pandolfo, Benedetto, *Leonardo,* Margherita, Domenico, Violante, Lorenzo, Giuliano, Antonio

Pierfrancesco,
detto Pierino
da Vinci, scultore.
m. in Pisa di ventitre
anni.

n. 1530
forse
fu prete.

naturale
n. 1452 da
una certa
Caterina,
che poi sposo
Accattabriga
di Piero di Luca da
Vinci.

autore della
linea che
crediamo
esistente
ancora.

squitti-
nato
nel 1524

n. 1479
notaro
della
Signoria
nel 1515.
m. Alessandra.
di Giov. Dini.

n. 1476

constituent of his inheritance and as feelings and emotions are generally believed to be inherited from the maternal side, we are directed to his mother as the principal donor of his talents. Although his mother is reported to have been a peasant girl, or servant, nevertheless we must seek in her the predominant donor of his artistic genius. That this genius could not have resulted from paternal inheritance is proved by the fact that his father had four other wives—Albieri di Giov. Amadori, Francesca di Ser Giuliano Lanfredini, Margherita and Lucrezia di Guglielmo Cortigi—by whom he had numerous children, nine sons and two daughters, none of which showed any artistic talents.

We know nothing of the genealogy of Leonardo da Vinci's mother, not even her parents, and certainly nothing of her ancestors. All doctrines of inheritance leave us without aid and we can only help ourselves with the hypothesis that such a colossal genius manifesting itself in earliest youth must be assumed to have had an ancestry of long-continued organic breeding, which must have extended into several preceding generations.

Mysterious as Leonardo's mother must remain, the only hypothesis that gives satisfaction is the one that explains her as being the female surviving line of an old extinct masculine line of genius which had sunk into the lower ranks as a consequence of many national and domestic catastrophies. However, all of these inimical forces could not extinguish the germ plasm of that secret family. Leonardo's mother was a descendant of an Etruscan. In this nation numerous talented families occurred throughout antiquity; but this same nation experienced numerous political catastrophies which forced the upper stratum of society down into the lower stratum.

In the ancient archives of Florence there is an old and well-authenticated register which served Signore Dei for investigating Leonardo's genealogy. There is no evidence in his investigation that he was able to procure any documents proving that da Vinci was subsequently declared legitimate, which from various circumstances appears to be extremely probable. This register states that Leonardo da Vinci was seventeen years old when his father was forty; therefore, Leonardo's father was twenty-two or twenty-three years of age when his illegitimate son was born. This is rather a striking exception to the general results of Casper L. Redfield's patient study, which would indicate that great men, in the majority of cases, are the sons of fathers who averaged over forty years of age at the time when their eminent sons were born.[12]

There appears to be a regrettable difference of opinion among Leonardo authorities concerning the age at which Leonardo died. The most reliable figures for birth are 1452, and of the death, 1519. This would make the year 1919 the four-hundredth anniversary of his death, which indeed has already been commemorated by an admirable exhibit of his works at the Boston Public Library on May 2, 1919.

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[12] Redfield, Casper L., "Testing Facts and Theories of Heredity," *Chicago Medical Rec.*, July 20, 1918.

When important events in the life of a great man are stated in such a contradictory manner by various authorities, it behooves the modern critical investigator to subject the records of the lives of these men to a most scrutinizing analysis, to test them against each other by collateral reading, and to look up personally the original documents and registers in the libraries and churches of the cities in which these geniuses spent most of their time.

Just as we find in Plutarch the authority for the biographies of the great men of the ancient Roman periods and Herodotus as an authority for the historic records of antiquity, so we find Vasari handing down to us the lives of the great artists of Italy in a very comprehensive work, which stands in the front rank of historic art literature. Giorgio Vasari had the advantage of contemporaneous information. Most editions of da Vinci's writings are preceded by a short biography, as a rule, copied word for word from Vasari. Another original biographer of Leonardo is Signor Carlo Ammoretti,[13] librarian of the Ambrosian Library at Milan.

In order to test the validity of Vasari's statements, I have compared them with many obtainable writers on art of his day, but perhaps as important an approval as can be desired is furnished by Schlosser [14] in the following words: "The warmth, fidelity and vividness of his presentation assured to them the interest of all times, although individual statements might be subjected to criticism."

Now Vasari states the following, concerning the death of Leonardo:

> At length, seeing himself near death, he confessed himself with much contrition; and although he was unable to stand, he desired his friends and servants to support him, that he might receive the holy sacrament out of bed in a more reverent posture. When fatigued with this exertion, the King came to visit him, and Leonardo, raising himself up in his bed out of respect to his Majesty, began to relate the circumstances of his illness, and the wrongs he had done both to God and man, by not making better use of his talents. In the midst of this conversation he was seized with a paroxysm, which proved to be the messenger of death; on seeing which, the King hastened to assist him, and supported him in his bed, in order to alleviate his sufferings. But

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[13] Amoretti, Carlo, *Trattato della Pittura. Milan,* 1804. This account is found in the introduction entitled *Memorie storiche su la vita, gli Studi, e le Opere di Leonardo da Vinci.*

[14] Schlosser: *Weltgeschichte,* ix., 429.

his divine spirit, knowing he could not receive greater honour, expired in the King's arms in the seventy-fifth year of his age.

In spite of this statement by Vasari, we cannot correctly assign a greater age than sixty-seven years to Leonardo. He died on the second of May, 1519. His goodness of heart is proven by the stipulations of his will, giving to his brothers the money he had deposited in the Hospital of Santa Maria Nuova at Florence. These very brothers had contested his right to be a recipient of the estate of his father.

> There were giants in the earth in those days, and also after that, when the sons of God came in unto the daughters of men and they bore children to them, the same became mighty men which were of old, men of renown.[15]

[15] Genesis vi, 4.

XXV

LEONARDO DA VINCI

PERSONALITY AND PSYCHOGRAPHY[1]

IN his work "On Makers of Men," Dr. Charles J. Whitby asserts that Leonardo da Vinci was a man of "homosexual instinct." He thinks that the dwarfing of life's most costly function was the price exacted for his unprecedented personal endowment. Whitby bases this assumption (for assumption it is) partially on a "fairly close study" of the principal literary remains of the artist which have satisfied him that if there ever lived a sexless being on this earth, one devoid not merely of passion, but even of curiosity about, or interest in, the wide sphere of sexual relations, that being was Leonardo da Vinci. This statement is a regrettable though flagrant sophistry in an otherwise highly interesting and scholarly work.

The "fairly close study" of Leonardo da Vinci's principal literary remains would take a competent master in mathematics, physics, engineering, anatomy, physiology, architecture, painting, sculpture and philosophy approximately twenty or twenty-five years, depending upon the rapidity of mental comprehension of the investigator.

When the "Codex Atlanticus" was brought to me, by an official of the Boston Public Library, it was rolled in by a man on a wagon that was provided with rubber tired wheels, each of the five ponderous volumes measured about three by two feet and four to six inches in thickness. The casual searching of one of these volumes for anatomic drawings that had escaped the Swedish editors, was still incomplete after thirty days of study (not all in Boston).

Then came the "Colossal Tomes" edited by the Swedish firm of Jacob Dybwad of Christiania, which required three years to edit and compile.

Then to think of Leonardo da Vinci's monumental work on "Treatise on Painting" which is as replete with ethical and philosophic reflections as Turner's "Modern Painters," and which is the most important work on art that ever came from an artist. Then the scattered physiologic, psychologic, botanic studies that Botazzi delightfully abstracts in the "Archivio per Antropologia e. la Etnologia," T. 32, page 253. These collected works require such a comprehensive and versatile brain that their adequate evaluation by one man is for the present impossible.

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[1] *Medical Life,* February, 1924.

The great German painter Kaulbach said that in reading Leonardo da Vinci's "Treatise on Painting" he could do nothing but "admire and marvel."

Suppose even that there is no record that Leonardo da Vinci ever loved woman, as Mr. C. Lewis Hind asserts (in his essay on the "Note Books of Leonardo"), this remark is only referable to the Note Books not to his other writings. The question is—must there necessarily be a record of such an intimate and, possibly to the parties concerned, so sanctified a relation? Must a superlatively gifted, strong man, indeed of immense bodily strength, noble bearing, beautiful physique, and astounding dexterity be declared "homosexual" (as Whitby does, l. c., page 376) that is, sexually pathological, simply because there is no transmitted record of Leonardo da Vinci's ideas and sentiments on this subject?

His intimate friends, Salai and Melzi, his pupils, are described as handsome men; was Leonardo for that reason alleged to be homosexual? Or was it because humans of a like psychic and physical structure naturally "are drawn together"?

Vergebens wird die rauhe Hand,
Am Schoenen sich vergreifen
Man kann nur einen Diamant
Mit einem anderen schleifen.

The sentence which Dr. Charles J. Whitby quotes on page 376 (l. c.) from "Il Codice d. L. da Vinci, nella Bibliotheca del principe Trivula, (Beltrami) Milano, 1893 F. 3 (Textes choisis de Leonardo da Vinci page 67.) does not refer to sexual love; the relation there described is one between the one who aspires to Art (the true, the beautiful, the good in Art) and the object of his aspiration. The words, "Work is the first thing which is born of the union," clearly and unmistakably prove this interpretation to be more correct.

The introduction of the memorandum (published by Vizielli) to one of Leonardo da Vinci's anatomical plates, VENEREM OBSERVAMUS SOLAM HOMINIBUS CONVENERE surely does not argue in favor of the singular view Whitby takes that Leonardo da Vinci was a sexless being. Meditate upon that Latin sentence, it is evident that it does not deny, but confirms an interest in sexual relations, but only "to accommodate human beings" i. e., as a fitting conventionality. Excepting in cases of criminal coercion it can hardly be conceived to occur under any other conditions. "Hominibus convenere" may also mean "as a biologic necessity": namely to follow the order "crescite et multiplicamini."

There is one important work full of the rarest drawings by Leonardo da Vinci which bears directly on the subject and which is not mentioned by Dr. Whitby. Only Dr. J. P. Richter of all Leonardo biographers refers to it and gives a reproduction of an imperfect drawing which Richter calls "Der Kampf der Geschlechter." A mutilated copy of this work is in the library of the Peabody Institute of Baltimore under the title "La Mechanisme des fonctions intimes."

Even if Leonardo's life had been immaculately chaste, it would certainly be nobler in the research historian to associate that with a natural celibacy, rather than imply that the splendid specimen of manhood was a "sexless being," thereby suggesting that he was not normal.

There is so much biography that is not historical science, which should be characterized by critical conservatism, broad and searching historic knowledge and an almost fanatical love of truth. Statements should not be repeated, no matter how ancient their derivation and transmission, if they conflict with the general truth of the best sources, or if they cannot be consistently harmonized with the psychology and biology of the man depicted.

Why for example should Cæsar be described as having no sense of smell (Whitby l. c., page 66) simply because on one occasion he ate rancid oil without remarking upon it? This would be insufficient evidence, even if true, for any remark on the rancidity of the oil may have been suppressed by Cæsar although conscious of it, because oil may have been scarce; or is it not possible for him to have been afflicted with a coryza at the time which prevented him from noticing the rancidity?

A similar restriction is applicable to making the red nose of Cromwell (Whitby, page 67) indicative of dyspepsia. I have had abundant experience with digestive disorders and should say that a red nose is a most deceptive sign.

Enough has been said to prove that any assertion of asexuality in Leonardo, as a pathologic trait, is groundless. Perhaps I should not have given so much space to this delicate subject. My excuse is that it stamped this artist-philosopher as a pathologic being, more so than even if he had been the opposite of sexless. In that respect I feel sure we can apply to da Vinci the saying of Terence, "Homo sum, nihil humanum mihi alienum sit."

The interpretation of Leonardo's views concerning the physiology of digestion, absorption, secretions and excretions is made difficult even to Italian scholars of broad experience, because the Italian of Leonardo's day is 447 years older than that of to-day; and the words in themselves were often used with a different meaning from what they are to-day, and the construction of the sentences, the syntax, appear to have undergone a gradual change. I did not rely exclusively upon my own knowledge of Italian, but consulted the best specialists I could find in Rome, Florence, and Milan. In Baltimore my friend, Rev. Mons. Leo P. Manzetti, has been of great assistance to me and I value greatly his help and generous encouragement.

One also has need of a mathematician and physicist to evaluate da Vinci's physiologic conceptions, for he is ever and anon enthused when he can explain a process on a mechanical or mathematical basis or by a formula. When one comes across such mathematical or hydrodynamic symbolism and it does not at once fit into our modern conception, one should hesitate to reject it, part and parcel. In several instances when the proper mathematical reference was found (most often in da Vinci's "Codex Atlanticus" it was discovered that his ideas were not so erroneous as first supposed. What on first reading appeared hopelessly bizarre and phantastic was on patient study found to contain more than "a grain of truth" in it (I mean modern truth). Where such a penetrating mathematic and engineering mind applies itself to the solution of a problem like the filling of the bladder from the kidneys by way of the ureters, it is most likely to arrive at least at a partial conception of the actual physiologic performances of these organs.

The main difficulty is within us—we could not combine in ourselves sufficient linguistic and mathematical proficiency to exhaust his manner of argumentation and demonstration. One can see for example how the diverse powers of the universal scientific genius, that Leonardo da Vinci was, illustrate each other, reciprocally coöperating in the great Florentine savant. The physiologist becomes complete in the ingenious hydrographic engineer who wrote a long treatise on hydraulics (in "Codex Atlanticus"). Of this fertile union was born the theory of the ureter entering the bladder and functioning according to old principles of hydraulics, a theory perfectly exact (so Verdier thinks) and which the entirely modern opinion of the peristaltic contractions of the ureter has but slightly advanced beyond the more recent principle of hydraulic pressure (Henri Verdier, La France Medicale, 1913, page 241).

To those who are intensely interested in scientific, medical Historiography, as well as to those who are interested in the literature of general medicine and surgery, it must have occurred that there are numerous writers who have an incurable itch or propensity to become authors but lack the maturer qualifications, as Juvenal states it: "Tenet Insanabile

Multos Scribendi Cacœthes." This is particularly noticeable when a man conceives himself to be the father of a new method in any of the departments mentioned above, but especially of a new method in psychoanalysis; he is then very apt to become infatuated with his own method and likely to apply it to cases and under conditions to which it has no possible relation.

Such is the case with the article of Sigm. Freud entitled "Eine Kindheitserinnerung des Leonardo da Vinci." Before I enter upon the psychoanalysis of this psychoanalyst, I wish to emphasize that I am convinced of the originality of his contributions to psychopathology and that they are of great value. They have met with the approval for instance of Prof. E. Bleuler in his "Das Autistisch-Undiziplinierte Denken in der Medizin and seine Überwindung," page 53, but when Freud bases his condemning conclusions concerning Leonardo da Vinci upon the hazy reminiscence of a baby lying in his cradle, he leaves the solid ground of psychologic investigation and loses his way in the meshes of speculation. In the article quoted Freud pretends to believe more than can be proven and his conceptions of the development of sexuality cannot stand the test of objective investigation.

Many a supposed psychic connection is guessed at more intuitively than proven objectively. Rieger states that almost always when someone writes about sexuality there is confusion and error on account of the secret inner condition of the subject matter and also because the author almost always writes in a secretive and confused manner. Dritter Bericht aus der Psychiatrischen Klinik der Universität Würzberg. Kabitsch Würzburg, 1910 S. 62.

From my article in the *Annals of Medical History,* volume III, page 26, "Leonardo da Vinci as a Scientist," see Chapter XXIV it can be learned that he had experimented with flying machines and that a model of one of his aëroplanes is exhibited in the U. S. Army Museum, Washington, D. C. In his "Codex Atlanticus," F 65 V. (Scognamiglio) he describes the flight of the vulture and then comes a sentence which has proved so fatuous for Freud. Leonardo says: IT APPEARS THAT ALREADY PREVIOUSLY, IT WAS DESTINED THAT I SHOULD OCCUPY MYSELF VERY THOROUGHLY WITH THE VULTURE, FOR IT COMES INTO MY MIND AS AN EXTREMELY EARLY REMINISCENCE, SINCE I WAS STILL A BABY IN THE CRADLE, THAT A VULTURE CAME DOWN TO ME AND OPENED MY MOUTH WITH HIS TAIL PUSHING AGAINST MY LIPS MANY TIMES WITH HIS TAIL.

Freud himself expresses scruples as to the possibility of reminiscence of events occurring during babyhood when the human being was in the cradle. He speaks of it as "kann aber keineswegs als gesichert gelten." He says it sounds improbable and like a fairy story and adds that the vulture scene is not a reminiscence of Leonardo's but a "Phantasie" which he formed later in life and transposed into the time of his childhood. Now the word "Phantasie" (fantasy) comes from Φαντάςειν which means to render visible (meaning something that exists only in the imagination), hence phantastic means something unreal and irrational. It would appear then that the entire fabric of Freud's article is based on something which is not real and Havelock Ellis has suggested that this reminiscence of Leonardo could be based possibly on something real after all. He argues that the big bird need not have been a vulture. The mother may have seen the visit of a great bird and may have repeated it to the child in after years so that the child readily confused the narration of the mother with a personal experience of his own but Freud cannot reconcile himself to this view. He seeks for a secret motive and uses a strange metaphor which is very surprising in a psychologist of his fame.

At the bottom of p. 22 he speaks of "die reale Nichtigkeit hervorzu holen." To fully appreciate this curious expression it is necessary to point out the meaning of the word "Nichtigkeit." It means non-reality or nothingness, in other words, Freud actually states he wants to bring out the real non-reality!! Now such an expression as this may appear to be perfectly clear language to a psychoanalyist and we will, therefore, take him at his word. His problem would then be to dig out the "real non-reality" of what Leonardo transposed into his baby existence from a "Phantasie" conceived in his later life, and throughout his book Freud does not let go the expression "Geier-fantasie." From this he develops a speculation concerning an unnatural love that is assumed to have sprung up between Leonardo and his mother. The German word for mother is *Mutter.* As an illustration, to demonstrate how far he reaches to prove his point he goes back to the *Hieroglyphica,* 1–11 of Horapollo. Here it is stated that the ancient Egyptians depicted the Mother by the image of a vulture and that they adored a Goddess of Motherhood who was represented as a female with the head of a vulture and the name of this Goddess was *Mut;* now from this word he concludes that the German word *Mutter* might have had more than an accidental relationship to the Egyptian Mother Goddess. What connection this should have with the hypothetical "Phantasie" of Leonardo is not quite clear, for the deciphering of the hieroglyphics was not a success until François Champollion (1790–1832) made it possible. Leonardo da Vinci's life extended from 1452 to 1519 but Freud speculates that Leonardo may have, nevertheless been familiar with this Egyptology because he was such an omnivorous reader and must have known Strabo, Plutarch, Ammianus Marcellinus.

The further story of how Leonardo came to develop his "Vulture Phantasie" is a pure hypothesis based on nothing but assumption. It is incredible that an anatomist like Leonardo, who in the publication of the "anatomic contributions of Leonardo da Vinci" evinces a knowledge of anatomy amazing for his time and who in my opinion also possessed a knowledge of physiology equally astonishing for his time, could have believed such a wild tale as the self-fertilization of vultures from the wind. All vultures were supposed to be of the female sex. Such a story would have impressed a man of such colossal intellect as too stupid for acceptance for he was even aware of and depicts the different intrauterine positions of the embryo, but Freud goes on to speculate that inasmuch as Leonardo was an illegitimate child he missed his father and therefore his mother had to replace everything for him. Thus in a sense he was also a vulture descendant, thus the birth of Leonardo is construed to harmonize with the fable about the vulture. He is made to compare himself with a "Geierkind." For this there is not the slightest basis in any of Leonardo's statements.

According to authentic documents the father of Leonardo da Vinci married the noble Donna Albieri di Giov. Armnadoni in 1452, the same year that Leonardo da Vinci was born, and his legal acceptance as a member of the family, i. e. of the union his father Ser Piero da V. and Donna Albieri and his being received into his grandfather's house is documented in 1457, when Leonardo was five years old. This legal adoption was caused by the fact that this first marriage of Ser Piero had remained childless during these five years. Freud attributes to Leonardo such a phenomenal precocity that he began to ponder over the riddle of derivation of children and the relation of the father to their origin. In other words it is suggested that before the fifth year of life, the child Leonardo was an investigator into one of the profoundest problems of human existence.

We are asked to believe that at this early age this child was not only conscious of the anomaly of his childhood, but also that it produced two extraordinary tendencies, first an infatuation for his own mother and secondly a research into the problem of aviation. It is necessary to add that Freud of course cannot understand the infatuation of the child for his mother as self-evident and natural, for such an idea would not fit into his fatuous trend and devious scheme; it is only a morbid infatuation that Freud can conceive of. Concerning the research into the mechanics and statics of aviation which Freud deduces from this infantile sexual research (infantile Sexualforschung), it must be said that none but the highly imaginative thinking of Freud could hatch out such a phantasy as for example (p. 32, III, l. c.) that the "coda" or tail of the vulture referred to in the child reminiscence of Leonardo da Vinci was nothing else but the phallus. (Nota bene—a phallus in an animal which tradition asserts to be of exclusively female character.) On pages 36–40 of his monograph, Freud loses himself more and more in speculations aiming to prove Leonardo da Vinci's homosexuality, all of which are based on nothing but phantastic enlargements of the already hypothetic vulture phantasy quoted from the child dream narrated from the "Codex Atlanticus."

All of his deductions that are founded on the romances of Mereschkowski are untrustworthy (Freud, l. c. p. 45) and the narrative of this highly speculative mind cannot be verified in any of the collateral or contemporaneous documentary literature of Leonardo da Vinci's period. So much so that I excluded any references to the novelistic enterprise of Mereschkowski in my previous study of Leonardo da Vinci.[2]

In the fourth chapter of Freud's Monograph, pages 47 to 68 l. c., he attempts to demonstrate that the obsession of Leonardo da Vinci by his childhood vulture phantasy compelled this great artist and scientist to perpetuate this obsession in two of his most famous paintings. For this purpose Freud gives an intensive analysis of the expression of the face in his Mona Lisa. Whoever has seen either the original of the painting itself in the Louvre in Paris, or a good reproduction of it, cannot fail to have been struck by the remarkable, bewitching, enigmatic, and subtle smile[3] on and around the lips of this female figure and one can notice it in almost all of the female figures which Leonardo has painted, particularly that of St. Anne which represents really St. Mary together with St. Anne and the Child on the same canvas. St. Anne, the mother of Mary and really the Grandmother of the Christ Child should have been painted as a Matron of more mature and earnest features than those of St. Mary. As a matter of fact she is represented as still a young woman of unfaded beauty and here is where Freud again indulges in fanciful speculation. Both women in the picture of St. Anne are given the blissful smile of the happiness of motherhood. Muther explains "Geschichte der Malerei," Bd. I, p. 14 that Leonardo could not bring himself to paint wrinkles and folds and therefore he gave St. Anne that same radiating beauty which he bestowed upon St. Mary.

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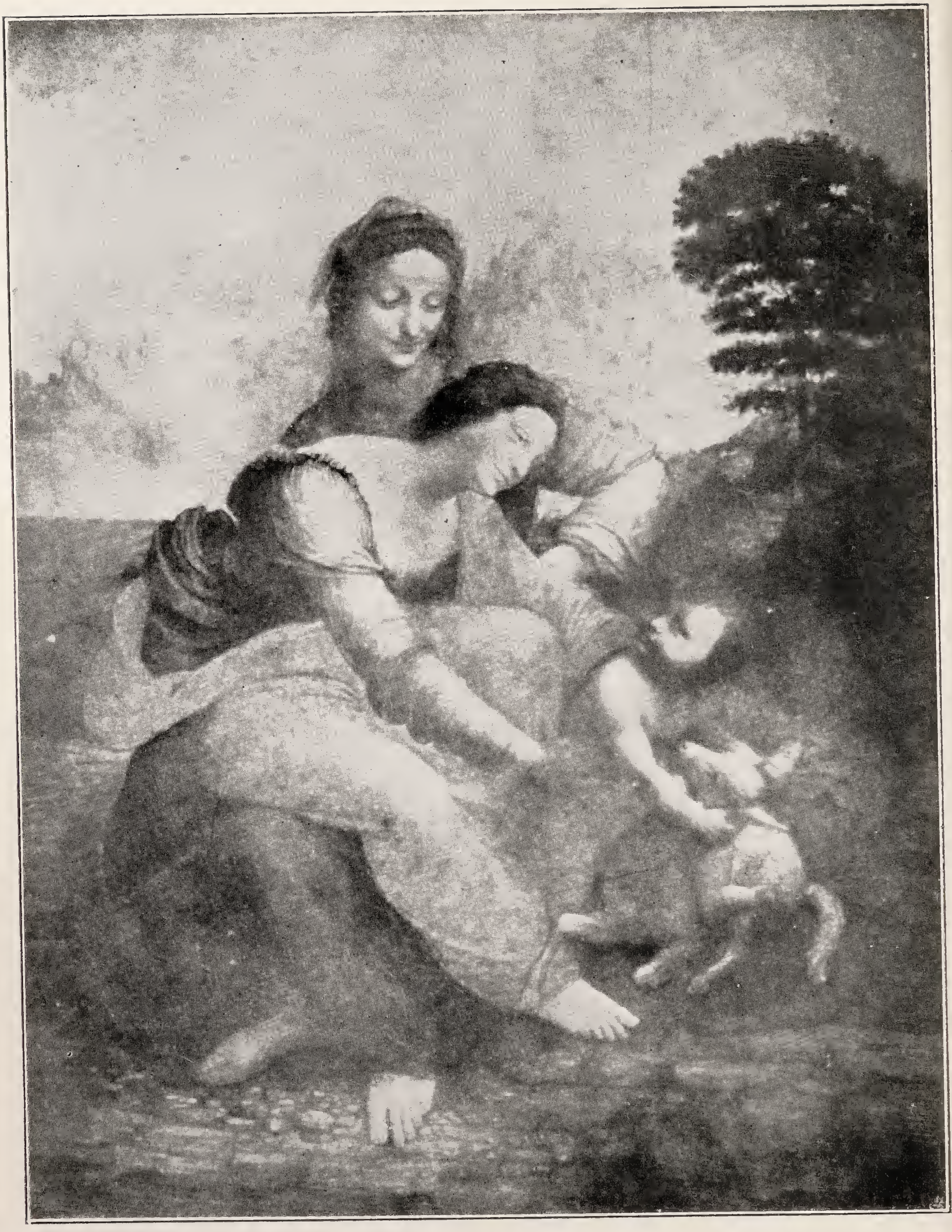
[2] Mereschkowski: Leonardo da Vinci, Ein biographischer Roman aus dem Ende des XV Jahrhunderts, Deutsch von C. von Gülschow, Leipzig, 1903. Dimitri Mereschkowski's romance has been translated into English by H. Trench: "The Romance of Leonardo da Vinci"—*The Forerunner,* New York and London, Putnams, 1902.

[3] This unfathomable smile has been designated as "leonardesque" and it is presumed to have been the smile of his mother Catarina. My impression is that Leonardo provoked it himself by the intense study of the pupils of Mona Lisa which the latter misinterpreted as due to uncontrolled admiration. In other words Leonardo faithfully portrayed a woman's ironic psychic confusion, caused by his study of her pupils (reaction to light and darkness).

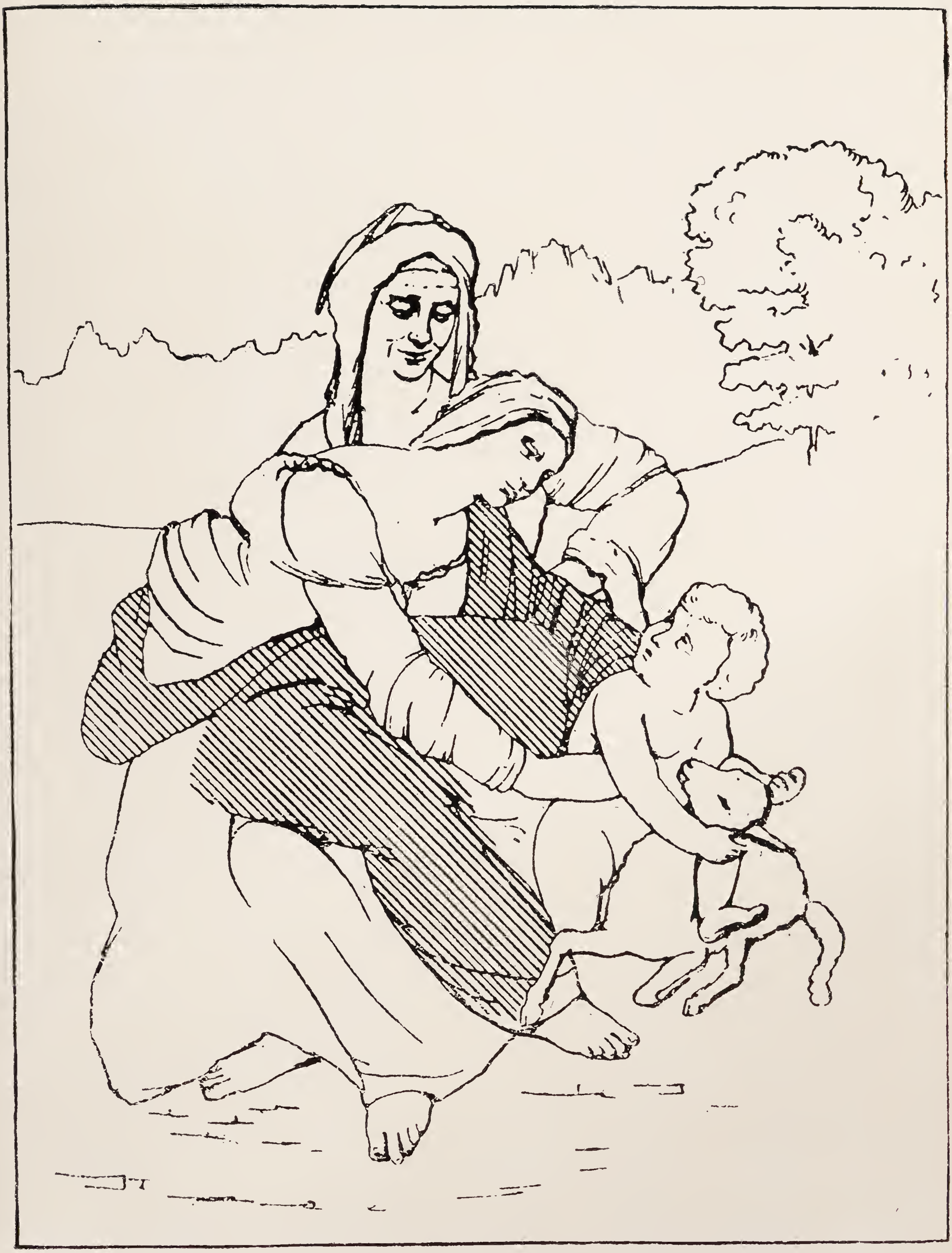
This is not an unreasonable idea but Freud will not let it stand. He would rather continue his hypothesis which he spins out through the assumption that Leonardo knew that he had two mothers, his true mother Catarina and his step-mother Donna Albiera, the first legal wife of his father Ser Piero and in drawing these two female figures of his infancy together he condensed them to a unity in the composition of St. Anne. St. Anne, the Grandmother of the Christ Child, with her left arm steadied on her hip in this painting, bears in her lap St. Mary; the Grandmother is conceived to be the image of Leonardo's genuine mother Catarina and the smile on her face is assumed to be a histrionic smile with which she attempts to disguise the jealousy which the unfortunate real mother must have felt toward her nobler rival. On p. 53, l. c. Freud following A. Konstantinowa and W. H. Pater thinks this smile has lost its subtlety and expresses intense bliss; on p. 54 he thinks this smile covers up a jealousy; truly a most versatile smile or a highly imaginative interpreter.

But now comes the most striking indulgence in speculative *Kryptography* which really reduces this magnificent piece of art to the low level of a puzzle picture. In order to give strength to the combination of the two mother idea with the Egyptian mythology of the Goddess Mut, the divinity of Motherhood, who wears the head of a vulture, Freud draws upon the pseudo discovery of Oskar Pfister, of the delineation of a vulture in the folds of the robe encircling the legs and hips of St. Anne. The contours of the vulture are shaded in a special drawing reproduced on p. 54 of Freud's article. He literally says, quoting from Pfister "Auf dem Bilde, das die Mutter, des Künstlers darstellt, findet sich nämlich in voller Deutlichkeit der Geier, das Symbol der Mütterlichkeit." Pfister also adds that the contours of this vulture which end in the tail feathers, are directed toward the mouth of the child exactly as represented in the childhood dream expressed by Leonardo. In the other work of Leonardo, the Mona Lisa del Gioconda, the subtle smile is also interpreted as a reminiscence of his mother during the first years of his childhood. Since that time all pictures of Madonnas bore the rare, blissful, subtle smile of the poor peasant girl Catarina, who had given the world this glorious son, destined to be an artist, an investigator, and a sufferer. But more than this, we are asked to believe that the ungratified love of his real mother expressed itself in such an exaggerated love for the little son as to lead



THE ST. ANNE GROUP

By Leonardo da Vinci, Containing the Supposed Cryptic Vulture in the Drapery



Oskar Pfister's Diagram

Of Leonardo's St. Anne Group, the Supposed Vulture Being Shown in the Shaded Portion. It Is Reproduced on page 54 of Freud's Monograph on Leonardo da Vinci.

to a premature development of erotism and deprived him of real manhood.

The first legitimate son was born to Leonardo da Vinci's father, Ser Piero in 1476; at this time Leonardo da Vinci was twenty-four years old. This legitimate son was born by the third wife. When Ser Piero was fifty years old he married a fourth wife to whom were born nine sons and two daughters (see genealogy in Hemmeter's "Leonardo da Vinci as a Scientist"—Annals of Medical History, l. c.).

Not one of these many children is on record as having given evidence of the slightest talent in any art or science, and it is therefore natural to look to Leonardo da Vinci's mother as the transmitter of the great gifts. Some have doubted whether Ser Piero was his real father and suggested that he assumed parentage to conceal the one who really was the sire of Leonardo da Vinci, some one in very high ecclesiastic position, it has been suggested. But nowhere in the authentic documents is any force lent to such a supposition, except this: Leonardo has left two self-portraits, executed with all the scrupulous regard for fine detail that is characteristic of him; there are also preserved at least three paintings and drawings of men of the noble house of Vinci in the Val d'Arno. One would expect to find some physiognomic resemblance between the self-portrait of Leonardo da Vinci and these Vinci family pictures; such, however, is not the case.

The greatest service that geneologists can render this profound problem of the genetics of genius is to ascertain who the maternal grandfather of Leonardo was. *Who was the father of Catarina?*[4] The entire documentary evidence suggests to me a case of crossed inheritance.

To return to the vulture-phantasy and its persistent forced idea (Zwangsvorstellung) even in the artist of mature age, I must emphasize that after Leonardo was received into the home and family of Ser Piero in 1457 (quoted from Scognamiglio—Richerche Documenti culla giovinezza di Leonardo da Vinci; Napoli, 1900), it was impossible for his natural mother to exert any influence over him—for she was excluded from this family. Therefore all such influence could only have been exerted prior to his fifth year and Freud expects us to believe that these infantile five years were employed in perverting a baby boy's mind to homosexuality.

As all function of generation is at the base a chemical process of the external and internal secretion of the gonads, it becomes impossible to understand how any influence of this kind can be exerted before the functions of the gonads are physiologically developed.

Psychoanalysts, like Freud, may assert that incipient physiologic function of the gonads may in rare cases occur before the fifth year, but they will find few among those who are technically trained physiologists and biologists, who will believe this.

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[4] See "The Father of Leonardo da Vinci," by Casper L. Redfield, *Medical Life*, December, 1924.

Therefore we may set aside such a hypothetical influence of Catarina on her son as not authentic; in confirmation of this aspect of the Leonardo da Vinci family situation, I must refer to another fact that Catarina was married to another inhabitant of Vinci soon after Leonardo's legal adoption by his father Ser Piero. And as the latter had no other children until Leonardo was twenty-four years old, he had more time than most fathers to devote to his only boy. So that Freud's theories that this boy was "exposed to exclusive female influence" (weiblichen Einfluss preisgegeben, p. 39, l. c.), so that Leonardo became enslaved in a fixation of love thirst for his mother, are not substantiated by historic investigation.

Concerning Freud's conception of the pathogenesis of homosexuality (pp. 38 and 39, l. c.) which he bases on studies by I. Sadger, W. Steckel, and S. Ferenczi, we may say that they should be disregarded in Leonardo da Vinci's case, because none of the antecedent conditions there described can be proven to have occurred in Leonardo da Vinci's case.

It is not even proven that the baby Leonardo was left in his mother's exclusive company prior to the legal adoption by Ser Piero. His mother's early marriage to another man in the same town speaks strongly against it and there is a dependable account that narrates of little Leonardo being placed in the care of his grandparents at Vinci before 1547, very probably when the boy was only between two to three years old (see Encyclopedia Americana vol. 28, p. 98—da Vinci).

The more thoroughly one studies the surrounding conditions of Leonardo's childhood, the more the opportunity for fixation of an infatuation for his mother of the type that Freud describes fades into nothing. All the dependable facts of his childhood point to the very opposite relation, namely that Leonardo da Vinci had less of direct contact with his mother than most other children. It is also incorrect to state that after his legal adoption into his father's home in 1547, when Leonardo da Vinci was five years old, "there he lived with the other children apparently on an equality with them and received with them an excellent education," for there were no other children until Leonardo was twenty-four years old. The very fact that Ser Piero's first two marriages were childless made it desirable and convenient for the little illegitimate son to be regularly adopted and so far from missing a father, he had indeed his father's sole devotion.

When the first legitimate son was born to Ser Piero by his third wife in 1476, Leonardo was twenty-four years old and had long before this left his father's house and entered the "atelier" of his first teacher Verrocchio.

Certain it is that in 1472 (four years before the birth of his first half-brother) Leonardo's name is recorded in the Registry of the *Compagnia dei Pittori*—Firenze, then one of the most noted organizations of the age.

My personal studies of Leonardo da Vinci led me to Vinci and Florence in 1913 and the statements of the non-resemblance of portraits and plaques, pointed out as Vinci family pictures, and Leonardo da Vinci's self-portrait and also the historic entry of Leonardo's name in the *Compagnia dei Pittori* were verified by original investigation.

All great sculptors and artists require human models for their creations in bronze, marble or paintings. These models are occasionally persons of unstable character and temperament. For if they were staunch in character and of well balanced mental poise, they would find better occupations than to pose in the nude. The art colonies of Rome, Naples, Florence, Paris, Vienna, Munich, Berlin have produced strange tales of the mutual relations between artists and models.

Now in order to narrate both the light and shade of Leonardo da Vinci's artist life, it is my duty as historiographer to introduce an event given by Scognamioglio (l. c. p. 49.); it appears that a number of other art students in Verrochio's Atelier, together with Leonardo had employed as a model a boy who had an evil reputation, and who slandered the entire group of art students, Leonardo included, so that the report came to the ears of the legal authorities who investigated and tried the case.

The investigation ended with an acquittal of Leonardo and others. One has to marvel that such accusations are not recorded more frequently in the lives of the great masters for there must have been models who were only too willing to fleece men of the scientific type of Leonardo da Vinci, Michael Angelo, Raphael and others. At all events Freud does not magnify Scognamiglio's tale to fit into his theory.

In his "Geschichte des dreissigjährigen Krieges," Schiller (Sämmtliche Werke, Bd. 9, S. 349) gives expression to a guiding maxim for historiography which is of such a lofty character that I must introduce it here as a standard for forming a judgment of Freud's methods in his Leonardo da Vinci article. Schiller's maxim is this: "Where the natural course of events is sufficient for a complete explanation, we must not dishonor the dignity of human nature by a moral accusation."

Historiographers and even psychoanalysts the world over might do well to accept this high-minded and axiomatic principle as a beacon-light in all research of a historic nature. But far from being guided by such a noble maxim, Freud starts from a preconceived opinion and construes every tradition, fact, record, etc., in such wise that it must per force be made to confirm this opinion, which is that Leonardo was a homosexual person, a construction utterly at variance with the reliable facts concerning this great thinker's life. Freud construes or rather misconstrues almost every ascertainable event in Leonardo da Vinci's life in order to coerce a synthetic presentation of his psychopathic delusion. He imagines to find confirmation of it in Leonardo's brief daybook entry of his father's death (July 9, 1504). Because da Vinci repeats the hour of this death, noting down *"a ore 7"* twice, and because he says simply *my father* (mio padre) which Freud argues should have been written my *poor* father, this enterprising psychoanalyst states that the death notice is not only robbed of every trace of pathos, but that it conceals a cryptic subtlety of colossal import, namely, that Leonardo had been in a relation of rivalry with his own father, and on p. 59 of the monograph it is stated in repugnant breadth that this abnormal relation extended between a five-year-old boy and both his real mother (which we now know from chronologic data was physically impossible) and to his stepmother.

On p. 58, Freud states that repetition of written dates to other men than psychoanalysts means nothing. Naturally; ingenious and sane men would follow Schiller's maxim and say that the shock and sorrow of his father's death produced a transient absent-mindedness, an affection that could happen to anyone. But Freud writes (l. c. p. 58) "der Psychoanalytiker denkt anders (sic) ihm ist nichts zu klein als Äusserung verborgener seelischer Vorgänge," (the psychoanalyst thinks otherwise, nothing is too little to serve as an expression of concealed psychic processes), and then he follows his irresistible tendency to make out a case against Leonardo according to his own invention.

This historiographer is left to choose who is the more logical and exact psychologist—Schiller or Freud. There is nothing in the daybook entries and expense accounts of Leonardo da Vinci concerning details of his difficulties with an utterly vicious boy (l. c. p. 43) nor in the expense account covering his mother's funeral (p. 44) which with the wildest stretch of the imagination can be construed and disfigured into the "Zwangsneurose aus verdrängter Libido," which Freud aims to establish in his Character u. Analerotik, l. c. p. 46.[5]

One cannot fully understand the labyrinthine persuasions of this psychoanalyst unless they are themselves subjected to a rigid historical as well as psychologic analysis. Thus we must follow Leonardo's supposed erotism through various phases, some of them diametrically opposed to each other. To attribute any sense to these whatsoever, we are asked to believe that the best endowed children pass through a period of infantile sex-investigation from the third year on (see l. c. p. 18). The first affinity is asserted to be his mother, and Freud is not certain how early the boy was taken from her—his exact words are between the third and fifth year and for evidence of his extraordinary assertions he refers us to his own publications in "Jahrbuch f. Psycho-Analyt. u. psychopath. Forschung": Bd. I, 1909, and Bd. II, 1910, in which one finds no uncontroversible evidence to fit the supposed case in Leonardo.

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[5] His term *anal-erotik* is emphatically to be rejected—orthologically as well as æsthetically.

Then he supplants the love for his mother by falling in love with himself. Auto-erotism (l. c., p. 40) which is here given a new term *Narcismus* after Narcissus who was infatuated with his own reflection in a mirror or in a water surface. Still later this infatuation is crowded out (Verdrängung) through diversion and preoccupation of all Leonardo da Vinci's potential intellectual forces into art and science.

Whilst Vasari is criticized by Freud as an unreliable historiographer on p. 2 (top of page), the entire second half of this same page and many other passages elsewhere are literal translations from Vasari, of whose importance as a historian I have written elsewhere. (Annals of Med. Hist. 1920).

A most important question has to be raised concerning the title page picture of St. Anne and its diagrammatic interpretation on p. 54. Many who study this reproduction cannot discern the vulture at all. When the picture is turned with the right side down and the diagram on p. 54 is consulted, the puzzle is made clearer; but the tail (coda) does not strike the child's mouth; it goes past and skips it. Freud makes a mountain out of a mole hill when he argues about the "coda," striking at the infant's mouth. *Is that part of the phantom bird that is directed toward the wrist of St. Anne really the tail?* It must have been an acrobat or contortionist vulture to be able to twist himself into such lines. Surely the tail should be where the right foot of St. Mary begins. As Freud must construe the tail to be near the infant's mouth—he imagines the part of the drapery over St. Mary's left shoulder and arm to be the tail and thereby gives the phantom vulture a fatal twist which ornithologists will view with tolerant amusement. Finally and worst of all for the psychoanalyst's view, St. Mary's left arm drapery must be about one foot behind the child's mouth. The accommodating diagram artist has left out all light and shade in the sketch on p. 54 and the perception of Leonardo's masterful perspective about the child's head and the left forearm of St. Mary is destroyed (si duo faciunt idem—non est idem).

The brilliant Italian physiologist Philippo Botazzi has written a work on Leonardo as a biologist and anatomist in which he quotes two famous sentences from the master himself which have provoked much discussion because these sentences have been taken not in their didactic simplicity, but misconstrued by presuming them to contain a dogmatic assertion. The first sentence is: "One has no right to love or to hate anything unless one has striven to acquire a thorough acquaintance with and understanding of its entity." The second sentence is: "Such critics should be silent; for that is the manner of perceiving the creator of so many wonderful things. Verily, great love arises from great understanding of the loved object and when thou understandest this but little, thou wilt be able to love it but little or not at all."

The enterprising psychoanalyst may sophisticate all he pleases about these sayings, but he cannot blemish their clarified dignity. They mean exactly what they say. Only they do not assert love to be as it is but as it should be. And they refer principally to love for art and for science, which indeed had in him gained the supremacy over any other kind. What is more axiomatic as that such a superb artist and profound thinker should eventually concentrate his intellectual energies upon art and science to the exclusion of everything else? To draw in a psychopathic proclivity to explain such an obvious development is biographic or rather pathographic calumny.

In order to forestall an inevitable rejection of his pathography, Freud on l. c., p. 69 seeks to invalidate the attitude and manner of narrative of biographers with a sincere naïveté. He alleges that they are "fixiert" to their hero in a very peculiar way, because they have only selected him as object of their studies on impulse of personal sentiments—bringing a priori, a special affection to him and their study of him.

Then Freud indulges in the slur that biographers become engaged in a work of idealization, tending to place the great man in the series of their infantile (?) prototypes. He boldly charges that biographers:

(1) Efface the individual features of the great man's physiognomy.
(2) Smooth out the traces of his life's struggle with inner and outer resistances.
(3) Tolerate not even a remnant of human infirmities or imperfections, and
(4) Present a frigid, strange, ideal figure instead of the man to whom we could even remotely feel related.
(5) They sacrifice truth to an illusion and in favor of their infantile phantasies (?), they renounce the opportunity to penetrate into the most charming secrets of human nature.

If there ever was a biographer that gave evidence of those defects, his incompetence would soon be detected and he would not be taken seriously.

When it comes to defects Nos. 1, 2, 4, and 5, I fear that the psychoanalysts have evidenced the greater liability to detraction of true characters.

The question of interest is: What do the dependable historic sources demonstrate to be either truth or error? The preceding pages have given my investigation on this problem. Understanding is better than apotheosis. But where an achievement is so sublime that for a time it surpasses understanding, then reverence is better than iconoclasm.

It is for instance irreverent to state and not prove certainly that Leonardo rebelled against his father (Freud, l. c., p. 61) and that this aversion constituted the infantile condition to his achievements as an investigator, whilst at the same time it injured his power of achievement as an artist.

Neither has posterity expressed a reproach against Leonardo for not completing his works. For none of Leonardo's works can be said to be incomplete except some anatomic sketches which were never intended to be published in the form he left them. All great artists, sculptors, research workers (especially composers) have left many such designs (Entwürfe) which were the first conceptions of some inspiration and original idea, or first rough result of experimentation and put down as aids to memory not meant for the public eye.

The citation of Solmi (Leonardo da Vinci, transl. into German by E. Hirschberg, Berlin, 1908) occurring on p. 8, fifteenth line from the top, Freud, l. c.), whilst it really is contained in Solmi's book, the original source is not definitely established. In reality this sentence is no proof of homosexuality; it impresses one more as the repugnance against sensuality that has developed in a chaste and abstinent man at an age when sex had lost all charms "when the grasshopper shall be a burden and desire shall fail."

The methodology of composing biographies is laid down in Bernheim's Lehrbuch d. Historischen Methode u.d. Geschichtsphilosophie," and the dangers in writing biography are set forth in John Martin Vincent's "Historic Research," pp. 130 and 265, and the test of objective biography is the experience of the author as an historiographer.

What is needed is a judicial discernment of facts, not a vindication nor a vilification; the investigator must not be led by sympathy nor by distaste for the life he aims to present.

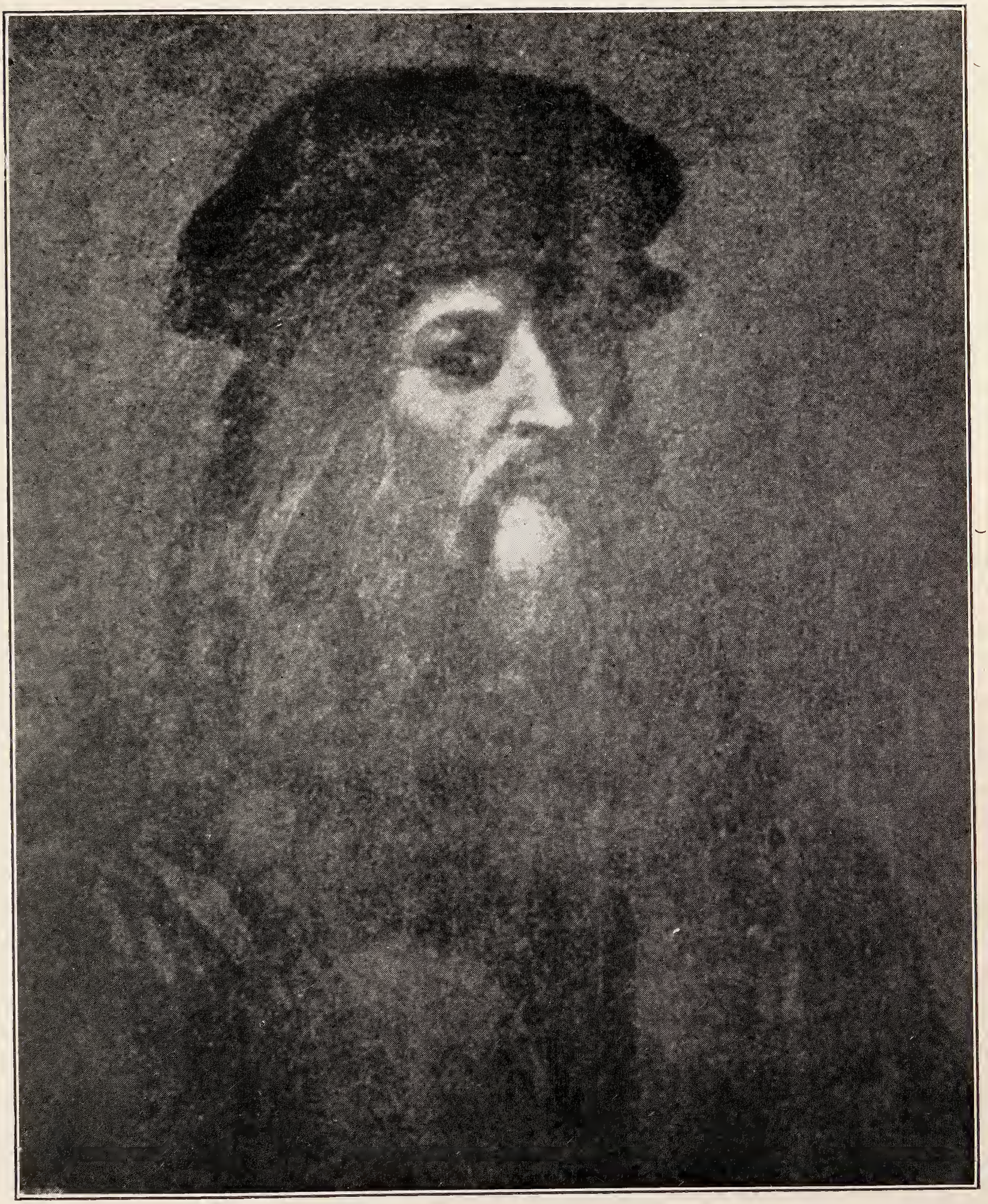
Although Freud asserts on l. c. p. 69 that he has never classed Leonardo among neurotics or nervous patients, and notwithstanding his statement that normal and abnormal cannot be sharply distinguished because all neurotic symptoms are phenomena of substitution, or replacing for efforts of crowding out previously existing psychic complexes, which we all have to perform during our development from childhood to adulthood, and in spite of the explanation that it is only the number, intensity, and distribution of these psychic substitution complexes which justify the practical conception of diseases and the conclusion of constitutional inferiority, in spite of all this dexterous casuistry, he classes Leonardo among the neurotic types, the "Zwangstypus" and the "Grübelzwang." "Zwang" means coercion, compulsion, force, and "Grübeln" or "Grübelei" = close and subtile investigation of trifles. There can be no classification on the basis of "Grübelei" only, but the addition of the word "Zwang" indicates that Freud means Leonardo was helplessly coerced to research in trifles by inner psychic compulsion. The crown of this dissimulation comes when he compares Leonardo's inhibitions with abulia (l. c. p. 70). This word denotes a mental disease characterized by loss of the will (Willenlosigkeit). So we have succeeded in following Freud from a defense of a possible classification of Leonardo as a nervous patient to his assigning him to abulia.

On page 74 the psychoanalyst appears to falter in his positivism; it almost looks as if he had at last become subconsciously aware of the one-sidedness of the method of pathography he has hitherto pursued. For after he has remarked that his psychoanalytic investigation has endeavored to expose Leonardo's original psychic impulses, to disclose his character from his reactions (dynamically), there comes this striking apologetic statement: "Wenn ein solches Unternehmen keine gesicherten Resultate ergiebt," etc. etc., translation. "If such an undertaking, as perhaps in the case of Leonardo, yields no definite or certain results, the fault lies not in a defective or insufficient (inadequate) Method of Psychoanalysis but in the uncertainty (also infirmity) and incompleteness of the material which tradition brings for such a person."

This defense of psychoanalytic methods in which the author has to shoulder the blame of inadequate historic material and in which he actually uses the term "tradition" in place of facts and evidences amounts almost to a doubt of the reliability of all that preceded this astonishing sentence.

We learn once more that the general analogies of individual psychology can never and nowhere suffice for an understanding of the connections of personal conduct but that they must be methodically controlled, supplemented and defined by collateral historic data and evidences.

To show a highly authoritative estimate and appreciation of Leonardo's



Leonardo da Vinci
Self-Portrait

achievement as a painter, I can do not better than refer to the following quotation from the five volume work of John Ruskin's "Modern Painters":

See: Vol. I, page 5, 84, 86–88, 112, 115, 134; XLII, Preface and Edition 2–42; Vol. II, page 80, 117, 162, 171, 208; Vol. III, page 28, 39, 42, 127, 245, 267, 342, 347; Vol. IV, page 44, 47–49, 57, 310, 367–8, 396; Vol. V, page 36, 123, 186, 256, 263, 323; page 66, General index.

Ruskin, Vol. III, page 27—Greatness of style consists in choice of subjects of thought that involve wide interests and profound passions.

Choice of subject from such a heart as the painter has, l. c. page 28.

What to seek in Art, page 35, whatsoever things are lovely. Page 42—(Vol. III) *Resumé* of powers that make a painter great. Page 268—Seeing and thinking, etc.

Vol. III, page 291—The most useful members of society are the dissectors, not the dreamers.

The style is greater or less in exact proportion to the nobleness of the interests and depth of passions involved in the subject. The habitual choice of sacred subjects as the Nativity, Crucifixion, etc. (if the choice be sincere), implies that the painter has a natural disposition to dwell on the highest thoughts of which humanity is capable. (Vol. III, page 28.)

Moreover I will quote from page 35, Vol. III of this classic work, a rule of guidance not only for discernment of beauty in painting but of the beautiful in any human endeavor:

"It is only by the habit of representing faithfully all things, that we can truly learn what is beautiful, and what is not. The ugliest objects contain some element of beauty; and in all it is an element peculiar to themselves, which cannot be separated from their ugliness, but must either be enjoyed together with it or not at all. The more a painter accepts nature as he finds it, the more unexpected beauty he discovers in what he at first despised; but once let him arrogate the right of rejection, and he will gradually contract his circle of enjoyment, until what he supposed to be nobleness of selection ends in narrowness of perception. Dwelling perpetually upon one class of ideas, his art becomes at once monstrous and morbid; until at last he cannot faithfully represent even what he chooses to retain; his discrimination contracts into darkness, and his fastidiousness fades into fatuity.

"High art, therefore, consists neither in altering, nor in improving nature; but in seeking throughout nature for 'whatsoever things are lovely, and whatsoever things are pure'; in loving these, in displaying to the utmost of the painter's power such loveliness as is in them, and directing the thoughts of others to them by winning art or gentle emphasis. Of the degree in which this can be done, and in which it may be permitted to gather together, without falsifying, the finest forms or thoughts, so as to create a sort of perfect vision, we shall have to speak hereafter: at present, it is enough to remember that art, *caeteris paribus,* is great in exact proportion to the love of beauty shown by the painter, provided that *love of beauty forfeit no atom of truth.*"

When Freud suggests on page 63 of his essay, that Leonardo in his prophecies insulted the sensitiveness of Christians that are devoted in their faith, he quotes words from Leonardo which Christ might have said himself. In fact the references to the eyes are taken from Jeremiah 5:21, and also Mark 8:18; and the reference to the ears is taken from Psalm 115:6.

The entire quotation in its general sentiment might have been an emulation of the 115th Psalm. But throughout the entire essay of Freud's, it would have been a safer guide, if the matter of Leonardo's faith was to be brought into discussion, for Freud to be familiar with the 4th chapter, 8th verse, of the Epistle of Paul to the Philippians:

"Finally, brethren, whatsoever things are true, whatsoever things are honest, whatsoever things are just, whatsoever things are pure, whatsoever things are lovely, whatsoever things are of good report; if there be any virtue, and if there be any praise, think on these things."

One of the most penetrating students of human nature, and most versatile historian, G. Sarton, expresses a sentiment in "Isis," no. 5, 1923, pages 5 to 19. The article is entitled "Knowledge and Charity." One of the expressions therein is the following: "Science alone is nothing without growth of heart. The forces of the body, the power of the intellect and the sentiments of the heart must form a unity. The historian of science in depicting a scientific personality should not be bound exclusively by the cold hard facts, but must endeavor to bring before our mind the entire man."

Freud failed in both of these indications. He did not even keep to the "cold, hard facts," and as regards "Herzensbildung" there is no trace of it. I may conclude with the semblance of a reverent feeling for humaneness, expressed by Kant, with the spirit of the quotation from Terence given at the beginning of this article. Kant says: "Ich glaube . . . Das moralische Gesetz ist heilig. Der Mensch ist zwar unheilig genug, aber die Menschheit in seiner Person muss ihm heilig sein. In der ganzen Schöpfung kann alles, was man will und worüber man etwas vermag, auch bloss als Mittel gebraucht werden; nur der Mensch und mit ihm jedes vernünftige Geschöpf ist Zweck an sich selbst."

(Transl.) "I believe the moral law is holy. Man may be unholy enough, but the humanity in his person must be holy to him. In the entire creation, everything concerning which we are enabled to will something, or enact something, can be conceived also as simply a means to an end, but as a purpose *in himself, there is only man.*"

XXVI

PHYSIOLOGY OF LEONARDO DA VINCI

VIEWS ON NATURAL PHILOSOPHY AND THE METHODS OF SCIENCE

THE abundance of literature that has accumulated about Leonardo da Vinci in all languages indicates the great power of attraction which his life and work has exerted upon the minds of thinking men ever since his death. Unfortunately many have not drawn their information directly from sources, and many attacked problems for which they were not equipped. It is for instance out of the province of a clinician to endeavor to untangle some of da Vinci's mathematical problems or his speculations in geology or hydrodynamics. The question arises, why do medical men again and again attack such problems as lie outside of their domains? The answer is that Leonardo da Vinci makes use of his dynamic, mathematical, and geologic information to explain physiologic and anatomic things. In his account of the function of the kidneys, ureter, and bladder, he introduces much of the hydrodynamics from his *Codex Atlanticus.* In the "Giornale Di Medicina Militare," fasc. 11, edited on the four hundredth anniversary of his death (nel IV Centenario de la sua Morte) there are articles of great excellence on the art and laws governing paintings of Leonardo, by Arnaldo Angelucci, Director of the Ophthalmologic Clinic of the University of Naples; on his Biologic Phonetics by Guglielmo Bilancioni; on "Una Ricetta Medica nel Codice Atlantico" by G. B. de Toni; on the Operations of the Organs of Sense as Conceived by Leonardo, by Guglielmo Bilancioni. On page 1255 of this journal there is a fascinating discourse of his impressions concerning music, and on page 1272 a discourse on his anatomy by Antonino Anile, Professor of Microscopic Anatomy at the University of Naples, and on page 1279, the great da Vinci expert, H. Hopstock, speaks of the anatomic physiologic manuscripts of Leonardo at Windsor. This is only a brief reference, however, to the great work by Hopstock, Vangensten, and Fonahn, brought out at Christiania in 1916, to which I will refer later.

The original sources are sometimes written in a form of the Italian language of the sixteenth century which is extremely difficult to understand, even by an expert scholar of modern Italian. Monsignor Leo P.

Manzetti, who very kindly translated many of these important articles for me, assured me that the Italian of that period gave many difficult orthologic problems to solve for the translator. What is necessary is a dictionary by Latin and Italian scholars instructing us how words were used and what were the specific meanings attached to them in those days. The present article was written with a view of throwing light on da Vinci's views concerning nature in general, concerning philosophy, his plan of conducting experiments, and his more special views concerning physiologic function. A number of my personal friends who are medical historians of distinction, Dr. Lewis Stephen Pilcher of Brooklyn, Dr. Charles L. Dana of New York, and Dr. Fielding H. Garrison of Washington, who has given us a noble history of medicine, have recommended that I intersperse the genetic historic account of the great lives I am presenting by homeopathic doses of such judicious levity, in the form of anecdotes and personal character infirmities, as to give a few high lights on the humaneness of these men. A good deal could be said of Haller, Billroth, and Goethe and also of Leonardo da Vinci concerning the humane side of their personality. The difficulty with these anecdotal insertions is that they are not as a rule well-founded historically, but it can be safely stated of Leonardo that he was a sportsman in every sense of the word, that he was a very capable musician and, as can be seen from his many caricatures, had a very keen sense of humor. A very subtile psychologic element of his personality creeps out in the revelation of his discovery of the contraction of the pupil when light fell upon the retina, and of the dilatation of the pupil when the environment was dark. He became so intensely interested in this physiologic observation, that in painting the famous Mona Lisa he must have changed from the rôle of a portrait painter to that of a physiologist and forgotten that he was reproducing the features of a very observing and self-conscious woman. The result was that those features frequently expressed a peculiar ironic smile which the artist himself must have caught, and, totally unconscious that he, with his deep physiologic studies, had caused it, reproduced it with great exactness on his canvas.

Another very interesting anecdote concerns the effect of vice and abuse of alcohol on the facial expression. It is stated that when da Vinci was painting the head of Christ in his Last Supper, "Il Cenacolo," he was looking for a human being as a model for the head of Christ. He had conceived of a man of classic features, yet expressive of endless kindness and benevolence. Such a person was naturally very difficult to find, but eventually someone discovered a man who approximated very closely to da Vinci's ideals of physiognomy. Years later Leonardo sought a model for the head of Judas representing him during his decline, as a result of treachery and degeneration. (Rodolfo Lanciani in his "Pagan and Christian Rome" (page 186) speaks of Nero's face in his youth, and then when it later showed marks of the degenerative stages of his more advanced years.) A man was eventually discovered with such villainous features that he appeared to meet da Vinci's ideas of degeneracy. When he reached the study of the great artist, the man expressed familiarity with the objects and surroundings of the room, and on being asked whether he had ever been there before, he answered, "Yes, I served as one of your models before." "In what way" asked Leonardo da Vinci. The individual replied, "I was your model for the head of Christ."

THE PHYSIOLOGY OF LEONARDO DA VINCI

Because a knowledge of structure must precede a knowledge of function Leonardo da Vinci's "Anatomic Studies" have met with a far greater interest than his ideas on function. The literature of investigations concerning his purely anatomical studies does not even at the present day, cover all that has been discovered of Leonardo's anatomic drawings. The *Annals of Medical History* (vol. 1, p. 326) contains a brief review of the Anatomical Charts of da Vinci that were found in the Royal Library of Windsor and published by the House of Jacob Dybwad in 1916. The review is by the skilled "Leonardo Kenner," Karl Sudhoff, and appeared originally in *München, Med. Wochshr.,* 1916 (63 p. 1622). The title of this great work is *"Leonardo da Vinci: Quaderni d'Anatomia V, Ventisei fogli della Royal Library de Windsor, Vasi, muscoli cervello e nervi—anatomia topographica e comparata. Quaderni d'Anatomia VI. Ventitre fogli . . . Proporzionfunziono deo muscoli-anatomia della superficie del corpo humano, pubblicate da Ove C. L. Vangensten, A. Fonahn, H. Hopstock. Con traduzione inglese e tedesca. Christiania, Casa editrice Jacob Dybwad,* 1916.

For further bibliography, consult:

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Holl, M.: Leonardo da Vinci: Quaderni Anatomia IV Arch. f. Anat. Leipzig 1915—1–40.

Klebs, A. C.: 1.—Leonardo da Vinci and his Anat. Studies Bull. Soc. Med. Hist. Chicago 1916—IV—66–83.

Klebs, A. C.: 2.—Leonardo da Vinci Scientific Research with particular reference to his investigation of the vascular system. Boston M. & S. J. 1916 p. 125—1–45.

In the anatomic studies of Karl Sudhoff and Moritz Holl, much is expressed that indicates a knowledge of function and also reveals the normal human proportions that are of such great value to sculptors and painters (See Karl Sudhoff "Archiv für Geschichte der Medizin" Band 1, 1908, p. 67 and also p. 317).

The modern physiologist who has taken delight in the admirable finer dissection of the heart by Franklin P. Mall may be surprised to learn that Leonardo writes (see the reverse side of the first page in Manuscript G of fifth volume of "Les Manuscrits de Leonard De Vinci" edited by Charles Ravaisson-Mollien, 1881–1891, Quantin, Paris), the following words: "I have described the course of the muscles which run from the base of the heart to the apex and the position and location of the muscles which originate at the apex of the heart and run back to the base." This is written in Leonardo's characteristic mirror writing and is accompanied by three exquisite drawings of transverse sections of the heart. In the second article in the first volume of Sudhoff's Archives (p. 317), Sudhoff furnishes the proof that Albrecht Dürer certainly had seen anatomic drawings of Leonardo da Vinci and had used them. There is a canon of human proportion by Dürer which had been edited by Robert Bruck in Strassburg (1905) published by Heitz and Mündel (40 pages and 160 plates) in which there is extensive use of Leonardo's drawings.

Leonardo had also comprehended the statics and mechanism of the human skeleton which remained unknown even to Vesalius (see Des Andreas Vesalius Sechs Anatomische Tafeln Vom Jahre 1538, in Lichdruck by Moriz Holl and Karl Sudhoff).

Coming now to Leonardo's ideas on physiology they are scattered through so many of his writings that I doubt very much whether even up to the present day all of his conceptions on functions have been discovered. There are many of them in the colossal *Codex Atlanticus* which is thought to be exclusively devoted to technical, constructive, and engineering subjects so that the vast extent of these writings necessitates a very slow interpretation and investigation.

Another great difficulty, as previously stated, is the peculiar character of the Italian language used from 1452 to 1519 and before and after that. It is very difficult even for an Italian scholar to translate it. I have received great instruction from an article by Filippo Bottazzi, (Archivio per L'Antropologia e la Etnologia, entitled "Leonardo da Vinci, Filosofo-Naturalista e Fisiologo," Vol. 32, p. 253, 1902). Even where Bottazzi appears to claim a little too much for his illustrious fellow-countryman, a close investigation reveals that the claims made by others are either not well substantiated or they are simply amplifications of what Leonardo already had described.

M. Maeterlinck in his book "Les disciples et les fragments de Novalis" (3rd edit., 1895, p. 17), makes Novalis say: "Let every man who belongs to this race and who has this faith, and who wishes to participate in this tilling of Nature, frequent the studio of the artist; let him hear the unsuspected poetry that percolates all things, let him never forsake the contemplation and the companionship of nature, let him follow her leadership, let him not spare himself, at her call, a hard march," and Bottazzi applies this to Leonardo Da Vinci; it reminds me of one of his own sentences.

Leonardo himself refers to the man who does not make use of all of his gifts and endowments to the utmost as "sleeping." "O sleeping one" he says, "what is this sleep? Sleep has the similitude of death; O why not labor so, that after death you may have the similitude of perfect life, rather than, living, make yourself, through sleep, like to the mournful dead?" and elsewhere Leonardo says: "Rather death than fatigue and if it were necessary to pass through marshes, he shall find indescribable treasures . . ." The following words cannot fail to indicate the human being of over-powering genius: "A long and unwearying intercourse, a wise and untrammeled contemplation, an attention caught by the slightest signs and the slightest indications, an inward life of the poet, trained senses, a soul pious and simple, these are the things essentially requisite in the true lover of Nature, and without them none shall see his desire prosper . . . It is not necessary that a single sense sleep . . ."

A number of modern biologic interpreters have raised the question whether Leonardo possessed the scientific method of investigation as it is conceived in 1926. Some of his contemporaneous admirers use superlative epithets which express their saturation with the admiration of his achievements. That "splendid master of theories" (Caverni, R. Storia del Metodo sperimentale in Italia, vol. iv, p. 26); that "Divine genius, creator of experimental philosophy in Italy," as Libri calls him in a letter to Gino Capponi; the man whom Alexander von Humboldt wrote (Kosmos II p. 324) that he was "the greatest physicist of his century, who united to deep knowledge of mathematics the most admirable intuition of nature"; and Gilberto Govi—another brilliant genius of Italy, little appreciated—wrote that Leonardo was the inventor of the system to which Galileo, a century later, owed his finest discoveries; the system to which Bacon, "who taught it without understanding it (he would better have said, without practicing it) is indebted for his reputation as the restorer of sciences" (G. Govi, Saggio, ecc., p. 7); Leonardo had the "scientific method."

Without the least doubt, he had it; or rather, he created it! Not ignoring the greater part of the work of the ancients, which he read himself, and with the aid of Luca Pacioli—his intimate friend in happy and troubled years—of Marcantonio della Torre, the contemporaneous Master of Anatomy, of the father of Paolo Toscanelli, and others, with a sagacious critical mind he placed experiment at the basis of all science and all art; experiment made complete by most astute thinking. "Wisdom is the daughter of experiment," was his decision. "Wrongly do men deplore innocent experiment, accusing it of untrue and fallacious demonstrations." "Experiment *never* fails, but *you* fail in your interpretation," he would say to those who did not understand the art of experimenting, "because you expect from it results which never can occur from such experiments." Somewhere else he declares that "experiment interprets the processes of nature to the human species." Experiment "reveals the methods which nature uses among mortals, impelled by necessity." One experiment alone is not enough. "Rather than make from one result a general law," he warns, "make two or three further experiments, observing whether these produce the same result." He despised the mere speculators, "who wanted, through imagination alone, to be interpreters between nature and man." In this statement Leonardo emphasizes the great importance of repeated verification to all experiments. True indeed it is that "experiments deceive those who do not understand them"; but he had the eagle's sight, and could perceive that "things which look the same are often of great variety." And he warned authoritatively: "Beware of the teachings of those speculators whose beliefs are not confirmed by experiment."

Having given to experiment and speculation their places as the basis of scientific procedure, let us see how the experimenter must conduct himself in the difficult field of science, which is "That mental discourse that is based on its own ultimate principles, beyond which in nature no other thing can be found, which is part of such science."

"All of our knowledge begins in sensation": this is his profession of faith as a naturalist. But all of our senses, by themselves, deceive us, some more (smell, taste and touch), others less (ear, eye). And even judgment, by itself, deceives us; indeed "there is nothing that deceives us more than to trust our judgment, without other considerations"; and "the greatest error of men is in their opinions." It thus appears that in a careful equilibrium between experiment and discernment lies the proper scientific method.

In what have the centuries, following his own, added to the profound thought of Leonardo? Shall any of our experimenters hesitate to recognize him as our pioneer in those special problems he undertook to solve?

Theory must go before practice. "Study first the science," he counsels, "then follow the practice born of science." "The science is the captain, and the practice is the rank and file." Could crude empiricism be condemned more openly, since he had already condemned "all that science that ends in words";—which "die, as soon as they live"?

Again, so as to maintain a perfect equilibrium between theory and practice, he says; "But first I shall make a few experiments, before I proceed further, because my intention is to present first experimentation, and then by means of reasoning show why such an exact experimentation is compelled to work out in such a definite way. When, however, the reason of the facts is grasped, there is no more use for experiments, which are made by man for the very purpose of comprehending this reason. "There is no effect in nature without a reason; understand the reason, and you will not need experiment." Then, too, if once the reason of a thing is grasped, by deduction, other and various consequences can be inferred." It is not objectionable to deduce or to form generalizations, if we proceed along scientific lines, that is, to develop some general rule, born of the preceding conclusion."

By the *reason* of a natural process or of a thing, Leonardo, however, is far from meaning the *essence* (Latin: essentia, being, nature) of the phenomenon or of the thing, for he knows that "the definition of not one essence of the elements is within the power of man; yet a great part of their effects is known."

There were so many things in nature to be discovered in his time that one is not surprised that he was keeping himself away from "the deceptive mental sciences," in his researches; by these severe words he clearly intends to indicate the metaphysical nonsense that had obscured the minds of the scholastics. "If we are to doubt everything that appeals to the senses, how much greater doubt must we have of those things that elude the senses, as for example the essence of God, the soul, and similar things, about which these men are always disputing and contending?" "Now look, Oh reader," he writes, "what confidence can we give the ancients when they wish to define what soul and life are, *things unprovable,* while those things which by experiment they could at any time have clearly known and proved were for so many centuries ignored and falsely believed."

God, soul, life: Insoluble problems! They constitute the "Welträthsel" (enigmas of the world) of Du Bois-Reymond, on which this celebrated naturalist philosopher put, as with a seal his famous "Ignorabimus"; although long after Leonardo had called them "things unprovable." Yet in those words of Leonardo there was contained nothing irreverent toward great spiritual things; on the contrary, in more than one place the Master makes manifest his orthodox philosophical convictions. "The soul," he says, "never can corrupt itself in the corruption of the body." Elsewhere he affirms the supremacy of the spirit with the following words: "Our body is subject to heaven, and heaven is subject to the spirit." From many passages it is clearly evident that he knew and admired Plato; and in his own writings are frequent expressions of his reverence for the Scriptures and invocations of God, among which this is famous for its poetic fervor: "What wonderful justice is thine, First Cause!" The exclamation was inspired always by profound scientific considerations.

Leonardo was then, like most of those fine intellects in the past and in the present, a follower of that way of understanding the world, to which the more profound thinkers and physiologists of the modern era—Kant, Du Bois-Reymond, Tyndall, Preyer, Helmholtz, Pasteur, Wundt, and many others—have inevitably inclined sooner or later in their lives.

But "no human investigation may claim to be true science, if it has not passed through mathematical demonstration." "Let not him who is not mathematical," says Leonardo, "read me in my doctrines." He wished in all science to come to "mathematical conclusions"; because "there is no certainty where the mathematical sciences cannot be applied, or are not already involved." In fact he thought, "The relations not only in number and measure, must be found, but also in sounds, density, weight, times, and position (suoni, pesi, tempi, e siti) and of what potential they are." He is not speaking of that "potential power" which is the cause of the phenomena of life, but of some subordinate question, not essentially connected with life, for example, as the classification of living beings.

Perhaps that is why Leonardo did not apply himself with great ardor to biology. From certain passages which I will quote later one might infer that Leonardo was afflicted with the infirmity which affects many modern "vitalists," and which is called "finality." But he only seems to be, he really is not; he often writes in terms of the ancient adherents of this cult, but he is really a most confirmed "determinist."

"O speculator on things," he says, "do not praise thyself for the knowledge of things, which nature usually of herself, in her ordinary course, develops spontaneously; but rejoice to know the end-object of those things, which is traced out in thine own mind." Of that finality alone which is in our own thought, are we permitted to reason. This means that in Leonardo's opinion we can reason about the nature or concept of things, as far as our mind permits us to grasp or understand them—their purpose, their end object—likewise only as far as our mind permits us to grasp or understand them but the limitation of our mind is not necessarily the limitation of nature.

Of necessity in the phenomena of nature, he treats here and there in a few sublime words: "Necessity is mistress and tutoress of nature." "Necessity is the basis and the discoverer of nature, its guide and rule eternal." "Nature never breaks her own laws." "Nature is compelled by reason of her own laws which exist everywhere throughout her." "O admirable, and wonderful necessity, thou dost bind, by thy law, every effect directly to its cause! *These are the miracles!*"

In an address at the Biological Laboratory of the Johns Hopkins University, Hans Driesch expressed this profound insight in the following words: *Causality is the continuance and consequence of some becoming* (November, 1923); i. e., we are not capable of knowing causality except by an action or occurrence that has already become perceptible. The limitation of human reason makes it impossible to be aware of causality except by an afferent perception of something already going on. Effect is "cause in action," i. e., that part of the cause which our limited understanding powers permit us to perceive.

In conclusion, all the precepts of Leonardo touching the strict scientific experimental method can be formulated in this brief sentence: "Good judgment is born of good understanding, and good understanding is derived from reasoning along good rules, and good rules are daughters of good experimentation, the common mother of all sciences and arts." (Filippo Bottazi, translator.) After having considered Leonardo as the founder of the experimental method and as the natural philosopher, let us see now what biologic observations and speculations can be ascribed to him.

He had a good knowledge of plants. Vasari says: "An order was given to him for a portière, to be made in Flanders, of woven gold and silk, which was to be sent to the King of Portugal—a design representing Adam and Eve during the event of their first sin in the terrestrial paradise. For this Leonardo painted chiaroscuro with white lead, a meadow containing an infinite number of plants and animals, and so perfectly that one could say that in exactness of detail and in naturalness all the divine genius in the world could not produce its like." This design according to Caverni, "revealed in the artist a perfect botanist."

Not only did Leonardo describe the special disposition of leaves on the branches of plants (that part of botany which is now called phyllotaxis), but he sought the reason of it. Let us listen to the Master: "Nature has placed the leaves of the terminal branches of many plants in such a way that always the sixth leaf is above the first. In a treatise on Botany by E. Strassburger, Italian edition published by the "Società Editrice Libraria" of Milan, 1896, is stated on page 37: "It is evident that in this case, in which the transverse distance of two successive leaves is two-fifths the circumference of the branch that bears it, *the sixth leaf is placed vertically above the first, and seventh above the second* in the same way, and so on, and it occurs so successively, whenever the rule can be observed."

Note that stress is on the fact that each leaf has four spaces free above it. "This is done for two important services to the plant itself: first, when the branch and fruit will bud out the following year from the gemmating node, which is always in contact with the joint of the leaf, the rain-water which moistens the branch, may run unhindered down to feed the bud that is forming, by laying its drops in the concavity of the leaf that has already begun to grow. The other benefit is, that, when the branches are budding the following year, one does not cover the other, because the five branches come out facing in five different directions."

He observed also how the sap would flow to a part of the tree where the bark had been peeled off. "The tree which has been peeled off receives from nature which provides for its welfare, much more nutritive humor (sap) for the peeled off part than for any other; in such a way that where the bark is gone another grows much thicker than in any other place. And so impetuous is the humor (sap) that, arriving at the place to be succored, it lifts above it like a cup and ball, with sundry heapings and super-additions, not unlike those of boiling (frothing) water."

In trees, the concentric strata on the side looking south, says Leonardo, show more vigor than those on the side looking north. The circles on sections of the branches are equal to the number of the years of their existence, and their greater or less thickness is due to the greater or less humidity of the year. The axis line of the tree is also nearer the bark that is turned away from the south, for the reasons already given.

He knew how the bark grew; and made some experiments with poison on plants. The supreme influence that the sun exercises over the life of plants did not escape him, although he could not determine the nature of it; and that the earth (humus) nourished them.

Leonardo was a great lover of the sun; he could not see "in the universe a body of greater magnitude and virtue." "Its light illuminates all the celestial bodies, that course through the universe (meaning our solar system). All living beings are its children . . . and no other heat or light is in the universe. . . ." He demonstrated, too, by experiments, that "the sun is extremely hot": and affirmed that: "it is not the splendor of the sun that warms, but its natural heat," so differentiating its luminous from its calorific rays. In the calorific rays he discovered the only source of warmth, and the law that the intensity of the heat was in reciprocal ratio to the square of the distance. I have already referred to the publication of Leonardo's anatomic investigations by the Swedish house of Jacob

Dybwad, Christiania. This vast work was begun by a society of experts in 1916 and the reëditing required over three years. (See review in Annals of Medical History, Vol. I., p. 326.) Thus we are just being enabled to form a conception of the versatility, comprehensiveness, and far reaching adaptation of this incomparable genius from his enormous industry in anatomy alone. It will require a decade for some modern anatomist to familiarize himself with this wealth of medieval anatomic research.

The publication of the anatomical designs and writings of Leonardo by L. Beltrami (Il Marzocco, 1902. n. 2.) shows what a keen and learned dissector he was. When Cardinal Aragona paid him a visit in 1516, Leonardo prided himself on "having made anatomical study" of more than thirty cadavers of men and women of all ages. The "Anonimo," published by Milanesi, adds that these dissections took place in the hospital of S. Maria Nuova in Florence (E. Müntz, Leonardo da Vinci, l'Artiste, le Penseur, le Savant. Paris, Hachette, 1899, p. 346). He continued them, without doubt, when in Milan, having as colleague in his researches, not as teacher, the great and unhappy Marcantonio della Torre of Verona, and perhaps his only guide, the Manual of Anatomy of Alexander Benedetti, the first edition of which was issued in Pavia, in 1478. (See Mortimer Frank's translation of Ludwig Choulant's History and Bibliography of Anatomic Illustration, p. 99, for the fate of da Vinci's anatomic drawings and sketches.)

To show the care he took in exposing the veins, he himself describes the extreme pains he exercised in dissecting off "the flesh that is found around a vein, without causing any escape of blood, except indeed a slight capillary sweating. A single cadaver was not sufficient on these occasions, for it was necessary to proceed successively on as many bodies as were necessary to complete the study; each dissection I did twice, to note variations."

He certainly also occupied himself a little with comparative anatomy, otherwise he would not have affirmed that "all the (higher) terrestrial animals have similarity in members, that is to say, in muscles and bones." Very remarkable is his observation that "in the lion family the sense of smell is located in the substance of the brain"; in fact, we now know that the nerves of the organ of smell are, so to speak, an appendage of the anterior brain. Leonardo saw likewise the optical nerves "joining right at the brain," in these animals, and in the nose "a great number of cartilaginous pouches."

An intuition of the modern Darwinian theories is contained in the following note: "Man—the description of man, in which are included those that are of almost the same species, as the baboon, monkey, and many such" (See also Karl Sudhoff: Archiv f. Gesch, d. Med. Leipzig 1907. B. I. 67–317; also M. Holl: Archiv f. Anat. Leipzig 1905, 1910, 1913, 1914–1915).

To Leonardo the designation "Splendid Master of Theories" was justly applied by Caverni. There are two theories, which modern science has completely confirmed, among others which Leonardo formulated "in conclusive brevity": the one is: "Wherever is life, is heat." The other: "Motion is the cause of all life." Botazzi (l. c.) dwells at lenght on these two aphorisms and brings Leonardo's knowledge into present day relief when he reminds the exponents of biological science that they constitute the two pivots of present-day biology. All the calorimetric researches of the nineteenth century have proved with certainty that every phenomenon of life is accompanied by the evolution of heat, that every phenomenon of life is the expression of atomic and molecular movements.

Moreover, Leonardo was in advance of his times, in his intuition that the laws of mechanics dominate the phenomena of organized beings just as they do those of inorganic bodies, and gives his warning, as it were to the biologists of the future against a "vitalism" then not existing. "Instrumental or mechanical science," he writes in his quaint language, "is most noble and above all the others most useful, because by means of it all animate bodies which have motion perform all their operations . . ."

Under the heading *On Machines* he indicates four primary natural forces: (1) local movement which is produced by the three other forms of movement, (2) natural weight, (3) force and (4) percussion.(?)

"We shall therefore," he says, "first describe this local motion and how it produces and is produced by each of the three other Powers. Then we shall describe the natural weight, although no weight can be termed otherwise than accidental; but so it has in all its operations, of the nature of weight and is therefore called accidental weight; and this is set up as the third Power of Nature or the one produced by Nature. The fourth and last Power shall be called percussion, i. e., end of impediment of motion. And we shall first mention that every local involuntary motion is produced by the voluntary motor, like the counterpoise of a clock lifted up by its motor, Man." (Quaderni I. f. tr.)

"Thus in his opinion every local or involuntary movement is, in the ultimate analysis, produced by a motive power that is itself voluntary, just as the involuntary movements of a clock depend ultimately on the voluntary movements of him who raised the clock weight." (Hopstock in Singer's Studies in the History and Method of Science, vol. ii, p. 153.)

When he had established the law now recognized as universal, that "every action must manifest itself through motion," Leonardo wonderfully defined Force or Energy, and its transformation (Inertia), with words that must unfortunately, for the sake of brevity, be omitted. And since "the animals are models of the life of the world," it can be seen plainly from these and from other expressions what a wholly "positive" basis he had in his biological speculations.

Let us consider, however, a few of these more closely. Leonardo defined in a wonderful manner the ceaseless change of material that goes on in living organisms. For example: "The body of everything that takes in nourishment continually dies and continually is born again (the great French physiologist Claude Bernard, four centuries later, said the same thing in more words but not more exactly) because nourishment cannot enter except in those places where the preceding nourishment has broken down (been oxidized); and if it has broken down it has no longer life; and if thou *dost not* supply it nourishment equal to the nourishment broken down, then the life lacks in vigor; and if thou dost wholly deny it nourishment, the life "in toto" becomes destroyed. But if thou *dost* return to it just enough to equal the daily destruction, then a portion is restored to life equal to the waste; just as the light of a candle is supplied with nourishment from the melted (wax) candle. . . ."

In these last sentences we find precise and clear the present-day conception of nutritive equilibrium, according to which conception an adult animal can continue in life and health as long as an equilibrium is maintained between the ingestion of food and the daily waste. (Balance of Metabolism.)

Even a conception of the circulation of matter is roughly sketched in the following words: "Man and the animals are just a passage and channel for food, sepulcher of animals, lodging place of the dead, envelope of corruption, taking to themselves life through the death of others."

"Behold the light," continues Leonardo, "and consider its beauty. Close your eyes, and view it again: that portion of it which you see was not existing before, and that portion which was, is no longer."

How wisely and in what beautiful symbolic form does the Artist-scientist express the ceaseless changing of things which is taking place underneath their apparent stability!

The physiology of the nerves and muscles, of the organs of special sense and likewise psychological physiology were the objects of judicious observations by him. Here is an example of Leonardo's psychology:

"The soul (anima) appears to reside in the judicial part (intellect), and the judicial part to reside in the place where all the senses come together, which is called sensorium (brain), and is not throughout the whole body, as many have believed . . . because, if it were all throughout the whole body and all (complete) in every part, it would not be necessary for the agencies of the senses (organs of sensation and their centripetal nerves) to run from every part to a single place, i. e., converging to one and the same location, but it would suffice that the eye should perform the duty of perception (the visual function) on its own surfaces without sending, by way of the optic nerve, the similitude of the thing seen on to the sensorium (the brain), because the soul, for the reason just given, could have perceived it on the surfaces of the eye" (if the soul were located there).

So Leonardo indicated that he knew that the phenomenon of perception proper took place in the brain, and not in the peripheral organs of the senses.

"Similarly, with the sense of hearing, it would be sufficient solely that the voice heard should resound in the concave channels of the petrous bone, which is behind the ear, and not make from that bone to the sensorium another transit . . ." (The medieval Italian word for brain or sensorium has by some been translated by "common sense").

"The sense of smell also is of necessity constrained to communicate with the same sensorium; that of touch passes along the perforated cords (peripheral nerves), and is carried to the sensorium; these cords extend with infinite ramifications all over the skin, which covers the members of the body, and (into the) viscera (innervation of sensation, cutaneous and visceral)."

Here Leonardo goes on to treat of the nerves of motion and the muscles. "The perforated cords carry the command (nerves of motion) and sensation (sensory nerves) for the officials[1] (muscles of the limbs), these cords and nerves (meaning tendons), located between the muscles and the ribs, directing the said movement; these officials obey, and their obedience (contraction) is carried into action through a swelling (thickening of these muscles); and the swelling shortens their length, drawing tense the nerves (tendons), which form a network throughout the tissues of the limbs, being implanted even in the tips of the fingers, and carry to the sensorium (brain) the reason of their contact." (Whenever Leonardo says *nerves* he means *tendons,* and the word *cords* means *nerves.*) The nerves (tendons) with their muscles, serve the cords (nerves), as soldiers their officers, and the cords (nerves) serve the sensorium, as officers their captain; so the joint of the bones obeys the nerve (tendon), and the cord (motor nerve) obeys the sensorium, and the sensorium is the seat of the soul, and memory is its magazine or storehouse and comprehension (this may mean consciousness) is its counsel."

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[1] Officialis means "depending on proper authority," i. e., the muscles cannot normally contract of themselves, but depend on nerve stimulus.

Accurate conceptions of voluntary movement and sensation spring easily from these descriptions, couched as they are in terms which need illustration to be understood by moderns, accustomed to a different nomenclature. Notice the expression "magazine," to signify the reserve of mnemonical images, which is true food for thought.

The mechanism of sensation is even better expressed, thus: "The sensorium is that, which judges the things given to it (afferent) by the other (five) sense agencies." "The sensorium is put into motion by the things given to it by the five senses."

"Sensory processes are aroused by external objects, which impress their images (stimuli) upon the five senses, from which they are transferred to the apprehension (comprehension), and thence to the sensorium (perception); after being judged there, they are sent to the memory, in which, according to their potence, they are more or less carefully preserved."

Further on he explains himself still better: "The sensorium is set into activity by the apprehension, which is placed between it and the organs of impression. The apprehension is set into activity by the impressions of things given to them by superficial sense organs, that is, the sense endings, which are placed midway between the exterior things and the apprehension; and similarly the superficial organs are set into activity by external objects (energies). . . . That sense is quicker in function, which is nearer the apprehension; so the eye is the superior and chief of all. . . ."

Of the five senses: sight, hearing, and smell are little capable of inhibition, touch and taste not so. (Here the text and ancient meaning of old Italian orthology are obscure.) The close relationship that is admitted to exist between smell and taste is expressed by him thus: "Smell brings with it taste in dogs and other gluttonous animals." [2]

Having referred to the superiority of the eye, and the lesser perfection of the ear (in certain respects), Leonardo expresses thus the uncertainty of smell: "Smell lacks assurance of the place from which an odor comes"; then he adds: "but taste and touch, which come into direct contact with the objects, are alone aware of the thing touched." Leonardo discovered the contraction and expansion of the pupil, the camera obscura and the fact that, on the fundus of the eye, the images of exterior objects are depicted upside down.

"The pupil of the eye assumes as many sizes as there are variations in the intensity of the lights and shadows of the objects which appear before it. . . ." And again: "The pupil of the eye, when one is in the open air, changes with the progress of the sun from rising to setting." He discovered this phenomenon while looking at the face of a person (apparently the beautiful Mona Lisa, wife of Francesco del Giacondo, commonly called la Giaconda) whose portrait he was painting.[3]

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[2] Difficult to ascertain in dogs; only by a flow of gastric juice in Pawlow's dogs on smelling of food could such an association of smell and taste be made.

Much later, Galileo laid a wager that there were not two persons in a thousand that had observed the phenomenon of the reaction of the pupil to light, and, says Caverni (loc. cit. I, p. 79), by this he wished to convey the impression that the observation was his own, although Porta had described it in his "Diottrica" and Fabrizio d'Acquapendente had published the fact that Sarpi had observed it in the eyes of cats and, later, of men.

"The invention of the camera obscura" affirms Caverni (p. 79) "appears certainly to belong to Leonardo, at least that part of it which concerns the application of it to the theory of sight: an application which can not be attributed to Alberti because he openly professed the Platonic doctrine of visual rays that proceed from the eyes and assemble again with the celestial fire, and as his own statement declares that the vision reaches out and expands toward the thing seen (Opere volg., Firenze, 1847. Tomo IV, p. 100), and that the sight-ray proceeds from the eye of the observer (Architett, Milano, 1833, p. 362)."

Whether Leonardo made the experiment that is now-a-days a laboratory practice in experimental physiology, to show the inverted image of outside objects on the fundus of the eye, or some analogous experiment, or whether from the knowledge that the image is portrayed upside down on the screen of the camera obscura, he inferred that the very same thing must occur also in the eye, we cannot ascertain. Without doubt, however, he wrote: "To no distance can even the image of the smallest body enter the eye without turning upside down." And elsewhere: "Write in thine *Anatomy* how, in such a very tiny space (of the eye), the image (visual) can live again and regain its size." He believed, then, that we perceive the real size of objects looked at, through a process taking place in the interior of the eye (which is not exact). And absurd too is the hypothesis by which Leonardo proposed to explain why, notwithstanding the inversion of the image, we see objects upright. But let us remember that these are among the most difficult problems of psychological physiology, let us think of the paucity of anatomical and physiological knowledge, of the insufficiency of experimental facilities, at that time, before we wonder at what seem to us absurd interpretations.

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[3] The strikingly ironic, subtle smile which Leonardo faithfully portrays was provoked by himself, on *Mona Lisa.* His intense study of her pupils for many hours was interpreted by her probably, as most beautiful women would interpret it. That subtle smile is unconscious irony that the great objective artist is simply an admirer, after all.

Finally, it is known that he discovered that the radiations and "twinkling observed in certain stars are produced by the eye," and that he invented the very simple method of viewing the stars through a "tiny hole made with the tip of a very fine needle" in a piece of paper, to correct this defect in vision.

Leonardo described, in an almost perfect way, the automatic movements.

We have already seen how he understood the voluntary movements, in general; and how to him the muscular contraction seemed to consist in a shortening with thickening of the muscle.

"Leonardo's nomenclature is very deficient. Bones, muscles, nerves, and vessels, have, as a rule, no definite names but the old names that were in use by medieval writers often occur, for example, *adjutorium* for humerus, *furcula* for clavicle, *Focile Maius* and *minus* for the ulna and radius; the muscles are also indicated by their origins and insertions. thus *pars domestica* and *pars silvestris* describe the palmar and dorsal sides of the extremities, *rascetta* and *pecten manus* indicate carpus and metacarpus; and then there are the medieval Arabic terms *meri* for oesophagus, *Sifac* for peritoneum, and *Mirac* for abdomen." (Hopstock in Singer's Studies in the History and Method of Science, vol. ii, p. 155.)

In another place he says: "The force (muscular) originates in a movement of spirits (nerve impulse), which movement, gliding through the members of sentient animals (along the "perforated cords," that is, the nerves, as he has already frequently stated) thickens the muscles, in order that, when thickened, these muscles may shorten and draw to themselves the nerves (tendons) which are connected with them; and in like manner the force is produced throughout the human members." [4]

To-day we know perhaps more than this on the same subject owing to much modern research on nerve and muscle physiology. The velocity with which that "movement of spirits" traverses the nerves has been measured, later Alexander Volta named it, "nervous action" not knowing himself what it was; researches upon researches have been made, hypotheses propounded, theories formulated; but altogether we do not yet know at this day what it is that traverses the nerves and calls forth the contraction of the muscles.

HISTORY OF THEORIES OF MUSCLE CONTRACTION. As far as this last is concerned, we cannot find in the history of general physiology of the muscles another conception as exact as Leonardo's in its simplicity, until we come to Niccolo Stenone or Steno (Specimen Myologiae. Firenze, 1667, p. 16) who held against Borelli that "in every muscle, when contracting, a thickening occurs, even if the contracted muscle remains equal to the uncontracted one," and threw down all those ancient theories of "vital spirits," or of humors assumed to descend in the nerves from the brain, and accumulate in the swollen muscles, in the act of contraction.

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[4] To understand this orthology, recourse must be made to the preceding to bear in mind that when Leonardo speaks of *nerves* he means *tendons* and by *perforated cords* he means *nerves.*

Putting aside Cartesius (Descartes), who wrote fanciful things on this subject, Botazzi recalls that Borelli (De Motu Animalium. Romae, 1681, p. 36) cut a nerve under water to see if the famous bubbles of vital or animal spirits would come out; and seeing none he concluded: "Therefore, not by spirits belonging to the body are muscles made thick and moved." He also proved false the belief of those who held that muscles were thickened by blood, exuded by the arteries, which could not be taken up by veins, inventing for this purpose a pair of scales on which he had a man repose with his body evenly balanced upon the two pans. If the belief had been well founded, the pan under the feet or under the head should have gone down on account of the flow of blood into the muscles of that extremity when the man contracted the muscles of the legs, or those of the arms: "which, however, when the experiment was made," said Borelli, "did not occur." Notwithstanding these ingenious experiments, Borelli accepted the strange hypothesis of Malpighi, that from the brain, as from a gland, a humor is secreted which, through the nerve fibers, reaches the muscles; although Bartolini had previously shown that on ligating a nerve trunk with thread, it does not swell up on either side of the ligature. Accepting this "succo nerveo," Borelli imagined that in muscles "by the mixing of 'succo nerveo' with lymph, or with blood, a fermentation and gas production occurs similar to that which at times is observed in chemical operations" (Loc. cit. p. 63) ; and that this was the cause of the muscular thickenings. See Lectures on the History of Physiology by Sir M. Foster, p. 82–83, *Borelli and the influence of the new Physics.*

Who can tell what keen attacks upon these phantastic lucubrations Leonardo would have made, had he lived after they were uttered? How far he was from such errors is evident in this, that he uses at first the term "movement of spirits (moto spiritale)" and later simply "movement (moto)" to describe that which runs along the nerves and makes the muscles contract.

Something like Leonardo's conception is found in the "biotic fluid" which (as suggested by Newton, and very much like his "ether") many accepted as an explanation of the phenomena of life. Newton himself, however, had written (Quaestiones Optic. Patavii, 1773, Lib. III, p. 144): "Animal movement is produced by means of this ether, by vibrations which are excited in the brain by the power of the will, and thence, through the solid, pellucid, and continuous filaments of the nerves, are propagated to their muscles, causing them to contract and thicken." Newton held that all the filaments of the nerves are solid and uniform, so that the vibrant movement of the ethereal medium may be propagated through them uniformly and without interruption from one end to the other.

One could object that Leonardo calls the nerves "perforated cords," and infer from this that he believed in a circulation of fluids through them. But the inference does not seem to me legitimate. Often he uses ancient words to express new and correct ideas. It is incredible that in the numerous dissections that he made he did not discover that the nerves were not hollow tubes; then, too, he speaks of a movement (moto) that runs (scorre), and he does not speak anywhere of a fluid.

To us also the "nervous action" is a "movement"; ethereal, as Newton thought, and more specially electric, as Stephen Gray and Luigi Galvani first believed, or material, mechanical, chemical—we do not as yet know.

To us, as to Leonardo, muscular contraction manifests itself in a shortening and thickening of the muscle; without change in its volume, according to Stensen (Steno) and in consequence of its own capacity for contraction, as Albrecht von Haller and Lorenzo Bellini had thought. Bellini was a pupil of Borelli's and in 1662 discovered the urinary tubules in the kidney, when he was only 19 years old.

But let us come to the description of automatic movements, which is one of the best ever written by Leonardo: "Nature has made in man the voluntary muscles, tractors of the nerves (tendons) which move the limbs, subject to the will and desire of the sensorium, like officers located by a lord in various provinces and cities who in these places represent and carry out the wishes of their lord. But the officer, who in a number of cases has obeyed the spoken instructions of his lord, will afterwards do of himself, in similar cases, the things which are in accord with the will of his lord. Thus, often, one may see the fingers do, according to instructions, with most perfect obedience, on an instrument, things commanded by the judgment or will, and after they are once learned, they do them without any attention from the judgment."

"Do not the muscles also which move the legs perform their duty without the man being conscious of it?"

Only, Leonardo erred in thinking that the capacity to execute automatically movements, first repeated many times under the impulse of the will, resided exclusively in the muscles, when, in reality, it belongs to the whole motor-reflex apparatus, and particularly to the spinal cord. But we cannot require of him a knowledge of the functions of the spinal cord, an organ, unlike many others, of small bulk and deeply hidden in the canal of the vertebral column.

The axis cylinder or axone of nerves was first described by Remak, pupil of Johannes Müller in 1843. He distinguished medullated from non-medullated nerves and in his inaugural dissertation (1838) he made important contributions to the course of fibers in the brain and cord. In 1850 such a thorough anatomist as Kölliker, still held that the transit of impulse from sensory to motor fibers to and from the brain took place through the spinal cord in a continuous uninterrupted manner. The connection of motor nerve fibers with the large multipolar cells of the anterior horns of gray matter of the cord was first demonstrated by Deiters (1834–63).

Worthy of note are also these words of Leonardo: "This is very evident when you see paralytics and chilled and benumbed men, moving their shaking members, as head and hands, without the consent of the soul, which with all its force cannot prevent the members from shaking. The very same thing occurs in epilepsy, and in several parts such as the tails of lizards."

Leonardo gave a definition of the voice and how it is produced.

"Voice is a current of air rubbing harshly (confricata) upon a dense body or a dense body rubbing harshly upon air (it is the same thing either way), and this harsh rubbing of dense with rare condenses the rare and makes resistance; and likewise swift rare and slow rare, on contact, are both condensed, and produce *sound,* sometimes a tremendous roar. The sound or murmur is made when rare moves in the rare, with moderate velocity, as a great flame, producing sound in the air about it; the tremendous roar is made by rare with rare, when the swift-rushing rare penetrates into mobile rare, as the flame issuing from the throat of a cannon and smiting the surrounding air, and as the flame issuing from a cloud, also, which smites the air, with the generation of the thunderbolt."

Further on he adds: "The wave of the *voice* goes through the air, as the images of the objects go to the eye." So we see "there can be no *voice,* where there is not a movement or percussion (vibration) of the air; there can be no percussion of the air, where there is no instrument; there can be no instrument without material substance."

All this he said, to demonstrate, as though addressing even the modern spiritualists, that "a spirit cannot have either voice, or form, or physical strength, and, if it should assume a body, it could not penetrate or enter where the entrances are closed. And if one should say: that by means of air compressed together and confined, spirits obtain bodies of various forms, and with these instruments speak and move with force:—to this I answer, that, where there are nerves and bones, force cannot be exerted, in any movement, made by imaginary spirits."

Müntz observes that Leonardo did not show any interest in moral and social questions. In regard to the latter, it seems probable that he did not concern himself much, although his "facetiae," and "prophecies" and "fables" abound in satirical allusions and sometimes hit hard at persons and events of his times.

But that Leonardo often speculated on moral problems, can be clearly seen from his writings. I remember, in the first place, the exclamation contained in the following passage from his *Anatomy:* "O that Our Creator might grant that I might demonstrate the nature of men and their customs, just as I describe their figure." Many are the places where he speaks of the commandment not to destroy this "marvelous device," the human body, and of virtue in general, and of doing good to one's neighbor. "I never tire of doing good," he says, "and of bearing with patience great injuries"; also about working on festival days, a passage which draws its inspiration evidently from the Gospel, and which I cannot help quoting, such is the sincerity that permeates it.

"Let these cavillers keep silence (who criticize those, especially artists, who work on church festival days) because this is the way to know the Creator of so many wonderful things (that is to say 'of acquiring a knowledge of the works of Nature, with earnestness,' as he said before), and this is the way to love so great an Inventor! Indeed, great love is born of great knowledge of the thing which you love: and if you do not know it, you can love it little or not at all. . . ." In my study of Leonardo da Vinci in Chapt. XXV, I have defended this sentence against the misconstruction of Sigmund Freud.

The thoughts of Leonardo on pleasure and pain, and particularly those on the function of pain in nature, which are more closely related to physiology, must be briefly referred to.

What he says of pleasure and pain resembles too closely the "Phaeton" of Plato for us not to notice how entirely he followed the ideas of Plato. "Pleasure and Pain—they are represented as twins, because never is one severed from the other; they are represented back to back (or perhaps with their backs in opposite direction), because the one is contrary to the other; they are represented with a common trunk, because they have a common foundation; in fact, the foundation of Pleasure is relief of weariness caused by displeasure, the foundations of displeasure are the manifold lascivious pleasures." This biological function of pain is a theory like the modern—experience from trial and error, "Nature has placed in front, as man moves, all those parts which, when struck, cause distress; and this is felt in the shins, and in the forehead and nose; it is done for the preservation of the man, since, if such pain were not provided in those members, certainly the many blows received in these members would result in their destruction." He goes on to speculate why plants do not feel pain: "If nature has ordained pain, in vegetative souls that move (the animals), for the preservation of their organs; which otherwise might through motion be made less efficient or destroyed; the vegetative souls without motion (the plants) are not liable to strike objects placed in front of them, therefore pain is not necessary, and therefore when struck they do not feel pain, as the animals do."

By many wise expressions, and in various places, Leonardo has voiced his convictions concerning the conservative function of pleasure and pain. Here are some of them:

"Sexual pleasure is for the propagation of the race." "Appetite is for the maintenance of the life." "Fear, or apprehension, is for the continuation of the life." "Pain is for the protection of the organs." And to these Botazzi adds another, most beautiful expression: "The greater the martyrdom, the more refined the martyr."

If Emerson had known, as we do, the work of Leonardo, would he not have added to his "Representative Men" one more chapter: "Leonardo, the Naturalist-philosopher"; or would not Carlyle, to-day, place him among his "Heroes"?

Religious he was, in the sense that Carlyle wished it in his heroes, because no one had a knowledge more complete "concerning his vital relations to this mysterious Universe, and his duty and destinies there." "Simple, open as a child, yet with the depth and strength of a man": so he is described by all writers. And we can truly follow that great English thinker: "To the wild deep-hearted man all was yet new, not veiled under names or formulas; it stood naked, flashing in on him there, beautiful, awful, unspeakable. Nature was to this man, what to the Thinker and Prophet it forever is, *preter*-natural."

Were not these words written of Leonardo?

In immediate contact with nature, he understood it completely, the artist and the philosopher: "a marvelous instinct preserved him from almost every error, as well in method as in interpretation (E. Müntz, p. 314) within the limits of the possibilities at his time."

He was one of those heroes, who can say of themselves: "The currents of the Universal Being circulate through me; I am a part or particle of God" (R. W. Emerson's Works, Article on *Nature,* p. 548).

ON THE HEART, CIRCULATION

In January, 1916, Arnold C. Klebs published a highly interesting article in the Bulletin of the Society of Medical History of Chicago, in which he expressed the following words (1 c. p. 66): "The fact that nothing from the hand of Leonardo has reached us in a completed, well-ordered form makes any investigation very difficult." The announcement of the publishing house of Jacob Dybwad of Christiania bears the date of May, 1914, in which is announced the completion of Leonardo da Vinci's Quaderni d'Anatomia, I–VI, Fogli della "Royal Library di Windsor," Anatomia e. Fisiologia, Edited by Ove C. L. Vangensten, A Fonahn, H. Hopstock. In the same number of the Bulletin, Fielding H. Garrison wrote an article (on page 47) in defense of Vesalius, defending him against the charge of E. Jackschath who had opened a controversy declaring that the *Fabrica* of Vesalius was a gigantic plagiarism from the anatomical drawings of Leonardo da Vinci. This whole matter had already been discussed in 1906 by J. Playfair McMurrich (in the Library and Historical Journal, Vol. IV, p. 338). All of the articles mentioned are highly to be recommended for Leonardo students, for few of them could make use of the phenomenal work of Vangensten, Fonahn, and Hopstock, in its completed form.

In studying the circulation, and the structure of the heart as far as Leonardo knew it, this great publication is indispensable. An article which is based upon this anatomy was published by one of these three editors, H. Hopstock, in the "Studies in the History and Method of Science" by Charles Singer (1921, Vol. II, p. 151). In the beginning of his article H. Hopstock demonstrates the superiority of his monumental arrangement of Leonardo's drawings. As a rule this publication is briefly referred to as the *Quaderni* whilst the publications of Paris in 1898 are called *Fogli A* and the editions from Turin of 1901 are designated as *Fogli B*. The two together, *Fogli A* and *Fogli B,* constitute the edition of the Russian Sabachnikoff, and the Italian Piumati, with a preface by the distinguished French anatomist, Duval. The *Fogli* only have a French translation of Leonardo's descriptions while the *Quaderni* have, in addition to the original Italian, also German and English translations. The three editors are not only experienced philologists, but also experienced anatomists.

The use of an older transcription of Leonardo's encyclopedic endeavors to write a system, as he calls it "Anthropologic-Cosmologic Encyclopedia," by the physiologist, H. Boruttau (Berlin), involved him in an unpleasant controversy with these three editors, Vangensten, Fonahn, and Hopstock. (See Sudhoff's Archiv für Geschichte der Medizin, Vol. VI, article by Heinrich Boruttau, "Leonardo da Vinci's Verhältnis zur Anatomie und Physiologie der Kreislauforgane" p. 233, and article by Ove C. L. Vangensten, A. Fonahn, H. Hopstock, "Zu Heinrich Boruttau's Artikel: Leonardo da Vinci's Verhältnis zur Anatomie und Physiologie der Kreislaufsorgane'" p. 397).

The compilation and transcription which Boruttau used were the ten volumes of Rouveyre's "Reproductions" published in Paris in 1901. In this work there are facsimile reproductions arranged according to organ and body regions, but without any transcription or translations of any kind. When Boruttau wrote the article on the "Physiology of the Circulatory Organs" as conceived by Leonardo da Vinci, only the first fasciculus or "Heft" of the *Quaderni* had been published. Therefore, Boruttau was compelled to use the other compilations, especially that edited by Rouveyre. As these ten volumes had met with a complete condemnation by Vangensten, Fonahn, and Hopstock, the physiologist Boruttau incurred the opposition of these three editors to his articles written in Sudhoff's *Archiv*. As both parties concerned are eminent authorities and universally acknowledged as thorough investigators, the polemic between them was thoroughly studied by me to ascertain whether historic truth and scientific accuracy had been enhanced thereby.

In studing the physiology of the heart and circulation as understood by Leonardo, we find ourselves suddenly on one of the borderlands between philologic knowledge on the one side and anatomic and physiologic knowledge on the other. The translations in the *Quaderni* cannot be improved upon. They are simply perfect, but at the same time the physiologic deductions of Boruttau are similarly faultless. The editors of the *Quaderni* convict Professor Boruttau of a number of linguistic inaccuracies. Text criticisms, although extremely valuable to the philologist, are after all only adjuvant to medical historical research. Errors in transcription and translation may bring a certain distortion of physiologic facts with them, but in this discussion between Boruttau and the three editors, I have become convinced that no serious distortion of an anatomic or physiologic nature has occurred. The majority of the incorrect readings which are charged by the three editors, change nothing in the context nor in the construction of Boruttau's concerning the physiology of the circulation as Leonardo understood it. I state this discussion so explicitly in order to forestall any future reproach, that I had based 1926 deductions of this subject on exclusively personal research and not tested the sources upon which experienced historiographers must found their investigations.

LEONARDO DA VINCI'S PLAN OF A GREAT ANATOMICAL TREATISE

In *Fogli A 17-R,* Leonardo states "questa vernata del mille 516 credo spedire tutta tal anotomia," "this winter (1510) I believe I can send off this whole anatomy." He also speaks of 120 volumes, but there is another reference to an outline of a work on anatomy. This occurs in *Fogli B,* p. 129. Hopstock speaks of this outline as *the plan for the book.* The Italian title is "Del Ordine del Libro." In fact Vasari, whom I have found to be a reliable recorder of the Cinquecento artists, actually states that Leonardo had a plan for a great work on anatomy. What we in reality have had up to the publication of Vangensten, Fonahn, and Hopstock—which is a coöperation of philologists, anatomists, and physiologists—was nothing but a gigantic conglomeration of sketches and notes, partly numbered pages, anatomical studies, mingled with mathematical and architectonic subjects, so that it was indeed very difficult to make headway on any particular physiologic subject prior to the great Swedish *Quaderni.* It is difficult to speak of his conception of the circulation without going too much into anatomy. In three of the *Quaderni* Leonardo gives evidence of the study of the structure of the heart and the movement of the blood through and in it, and through the larger blood vessels.

For the most part it appears that Leonardo's investigations were made on the hearts of animals, particularly oxen and sheep. Every now and then there are accurate drawings of the human heart which he draws in its correct conical shape with the base upward and to the right. He describes the coronary veins, which he calls "vene nere" because of their dark color. On the surface of the heart in his drawings are seen the coronary arteries as well as the coronary veins. The arteries lie deeper than the veins, but some of the former lie above the coronary veins (*Quaderni* II, F. 1, r). These arteries feed the heart substance (*Quaderni* II, F. 4 r) and they arise from the aorta.

More than one-fourth of all the anatomical and physiological drawings in the six *Quaderni* volumes, treat of the heart, its structure, conformation of its cavities, the mechanism of its contraction, the valves, and of ceaseless efforts to trace the course of the blood. In his endeavors to succeed in this, he combines a highly developed technique of dissection, a penetrating insight, and a rare knowledge of physics. The student of Leonardo cannot fail to be impressed with the innumerable points of inquiry (Fragestellung). Authors are cited by him and their assertions tested from every angle. The drawings, sketches, notes, discussions, and experiments on the heart would fill a volume as large as Singer's "Studies in the History and Method of Science" and over 600 pages. In fact, Leonardo states, "Which are the words in which you describe this heart so that you do not fill a book, and the more minutely and elaborately you describe it, the more you will confuse the mind of the listener. Many times you will require a commentator or have to revert to experience which in your personal case has been very limited, and can explain only few things concerning the subject in its entirety, a subject about which you desire full knowledge." (This reminds one of the 12th verse in the 12th Chapter of Ecclesiastes: Of the making of many books there is no end and much study is a weariness to the flesh).

I have already quoted Charles Ravaisson-Mollien in his "Les Manuscrits de Leonard de Vinci" as evidence that Leonardo must have studied the course and direction of the ventricular muscles in a manner not much different from that of F. P. Mall and, preceding him, of MacCallum. He also had a complete knowledge of the cardiac anatomy as far as it can be perceived by the naked eye. He states that the heart has four ventricles, and according to Holl, da Vinci was the first to introduce the designation *ventricle* for the cavities of the heart. Before that, other expressions were in use, such as *concavitas cordis* and *vacuitas* or *sinus*. All of the four heart chambers are called ventricles sometimes, but sometimes he calls the upper ventricles, which we now call *auricles*, HEART-EARS (Orechi). He gives a very fitting description of the expansion of these "ears of the heart" when the ventricles contract because he assumes that a portion of the blood goes backwards through the auricular-ventricular valves while the greater part of the blood goes forward into the aorta in the left side and the pulmonary artery on the right side. He calls the impetus imparted to the blood by the contracting ventricles the PERCUSSION. When the left ventricle contracts, the blood goes through the aorta (Q. II. f. 17. v. the upper vessel), and the blood wave thus formed (pulse wave) goes through all the arteries and with the pulsation of the blood in the heart which closes the valve a sound is produced which goes through every artery and which the ear can hear in the temples (Q. II. f. 3. r.). The semi-lunar valves open when the blood enters and close after it has passed out into the aorta. He speculates as to the extent by which the openings of the heart could be closed by muscular action exclusively in the absence of valves, and he concludes that the valves effect the closure more completely and promptly. But that this question should have arisen in his mind at all, is an astounding revelation concerning this master, for it is one of the most modern problems of cardiopathy as to whether imperfections on diseased valves could be obliterated by extraordinary concentric contractions of the cardiac muscles around the ring to which the valves are attached.

WAX, GYPSUM AND GLASS CASTS OF THE HEART. (Q. II. f. 12. r.)

In Q. II. f. 12. r., he describes "a form of gypsum to be inflated and a thin glass within and then break it off from head to foot" and further on he states "the form of the glass to see in the glass what the blood does in the heart when it shuts the openings of the heart." It appears that he first made a wax cast of the ventricles and their vessels, over this he made a gypsum cast, and from this he made a glass cast. Through this latter he watched the whirls created by the blood when it is driven out by the systole into the aorta on the left side and into the pulmonary artery on the right side (Q. II. ff. 12. r. & 13. v.). It is not understood how he could see any whirls in the blood. They can be made visible in artificial heart models made of glass and metal when the liquid is transparent, for example, a thin solution of gelatine in which lycopodium or other visible particles are suspended. His gypsum must have been not simply calcium carbonate or plaster of paris, for he says it could be inflated. He must have invented some material which, whilst it retained its form, was elastic enough to bear a certain degree of distention.

In Q. II. f. 9. v. he gives glass casts of the semi-lunar valves in different positions to show their action, and in Q. IV. fi. iv. he gives a diagram of the semi-lunar valves with the whirls and eddys in the blood marked out in the form of spirals in the current. Three circles in the lower left-hand corner of this latter drawing are marked with geometrical figures which are lettered suggestive of some mathematical calculation as to impact force and direction of the blood current.

An experiment which could not be improved upon by modern physiologic technique is Leonardo's study of the contractions of the living heart. He observed that metal levers thrust into the myocardium of the living heart moved with the action of the heart: at the diastole the point of the lever in the ventricle goes upwards, and its handle downwards, with the shortening of the heart,—the contrary with its lengthening. With the cessation of the motion of the heart, the lever becomes stationary in the exact middle of the two extremes.

Are we justified to draw the conclusion from Leonardo's anatomic studies of the heart, and from his verbal descriptions, that he understood the circulation of the blood? Jean Paul Richter, in "the Literary Works of Leonardo da Vinci" (London, 1833), has drawn the conclusion that he knew or at least divined the blood circulation. Arnold C. Klebs,

(l. c. p. 77), concludes that he does not seem to have arrived at a clear conception of the circulation of the blood. Marie Hertzfeld in her book on da Vinci repeats the assertion that he knew the circulation. Pagel, however, considers this assertion to be a pure hypothesis, and Bottazzi, in several Italian essays, does not consider this question at all which he would if he believed that da Vinci had understood the circulation, because Bottazzi is not only a countryman of da Vinci, but a very brilliant physiologist. Roth ("Archiv f. Anat. und Physiol., 1907, Supplement S17") considers it improbable that Leonardo understood the circulation, but Holl, in opposing him, interprets a passage from Leonardo as indicating that he did have a conception of the circulation. This sentence is the following: "Just as the blood, which returns when the heart dilates, is not the same blood which closes the gates of the heart (we must remember here that he means by the gates of the heart, the valves). . . ." Such a brief remark is not sufficient to warrant drawing any conclusions from it. Moreover, there are distinct statements by Leonardo and even drawings which are so erroneous as judged by our modern conception of the circulation, that it is actually impossible to attribute to him a correct knowledge of the blood currents through the heart and through the body.

It cannot be denied that at times the anatomic connection of the large blood vessels on the venous as well as on the arterial side of the heart, was not clear to him, and even the connection of the large vessels of the lung was not clear to him. For example, he speaks of the dependence of the expansion and contraction of the lungs upon the heart movements. The air currents in the lung, therefore, are dependent upon the contractions of the heart, according to him. He even describes the presence of air in the pericardium and the exchange of air with the contents of the heart ventricles directly. These drawings and texts are very clearly shown in the Quaderni of the Christiania edition. While the illustration on p. 31 of Hopstock's article taken from Q. II. f. iii. v., is a fairly correct drawing of the outside of the heart with the coronary vessels, the drawing in Q. H. f. i, r., right underneath it, the dissection of the bronchi and bronchial vessels, is a confused delineation. The same can be said of the schemas of the vascular supply of most viscera, except perhaps that of the kidneys. In some of his drawings the bronchial tree enters the uppermost tip of the lung instead of the hilus. In one of the feuillets of Sabatschnikoff there is a representation showing the right heart ventricle to communicate with the right lung, and the left heart ventricle communicating with the left lung. The Christiania edition uses a vignette in its title-page which is found in No. 5 of the Rouveyre publication (*Nerfs et Vaisseaux*). This correctly shows that the left ventricle gives rise to the aortic arch, and that the upper and lower vena-cava enter the right auricle —so far, this is splendid, but on closer examination we find that this same right auricle is the termination of a vessel that comes from the right lung, which is a regrettable error, unless further studies will reveal it to be the pulmonary artery.

The coronary vessels which Leonardo described and draws so accurately are also thoroughly understood by him. He speaks of them in the following words, "Quelle vene che nutriscono il cuore sempre sono sopra esso core; lor parti laterali son circondatidi grasso sevoso, cioe grasso denso" (Rouveyre, "Notes dessins sur la Cœur," Fol. 2). The coronary vessels, then, serve for nutrition of the heart. This is modern in view, but we miss the distinction of these vessels into arterial and venous coronaries, and when we investigate the designation which he gives to the large blood vessels, we find that they are identical with those given by Galen. Concerning the right ventricle he speaks in a confused manner. He says "The right ventricle has two openings one of which opens the aortic artery from within outward (He means the aortic valve). The other is the opening of the venous-like artery. (Arteria-venosa of Galen) which rises up to the lung." The most generous interpretation of this sentence is that Leonardo committed a slip of the pen and wrote right ventricle where he should have written left ventricle. It is however apparent that at times the correct anatomic connection and course of the large vessels with the heart chambers and the lungs was not always clear to him. Even in the Christiania edition in the section on the ventricles of the heart, he states that the lower chambers of the heart *are separated by a partition which is porous and through which the blood penetrates from the right into the left ventricle.*

A few modern medical historians, among them Arnold Klebs, and J. Playfair McMurrich, are not sure that da Vinci had come under the direct personal instruction of Marcantonio Della Torre. I will not discuss this question here, but simply state that Sudhoff in his *Gesch, d Med. im Überblick* (page 280), speaks of this anatomist as Leonardo's *Lehrmeister.* Contemporaneous documents indicate that Della Torre, who died very young, only thirty-three years old in 1506, was a very gifted anatomist. But he was an ultra-galenic anatomist. This explains why Leonardo could not wrest himself loose from the errors of Galen concerning the circulation of the blood. In the table by Rouveyre, in the upper right hand part, Leonardo draws a beautiful scheme of the four heart chambers in which he indicates the pores of the septum by fine, horizontal, straight lines, drawn through this partition (See also Q. I. f. 8. r.). But in

Q. IV. f. 11. v., he states that he was unable to find these meati himself and speaks of them as being invisible.

THE POWER OF TRADITION AND EXCESSIVE REVERENCE FOR AUTHORITIES AS A CAUSE OF HISTORIC ERRORS IN ANATOMY AND PHYSIOLOGY

This is nowhere more clearly shown than in Leonardo's struggle with Galenic opinion, which in that period amounted to a fetish. In my "History of the Circulation of the Blood; Contributions of the Italian Anatomists and Physiologists; their bearing upon the Discovery of Harvey," see Chap. XI it is stated that Galen (125–201 A. D.) had cleared up the error of Erasistratus (300 B.C.) that the left heart and the arteries were always entirely empty and served the purpose to propel the vital spirits (pneuma) from the lungs into the various parts of the body, and that the left heart did this through the small bronchi which stood in connection with the *arteria-asperia*. This is the term given to the trachea, and only the veins contained blood. Galen showed by his vivisections that when the left heart or any artery was stuck with a sharp instrument, blood issued forth in streams which was different from the venous blood, for it was vaporous, thin, and genuine, and therefore it must consist of a mixture of the blood with the air drawn from the lung. *Mixtum quid ex ambobus.* Galen assigns the starting point of all arteries to the left heart, which by its systole drives the air containing blood (sanguis spirituosus) into all parts and organs of the body to revive them. The center or starting point for the veins, however, is the liver, from which the nutritive blood (Sanguis nutritivus) by a kind of attractive and selective force is led to all parts of the body. The blood of the right heart received from the vena-cava for the most part goes into the left ventricle, through pores in the septum (which Galen assumes theoretically, for he declares that they are invisible). Here by a mixture of the aerated blood, it becomes "spirituosus" and is distributed through the aorta to the whole body; but a small part of the blood contained in the right ventricle goes through the "Vena arteriosa" (pulmonary artery) and reaches by way of the arteria venosa (pulmonary veins) the left ventricle.

According to this, Galen had a conception of the pulmonary circulation, although it was an incomplete conception. He knew that the veins anastomose somewhere with the arteries, and he had observed accordingly that an animal could bleed to death from a single artery. There are three serious errors in the doctrine of Galen which his colossal influence perpetuated until the time of Harvey, 1578 to 1657. The first error is the assumption that the blood of the pulmonary vein flowed back into the veins with every systole by a kind of physiologic insufficiency of the bicuspid valve, and when it got to the lungs it became freed of the "soot." In this way he attributed to the pulmonary vein a double and diagonally opposite purpose, first to bring the revivified blood from the lungs to the heart, and then again to return a part of this same blood from the heart to the lung. This is called the error of the systolic reflux. The second great error was that Galen attributed to the portal vein a similar double and antagonistic function, for during digestion this vein transported the chyle from the intestine to the liver, but when the intestinal canal was empty, the same vein transported the blood from the liver to the intestines. It is sure that these two errors together with his hypothesis of the permeability of the septum obscure the splendor of this great anatomist's and physiologist's doctrines concerning the lesser circulation. But it cannot be denied that he was the first who had any conception of the lesser or pulmonary circulation, and later physiologists for example Harvey, Maurocordato, Douglas, Haller, acknowledged this, basing their belief on a passage occurring in Galen's book, *De Usu Partium,* 10th chapter. It is frequently said that Vesalius in accepting without denial Galen's error of the permeability of the septum spoke of the pores ironically when he stated that they were invisible, and admirers of Vesalius believe that he expressed his doubt of their existence by this irony, but I have already quoted Galen as himself declaring that they are invisible. Therefore Vesalius accepted in unchanged manner the phraseology of Galen.

There is also a contradiction in the arguments of Leonardo concerning the complete normal closure of the atrio-ventricular valves and the to and fro movement of the blood between the ventricle and the auricle. He must have been struck with the dilemma of harmonizing his drawings and experiments concerning the exact fitting of the atrio-ventricular valves and the idea that some of the blood regurgitated from the ventricle back into the auricle when the ventricle contracted. It is true he states that this regurgitation of blood into the ante-chambers occurs before the atrio-ventricular valves have closed entirely (Q. II. f. 3. v.). But he assumes that these valves are stretched and increase somewhat in size during the ventricular contraction, and that thereby a complete closure of both the right and left atrio-ventricular aurifices is brought about. This conception of stretching and increase in size of the valves is an assumption of Leonardo's (Q. II. f. 3. r. 8. v., 11. r., 12. r). The valves of the heart close absolutely tight to blood without being stretched or increasing in size. In fact there is an experiment in which an electric light is sewed through the myro-cardium and rests in the interior of the ventricular cavity. A circular glass window held in a brass frame is tied securely at the place where the pulmonary vein enters the left auricle. A clear circulating medium such as Ringer's solution is used so as to permit the experimenter to look through this clear medium into a highly illuminated ventricle. Through the same channel which holds the insulated conducting wires for the electric lamp a hollow tube is passed. The circulating medium is injected by means of a small rubber hand pump into the ventricle. The observer is then capable of seeing that the heart valves close instantly and absolutely when the intra-ventricular pressure reaches a certain height. I have performed this experiment on the heart of large sharks without the hand pump, and the heart executing its own normal contractions. Still the time and the extent of the absolute closure of the auricular-ventricular valve was a matter of discussion among physiologists up to twenty-five years ago, and could not have been expected of da Vinci as a perfect and dogmatic solution of this problem. In Q. II. f. 17. v. he states that with the systole of the right auricle *another* portion of the blood goes through the pulmonary artery to the lung, while a third portion passes through the septum into the left ventricle. He thus conceives of three currents of blood when the right ventricle contracts. One current goes into the pulmonary artery through the lung, another goes backwards into the right auricle, while a third passes through the septum into the left ventricle. That he could not have grasped the circulation as it is understood to-day is evident also from the following sentence: "Less blood is driven back from the right ventricle to the right auricle, than goes from the auricle to the ventricle, and the right auricle has its quantity of blood made good from the vena-cava inferior through the liver which is the treasurer and the generator of the blood."

In his article on Leonardo as an anatomist (in Studies in the History and Method of Science, edited by Charles Singer, Vol. II, p. 180, and p. 181), H. Hopstock expresses the belief that da Vinci had a fairly correct conception of the circulation in spite of errors and omissions, but we have cited enough of his errors to supply the evidence that it was absolutely impossible for da Vinci to understand the circulation in its entirety. This eminent artist and actual creator of anatomic illustration who gives in admirable detail the entire valvular apparatus of the heart, missed grasping the complete problems of the circulation of the blood for two reasons: First, because he made no experiments on the living animal, and it was impossible for him to see the capillary circulation before the microscope was discovered; second, because he could not emancipate himself from the tyrannical domination of the authorities of his time, particularly Aristotle, Galen, and the Arabic physicians. He seeks to bring their fundamental physiologic opinions into harmony with his anatomical findings, and thus becomes involved in a false interpretation of the facts.

I will conclude the discussion of Leonardo da Vinci's work on the heart and circulation by a reference to one of the most brilliant discoveries of a part of the heart that he has made. In Q. I. f. 14. r., he sketches the intra-ventricular moderator band which takes its origin in the septum and is attached at the base either of papillary muscles or the trabecula. In Q. IV f. 13. r. Da Vinci calls this band the *catena* and he argues that its purpose is to prevent over distention of the heart, for without this structure the heart would draw too much blood from those vessels into which it had previously expelled it. (See Plates XXXIII, XXXIV, XXXVI). Holl points out that Leonardo was the first to observe these fibers passing through the ventricular cavities and suggests that, in honor of their discoverer, they should be called Leonardo da Vinci's columnae carnae. Tawara in 1908 first pointed out that these fibers form bridges through which the fibers, the atrio-ventricular bundle of His, reach from the septum to the papillary muscles.

Leonardo even has thought concerning what modern physiologists term the neurogenic and the myogenic origin of the heart contraction wave. He speaks of this when he investigates the relation of what he terms the recurrent nerve to the heart (Q. IV. f. 7. r.). He tells us that it is important to learn whether this nerve imparts movement to the heart (neurogenic) or whether the heart contractions are spontaneous (myogenic). He concludes that the heart contractions and dilations are spontaneous, and describes that they occur in direction of the longitudinal axis of the heart. It would appear, therefore, that on the whole he favored the view that the heart contraction wave is due to some quality inherent in the heart muscle.

RESPIRATION

In the preceding we have learned that Leonardo calls all heart chambers ventricles. He says there are four heart ventricles, two upper and two lower; thus even the cavities which we now term auricles are by him designated as ventricles. He discourses at length that the upper ventricles are not one and the same with the lower ventricles, and in the edition of Rouveyre (notes et Dessins sur le Cœur, f. 2), he presents arguments *on the right ventricle* tending to prove that it actually presses blood through the *meati* of the septum into the left ventricle, but in the f. 4 of the Christiania edition, he presents a very extensive argument that tends to show that not as much blood regurgitates into the right auricle as originally came out of it into the right ventricle because some of this blood has passed through the septum into the left ventricle. "And therefore," he continues, "new blood must be sucked up through the vena chyli (inferior vena-cava) from the liver." The significance of the right auricle is dwelt upon, on the same leaf on the reverse side. Leonardo says that the flood and ebb of the blood could not occur if the right auricle gave up so much blood to the right ventricle as it receives from the liver. "This movement of the blood resembles a lake through which a river flows. The lake receives as much water from one side as it loses from the other, but it is distinguished in this feature—that the movement of the blood is discontinuous but the movement of the river through the lake is continuous, and if there were no ebb and flood the blood could not be heated, and in consequence thereof, the *spiritus vitales* could not be formed and life would be destroyed."

It is evident that he assumed that part of the blood is constantly consumed for the formation of the "vital spirits," and therefore a constant supply of new blood from the liver or its sources (intestines, chylus) must be brought into this hypothesis. This is exactly the doctrine of Aristotle and Galen. There is no mention of a circulation, only of an ebb and flood.

This imperfect conception of the action of the right side of the heart prevented him from gaining an insight into the pulmonary circulation, and together with this he failed to understand the function of respiration. True, he might have penetrated to the mechanism of the lesser or pulmonary circulation without understanding the chemistry of respiration, just as Michael Servetus did (See Chapter xx on Servetus—Discovery of Pulmonary Circulation. His Life and Work.) The chemistry of respiration could not have been understood before oxygen and carbon dioxide were discovered (See the chapter xxi Lavoisier and the History of the Physiology of Respiration and Metabolism. Contemporary Views of Life Processes).

Galen discusses two contrary or reciprocal functions of respiration. One is the cooling effect and the other is the respiratory rôle of maintaining the inborn fires which he finally permits to stand side by side without explaining how one and the same function could heat and cool at the same time. In the discussions of the Arabic physicians one finds this same question spun out indefinitely. In his theory of respiration, Leonardo again shows himself bound by the might of tradition, for he says, "Thus by the ebb and flood which proceeds with great rapidity, the blood becomes heated, and refined (si assottiglia) and finally it acquires such high temperature that the entire blood would reach such a degree of heat that the heart would smother and its life would be ended if it were not for the help of the bellows which we call the lungs, which by their expansion draw in fresh air and then press it on, touching the walls of the arterial branches, refreshing them." That he was acquainted with the anatomy of the pulmonary artery, which he calls the "door of the lung," is obvious from the passage from Q. II. f. 2. v. where he says, "and its opening has three valves, which open from within outward with perfect closure, and these are in the right ventricle."

He raises the question in Q. I. f. 5. r. whether the pulmonary veins send back the blood to the heart when the lung contracts at the expulsion of the air. Evidently he understands that the sending back of the blood from the lung to the heart occurs and depends upon *expiration*. But one of the most beautiful insights is his perception that it is the contraction of the diaphragm which compels the lungs to expand (Q. I. f. 4. v.).

Leonardo's knowledge of the lungs (bronchi, trachea, and the muscles of respiration) is based upon the dissection of animals (e. g. Q. II. f. 1. r.) and of man (e. g., Q. II. f. 7. v.). He describes the lungs and their lobes as being enclosed in a delicate membrane, the pleura, which interposes itself into the spaces between the ribs when they expand. The pleura also covers the inner side of the ribs and the surface of the diaphragm. He experimented with the inflation of the lungs and denies the possibility that air could reach the heart from the bronchi. The only possible effect of the freshness of the air is that received by the pulmonary artery from the bronchi. It is clear that he put before himself the *Fragestellung* whether or not any air can penetrate into the heart from the bronchi directly, for he has experimented with blowing up the bronchi in order to ascertain whether any air escaped into the heart during this distension, and he concludes, "The lung is unable to transmit air to the heart," and further, "The air enters the lung continually dry and cool, but leaves it moist and hot." But the arteries which supply themselves in a continuous network with the ramifications of the bronchi that are distributed throughout the lung, are the vessels which take up the "freshness of the air" which enters the lung. One of the finest studies for the medical historians is Leonardo da Vinci's distinction between expiratory and inspiratory muscles of respiration, which are accompanied and illustrated by numerous admirable drawings (Q. I. f. 5. r. and 8. r.).

Arnold C. Klebs has already drawn attention to da Vinci's ingenious devices for the demonstration of difficult anatomic relations. For instance, he illustrates the complex action of antagonistic muscle groups by means of tape models, the schematic drawings of which make clear flexion, extension, pronation, and supination. It is very important to bear in mind that when Leonardo's drawings are correct, and his deductions are erroneous, that a close and persistent examination of his notes frequently reveal that he did not intend his conclusions to be final. They were put forward tentatively merely as working hypotheses to be corrected at some future time when additional material for dissection had been acquired.

The internal intercostal muscles are defined as expiratory, for they proceed obliquely outward and forward, but the external intercostal muscles are inspirators for they run in the opposite direction. Furthermore he states that the intercostal arteries, veins, and nerves pass between the ribs. All this is illustrated by drawings. The scaleni muscles and the serratus anterior are defined as inspiratory muscles, and that they prevent the diaphragm from drawing the costal cartilages inward. He describes the serratus posticus superior as an auxiliary muscle coming into play during deep inhalation. He mentions that this muscle is made up of six muscle bundles, three on either side, passing from the vertebral column to the uppermost rib. His sketches of the osseous structures of the thorax are almost perfect, and he describes the cartilage rings of the trachea which are very elastic, comparable to springs, and incomplete posteriorly where the œsophagus passes (Q. I. f. 9. r.). He describes the muscles which act on the larynx during phonation, and gives many drawings and sketches of them. The production of sound is considered in drawings and in the text. He also calls attention to the position of the lips and of the soft palate in the formation of vowels, and actually sets up a table of vowels arranged in conjunction with the different consonants.

Leonardo does not use the word larynx, calling it by different designations such as the *upper part* of the *trachea,* or the *fistula* or flute, that is, the place where the voice is formed. Sometimes he refers to the *epiglottis* as the part forming the voice. His comparison of the trachea to an organ pipe would be a good didactic simile if he had not added that he thought the voice could be modulated by lengthening and shortening, or the expansion or contraction of this organ pipe. It appears that he has pressed air in and out of the lung to see whether by contracting the "fistula," the generator of voices, sound could be produced. To the tongue he assigns great versatility of function. Twenty-four or twenty-eight muscles enter into the composition of the tongue, which he attempts to trace in their origin and insertion. He dwells on its function in speech, articulation of syllables, mastication, cleansing of the mouth and teeth. Concerning the function of the larynx and voice in Leonardo da Vinci see C. L. Vangensten, "Leonardo da Vinci og fonetiken," Videnskabsselskabets Forhandlinger, No. 1, 1913.

It is no detraction from the admirable methods of thinking on physiology and anatomy which we read in those works of Aristotle and Galen which have come down to us to say that their authority remained unquestioned throughout the Byzantinic-Monkish medicine of the Middle Ages and that this period was entirely too long for such views to hold despotic sway over the medical mind. This sycophantic idolatry of Aristotle and Galen's views lasted all through the pre-salernic period. In other words, it was still dominant from the fifth to the ninth centuries, inclusive, and medicine of this period has received the designation "Monk Medicine" and sometimes the "Latino-Barbaric Medicine." I refer the reader to Karl Sudhoff's excellent presentation on "Erste Aneignung Antiker Medizinischer Hinterlassenschaft in Westeuropa," page 172 in his "Geschichte der Medizin im Überblick mit Abbildungen," and also the other articles in the same book showing the new foundation of physiology and the views held by the various schools and masters to the time of Harvey. There is also a critical history of the various theories of respiration by H. Boruttau in "Archiv d. Geschichte d. Medicin," Vol. II, February, 1909, p. 301, which illustrates in a very convincing manner the utter impossibility of the medical thinkers of the periods just mentioned to have even a remotely correct view of the chemistry of respiration.

It must be evident that the period in which medical thinkers began to interpret respiration in a chemical way is that in which oxygen and CO_2 were discovered, and for this I must refer the reader to the article in this volume on Lavoisier, and to the lectures on the history of physiology by M. Foster, Cambridge, 1901, "The Rise of the Modern Doctrines of Respiration," p. 224 to 254. But to illustrate with what faint and delicate illuminations the human mind begins its faltering step from darkness to light, I wish to refer to my article on *Leonardo da Vinci as a Scientist,* in the Annals of Medical History, Vol. III, No. 1, 1921, where I point out that he had observed that air in which a candle had burnt long enough to become extinguished, was unfit to support the life of small animals,

XXVII

JOSEPH JANVIER WOODWARD

PIONEER IN PHOTOMICOGRAPHY, PATHOLOGIC HISTOLOGY AND MEDICAL HISTORIAN OF THE CIVIL WAR [1]

IN the preface of the first volume of Hemmeter's "Diseases of the Intestines," the author speaks of the work of Col. J. J. Woodward in the Medical Report of the War of the Rebellion as "monumental." One of my early teachers in pathology was Prof. William T. Councilman, later on Professor of Pathology at Harvard Medical School. In the year 1885, he was engaged in a study of the dysenteries which occurred epidemically at Bay View Hospital, Baltimore, and together with Dr. Lafleur he began a thorough research into the pathogenesis. Some of these intestines are depicted in my work above quoted. Some are there photographed, and others are drawn from high power magnification.

Dr. Councilman's researches led him to the Army Medical Museum at Washington, where he saw the specimens preserved there during and after the Civil War by Col. J. J. Woodward. The admiration which my teacher expressed for the penetrating thoroughness, scientific skill, and artistic ability of J. J. Woodward impressed this man's work upon my mind, and on a visit to the Vienna clinic, I called Nothnagel's attention to the magnificent illustrations in the "Medical History of the War of the Rebellion" which this great clinical master admired exceedingly.

This is the story of the awakening of my personal interest in the life works of a highly gifted American army surgeon, whose pioneer work and brilliant contributions to medical science I am pleased to bring before the profession in the perspective of recent special knowledge.

In the preface to the first volume of the "Medical and Surgical History of the War of the Rebellion," Dr. J. J. Woodward expresses due regard for the historical basis of the dominant opinions concerning the nature of the diseases to which this volume was devoted. He endeavors to trace all the more important facts to their sources, and in view of the many errors scattered through the textbooks of his day, some of which had been repeated for ages by authors, copying from each other, Woodward early resolved that he would cite no authority not before him when writing and that for the convenience of subsequent students he would, in every case, not only give the name of the author but the edition and page to which each citation referred. Natural as this seems in 1926, it was a very original undertaking immediately after the Civil War, and it is this painstaking regard for correctness that made Woodward's "Medical History of the War of the Rebellion" an exact piece of work for all future time.

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[1] *The Military Surgeon,* June 1923.

PATHOGENESIS AND MICROBIC ORIGIN

The *Bacillus dysentericus* (Shiga-Kruse bacilli—also sometimes known as the Flexner dysenteric bacillus) and the *Amoeba histolytica,* were not known in those days.[2] The dysentery bacillus of Flexner was thought by Adolf Schmidt to be identical with the Shiga-Kruse bacillus, but according to Leschly and Sonne, one-half of the cases of dysentery in Denmark are caused by the Shiga-Kruse bacillus and the other half by the Flexner bacillus. The difference between the various bacilli that are known as etiologic factors of dysentery is the varying degree of their toxicity. In addition to these, there are three allied types of dysentery bacilli, known as the type of Strong, the type Y (of Hiss and Russell), and the Harris type. It is possible that there is a mutation of individual strains of these organisms, but it is also conceivable that certain strains of the coli group may gain new properties on the basis of an already existing infectious colitis.

The dysenteries that occur in our country in time of peace in institutions for the insane, military barracks, etc., are, in my opinion, mixed infections of the milder type (Flexner) of bacilli and not near as toxic as the conditions which Woodward describes in his works. But it has also been found that dysenteries in which either the *Amoeba histolytica* or the *Amoeba coli mitis* is found in large numbers, there may be various bacilli of the type already mentioned, present at the same time. Yes, even streptococci, typhoid bacilli, and diphtheria bacilli may be present; both types of amoeba may be present in the same individual at the same time; genuine typhoid infection has been observed together with amoebic dysentery; also together with pyocyaneus infection. According to Curry, 66 per cent of the dysenteries in the Philippine Islands are caused by amoebae, and the rest are caused by the Flexner bacillus.

The technique of bacteriology and stool examination were an unknown territory in the days of Woodward. On page 278, "Medical and Surgical

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[2] F. Lösch published his first article on colon infections with *Amoebae coli* in Virchow's Archiv, Vol. 65, 1875.

History of the War of the Rebellion," Part 2, Vol. I, he cites Ihring, who wrote an inaugural dissertation on *"Die Microscopisch-Chemische Untersuchungen Menschlicher Faeces unter Path. Verhältnissen,"* adding that he has not seen this thesis in the original but is only familiar with an abstract.

On pages 279, 280, 281, 282, he quotes articles by Bechamp, Billroth, Ferdinand Cohn, Hallier, Gros and also the famous essay by Virchow—*"Die Fortschritte der Kriegs¢heilkunde besonders im Gebiete der Infectionskrankheiten,"* Berlin, 1874. There are also numerous American writers quoted in the footnotes on page 282, familiar among whom are John S. Billings, E. Curtis, W. H. Van Buren, Thos. E. Satterthwaite.

When J. J. Woodward abstracts the literature on any subject bearing on the diseases of the intestines, his words are not mere dry statements of the results of the investigations that he is studying, but he compares the results and deductions of various authors drawing parallel deductions and showing an amazing knowledge of the literature of his day and a very admirable knowledge of the French and German languages. His ability as a microscopist is acknowledged by all teachers of eminence and experience in the use of the microscope, even up to the present day.

Prof. Simon Henry Gage (Microscope, 13th Ed., 1920, p. 215) writes:

> Among those who showed the possibilities of photo-micrographs was Colonel Woodward of the U. S. Army Medical Museum. The photo-micrographs made by him and exhibited at the Centennial Celebration at Philadelphia in 1876 serve still as models, and no one could do better than to study them and try to equal them in clearness and general excellence. According to the writer's observation no photo-micrographs or histologic objects have ever surpassed those made by Woodward, and most of them are vastly inferior. It is gratifying to state, however, that at the present time many original papers are partly or wholly illustrated by photo-micrographs, and no country has produced works with photo-micrographic illustrations superior to those in "Wilson's Atlas of Fertilization and Karyokinesis" and "Starr's Atlas of Nerve Cells," issued by the Columbia University Press.

Professor Gage also quotes J. J. Woodward in his work on Optic-Projection, stating that limelight, which is the most brilliant after sunlight and the arc light, was used by Major Woodward in 1870, the first one to apply the oxy-calcium light for projection. A detailed list of his publications on photo micrography will be appended.

On page 279 of "Medical and Surgical History of the War, 1861–65," he described the result of microscopical examination of the stools of his cases. He gives the objectives and the techniques and the dimensions of the microscopic organisms which he pictures, most of which could be defined as cocci and bacilli, though he also pictures torulæ-like cells (yeast cells). The spherical forms which he describes, he identifies with the micrococcus of Hallier (*Zeitschr. F. Parasitenkunde,* Bd. I, 1869) and the sphaero-bacteria of Cohn. He has observed that they are in continual active movement (Brownian movement). Woodward is aware that the organisms he finds in disease stools can also be observed in normal stools. The distinction between normal and pathogenic organism had in those days not yet been carried to exact technical methods.

On page 281 he discusses the question whether the dysenteries and diarrheas could have been caused by the bacterial forms found in the normal feces and quotes the pioneer work of Hallier, in which this early bacteriologist claims that the organism which he has studied in culture experiments developed into characteristic species of microscopic organisms peculiar to each disease. This is one of the first statements expressing the specificity of pathogenic organisms. DeBarry and Hoffmann later contested this view on botanical grounds.

In reading this page 281 one cannot fail to be impressed with Woodward's critical discernment, for he discusses the arguments of Hallier, Burdon-Sanderson, and Cohn with a fairness and conservatism that is a model of scientific judgment. The main question here involved was whether one type of microörganism could transform into another type; for instance, whether a torula could become a penicillium as Burdon-Sanderson asserted, or whether micrococci could be transformed into bacilli. Woodward on page 282 leaves this question undecided.

The preface which J. J. Woodward wrote to his greatest work is dated March 25, 1879, and should any coming historian on the development of clinical conceptions concerning the pathogenesis of diarrhea and dysenteries, desire to find out the early American ideas on this subject, he can find them in this volume, particularly in the footnotes on pages 281 and 282.

Marcus Antonius Plenciz established the first theory of infection, that is, the origin of infectious diseases founded on the development of microöganisms, in 1762, quoted on page 207 of *"Ludwig Darmstaedters Handbuch zur Geschichte der Naturwissenschaften und der Technik."* According to Sudhoff, Varro expressed the opinion that malaria was caused by extremely small living organisms. Varro lived about thirty-five years before Christ. See also Chas. Singer, "The Doctrine of Contagium Vivum 1500–1750," Historical Section of the London International Medical Congress, 1913.

When one considers that bacteria were first seen by Leuwenhoeck in 1675 and in 1683 and that Marcus Antonius Plenciz described the etiologic significance of microörganisms for the development of infectious diseases in 1762, one must marvel at the slowness of progress in the conceptions of scientific men of the eighteenth century, concerning problems of causation of diseases which were destroying human beings by the hundreds of thousands at various times all over the earth; and yet, in spite of the possession of the microscope and the genius and vision of sundry scientific prophets, it required more than one hundred and twenty-five years after Plenciz before bacteriology was taken up as a separate department of human scientific investigation. This reproach applies not only to the eighteenth century but also to the nineteenth, for even in the days of J. J. Woodward there were teachers, professors in medical schools, who not only refused to accept the idea of the bacterial causation of diseases, but I can personally remember that they ridiculed it and, without having the slightest training or knowledge in bacteriology, they created in their students a feeling of disgust against the new science which, in many instances, acted as an incubus and deterrent to further medical study. In America, it was largely due to Surgeon General Geo. Miller Sternberg (1838–1915) and to J. J. Woodward that American medical schools were led to accept the great importance of bacteriology as an absolutely necessary science for the understanding of diseases. In 1880 Sternberg translated Antoine Magnin's French work on bacteriology and in 1892 he published his own "Manual of Bacteriology." He established the Army Medical School at Washington while he was Surgeon General, but J. J. Woodward deserves the credit for his diligent and penetrating research into the bacteriological literature of the world of his day, for the direct application of this science to the causation and treatment of intestinal diseases.

Although Sternberg was assistant surgeon in the U. S. Army and served at the first battle of Bull Run, when he was taken prisoner, making his escape and later again reporting for duty, and although he took part in the battle of Gaines' Mill, Malvern Hill, and in 1862 contracted typhoid fever and went through epidemics of such infectious diseases as typhoid fever, cholera, and yellow fever, he is not mentioned in the list of contributors, nor of the authors cited in the "Medical History of the War of the Rebellion." This is very likely to be explained by the exceedingly active service which Sternberg saw during the Civil War and by the fact that he was twice very ill himself, but certainly he would have been the man most fitted to lend assistance in the bacteriology of the diarrhea and dysenteries.

To give an idea of the clinical material that the various army surgeons had to draw upon, the table on page 2, "Medical and Surgical History of the War of the Rebellion," gives a total number of cases of—

Acute diarrhea at 1,269,027 with 4,291 deaths.
Chronic diarrhea, 182,586 with 30,836 deaths.
Acute dysentery, 259,071 with 5,576 deaths.
Chronic dysentery, 28,451 with 3,855 deaths.
Deaths from chronic dysentery, 7.3 per cent and from diarrhea 16.8 per cent.

There were all together, then, 1,739,735 cases of intestinal disease with 44,558 deaths. I have read through the detail of the clinical history of cases 93 on page 103 to cases 878 on page 264 and find that there are a great many of these reports which cannot compare in thoroughness of pathologic findings with those that were reported in the same volume as autopsies made by the Master Joseph Leidy. But apart from the thoroughness of the prosector, we have to deal with the insurmountable difficulties which the defective equipment of temporary war hospitals and the vast amount of autopsy material presented to the Medical Staff. We can, therefore, safely assume that a great many of the cases classed as diarrhea, acute and chronic, were, in reality, infections with either the *Amoeba coli* or the *Amoeba histolytica* or one or another of the types of the *Bacillus dysentericus* (either Shiga-Kruse or Flexner).

While it is true that Woodward speaks on page 371 of the amoeba coli of Lösch, he also speaks of the *Cercomonas intestinalis* of Lambl, and it impresses me that this is the protozoan which was later called *Lamblia intestinalis* and is now known by the name of Giardia. A protozoan organism which still is a frequent cause of severe intestinal infection in our southern states and concerning which I described the first distinct American infection, which was reported in the Washington Medical Annals in 1902, Vol. I, March, page 64, by Chas. Wardell Stiles. That these organisms were the cause of some of the dysenteries included in Woodward's statistics there is no doubt whatever in my mind, but there were also infections with the bacillus of tuberculosis, which Woodward recognized as such, and on page 575 he devotes a chapter to this condition with a splendid photo micrographic plate of a tubercular ulcer of the ileum facing pages 582 and 584, but he also has numerous smaller illustrations of tubercular intestinal ulcers in the text. In the section on page 587, however, while it does indicate tubercules in the submucosa, the general appearance of this ulcer is that which is frequently seen with amoebic dysentery.

One can hardly resist the temptation to compare the results of J. J. Woodward's long-continued studies on the bacterial and protozoan pathogenesis of diarrhea and dysenteries with our modern knowledge of these infectious organisms and the extensive work of Kofoid and his pupils and Sydney K. Simon.

The early history of dysentery is referred to by Woodward himself in his footnotes. He even refers to Aretaios on page 362, who lived in the second century after Christ, and he refers to the efforts of the Greek physician to distinguish between ulcerations of the small intestines and of the large intestines. They attached great importance to this question, for if the large intestine were ulcerated, they hoped to reach the seat of the disease by injections from the rectum, whereas, if the small intestine were ulcerated, this could only be done by medicines given by the mouth (Galen, De Loc. Affect, Lib. VI, Cap. 2) (Ed. Kuehn, VIII, p. 383).[3]

That the word *dysenteria* (δὖς-ill, ἕντερα-bowels) was used familiarly, in the same sense as at present, at the time of Hippocrates or even earlier, is shown by the passage in the history of Herodotus (Lib. VIII, Cap. 115), which describes a pestilence and dysentery that decimated the army of Xerxes while retreating through Thessaly.

The following definitions from Hippocratic writings of doubtful origin, harmonize well with the descriptions and allusions scattered through the genuine books. In Regimen (De victus rat.), Lib. III (Ed. Littre, VI, p. 617), after stating that diarrhea is one of the consequences of habitually taking too much food in proportion to the amount of exercise used, we are told that so long as putrescent alimentary matters only are passed from the bowels the disease is called diarrhea; "but when the body being heated, acrid matters are discharged, the intestine is excoriated, ulcerated, and the stools are bloody; this is dysentery, a grave and dangerous disease."

This Hippocratic quotation is followed by similar quotations from Celsus, Galen, Hildanus, and three-fourths of page 337 is covered with historic abstracts of such excellent arrangement and translation, that future historians will miss a great source of historic research on intestinal diseases if they neglect to study J. J. Woodward's historiographic investigations concerning acute dysentery from pages 336 to 348 of his great work.

The examinations of the stools on page 345 and the abstracts of the accompanying literature in the footnotes are highly instructive. They include special chapters on fecal matters (p. 353), on mucus and pus (p. 355), on blood and bloody serum (p. 357), on pseudo-membranes and actual cases of necrosed intestinal tissues (p. 359). This question as to whether the intestinal discharges are made up entirely of mucus formed into a pseudo-membrane containing accidental inclusions like bacteria, protozoa, blood corpuscles, pus cells, etc., or whether they are genuine pieces of the intestinal architecture itself, leads to a fascinating discussion on pages 360 to 362.

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[3] *Synonyms:* Dysentery (Hippocrates, Galen and other Greek writers;) (Galen —1 Com. in Prom. Hip., 8, Td. Kuhn, Tom. XVIII, B, p. 33), Tormina (Celsus); Rheumatismus intestinorum cum ulceratione; (Caelius Aurelianus); Difficultas intestinorum.

Woodward decides that among the earliest to conclude that in some cases the fragments discharged are portions of the actual intestinal coats while in others they are merely false membranes, was Morgagni, and he supported his opinion by reference to the condition of the intestinal mucous membrane after death in such cases (*Morgagni De sedibus et Causis Morborum,* Epist. XXXI, 339). In the chapter on Bacteria and Animal Forms, he attributes the first definition of infection after the inventing of the compound microscope to the ingenious Jesuit, Father Athanasius Kircher, who in 1658 asserted that microscopical animalculae were the true cause of pestilential diseases, but while this date is earlier than the reference given by me concerning Plenciz as 1762, yet in the description given by Kircher, I find that his *"vermes"* [4] are the result and not the cause of what he calls *"Omnia Putrida."* Kircher concludes that each living substance by its putrefaction gives rise to specific animalculae.

It is evident here that Woodward in the large printed text at the top of page 369 designates as *true* CAUSE *of pestilential disease* what the quotation from Kircher's own writing defines as the RESULT *of putrefaction.* Cause and result are here evidently confused. In the discussion on page 371 Woodward comes to speak of the *Lamblia intestinalis,* now called Giardia, and the *Amoeba coli.* On this page he gives the entire early history of the clinical recognition of amoebae as pathogenetic factors.

On page 377 he describes fat stools, and therewith is connected a historic study of the occurrence of fat in the stools which goes back to Hippocrates. On page 382 he has a reference to the formation of pseudo-gall stones caused by olive oil, and on page 383 he emphasizes that fatty concretions of various sizes and consistence can be found in the stools of patients who are suffering from dysentery or typhoid fever, when they are kept on a milk diet.

The so-called lymphoid swarms pictured on page 486, magnified only 300 diameters, remind one very strongly of the appearance of amoebae when hardened in Miller's or Zenker's fluid. The same can be said of the figure on page 469.

On page 825 there is the only history of methods for injecting fluids, etc., into the rectum and colon which I have found in the English language. Woodward quotes Herodotus (Euterpe, Cap. 77) that the Egyptians purged themselves every month, three days successively, seeking to preserve their health by emetics and clysters. An enema apparatus by Gatinaria from the year 1525 is pictured on page 825. The various types of solutions for enemas are given. Even iodine and nitrate of soda are described as enema ingredients, and also the therapeutic attempts of E. Hare ("Treatment of Tropical Dysenteries"; the Edinburgh Medical and Surgical Journal, vol 72, 1849). Hare used a long flexible rubber tube passed beyond the sigmoid flexure and a force pump, but Dulles found that simple gravity was safer than the force pump of Hare.

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[4] Omnia putrida innumerabile vermium . . . cognitum fuit from J. J. Woodward l. c. footnote p. 369.

This review of the principal chapters of Woodward's "Monumental Medical History of the War of the Rebellion" necessarily had to be simply an outline, for such a massive volume of 869 pages, not including twelve pages of introduction, could not be abstracted in any other way.

Victor C. Vaughn, in his admirable work on "Epidemiology and Public Health," speaks of J. J. Woodward as follows:

> The nature, prevalence, causes, and the varieties of the alvine fluxes prevalent among the soldiers during our Civil War were recorded and described in a most masterly way by Woodward. He (Woodward) was halfway convinced that the virus of these diseases is present in the stools of the sick, and in one or two places he almost hits upon the true cause of the wide prevalence of diarrhea and dysentery in the camps. Although Woodward seldom speaks with positiveness, there are to be found in his voluminous reports some suggestions, the value of which have been more recently demonstrated. For instance, no less an authority than Virchow had taught that dysentery may be due to constipation and the consequent ammoniacal fermentation of the fecal matter in the intestine. Woodward disposes of this error by showing statistically that dysentery in the great majority of cases is not preceded by constipation.

Professor Vaughan wrote to the author May 8, 1922:

> Dear Dr. Hemmeter:
>
> I do not think that you can say anything too eulogistic of Woodward. He certainly was far ahead of his time and the "Medical and Surgical History of the War of the Rebellion" is a monument to his learning and industry.

The following personal notes and a list of his publications were taken from the *Philadelphia Medical News,* August 30, 1884, which also contains an Obituary of Surgeon Joseph Janvier Woodward, U. S. A.:

BREVET LIEUTENANT-COLONEL JOSEPH JANVIER WOODWARD

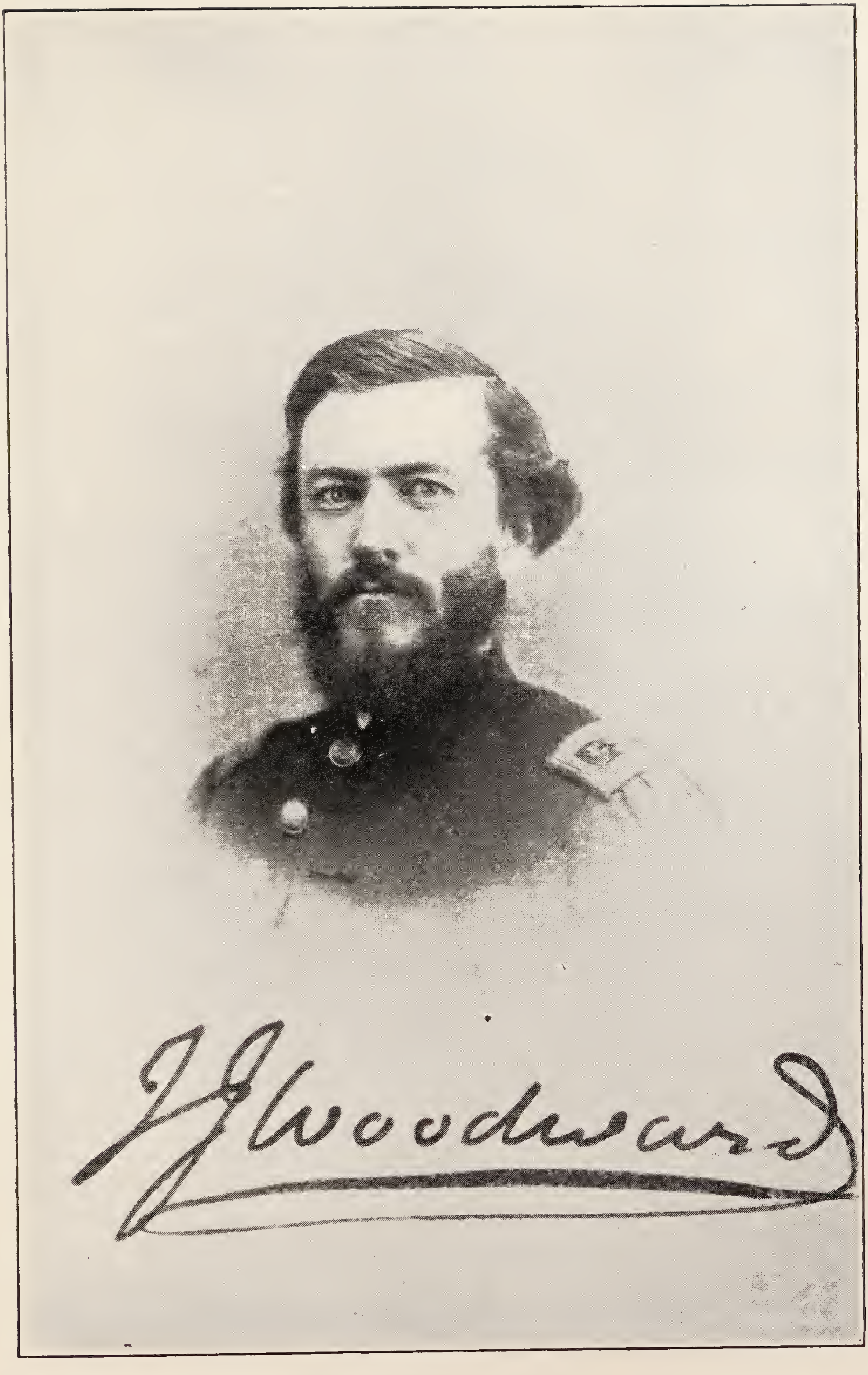
Surgeon, U. S. Army, who died near Philadelphia, on Sunday, August 17, 1884, at 1 P. M., was born in Philadelphia, October 30, 1833, and at the time of his death was not quite fifty-one years of age. He was educated at the

Central High School of that city, and in 1850 received the degree of A.B. He devoted himself to the study of medicine and graduated from the University of Pennsylvania in 1853. Two years later, in 1855, he received the degree of A.M. from the Central High School, acting as the valedictorian of his class. Immediately after his graduation, he entered energetically upon the practice of his profession in his native city, devoting a large portion of his time to that branch in which he afterwards achieved such a decided success—the microscopical investigation of pathological anatomy, and of kindred subjects. On May 17, 1858, he read before the Biological Department of the Academy of Natural Sciences, in Philadelphia, a paper, "On the Minute Anatomy of Three Cases of Cysto-carcinoma," which will be found on page 54 of *The American Journal of the Medical Sciences* for July, 1858. Other papers, read before the same society, followed rapidly: "Remarks on Anatomical Diagnosis of Cancer," November 15, 1858; "On Suppuration in Cancerous Growths," March 21, 1859; "On Errors in the Anatomical Diagnosis of Cancer," December 4, 1859; and "On a Secondary Cancer of the Axilla," January 16, 1860.

When the war broke out in 1861, he offered his services to his country, and entered the Army as Assistant Surgeon, U. S. A., on August 5, 1861. He was with the Second Artillery, in the Army of the Potomac, until May 19, 1862, when he was assigned to duty in the office of the Surgeon General in Washington, where he located and organized several hospitals, and, for a brief period, was in charge of the Patent Office Hospital. Relieved from this duty, he was placed in charge of the Pension Division of the Surgeon General's Office and of the Medical Section of the Army Medical Museum, which had then been opened for the collection of specimens of morbid anatomy, surgical or medical. At the same time, the task was assigned to him of collecting the material for the *medical* portion of the "Medical and Surgical History of the War," while his colleague, Dr. Otis, had charge of the collection of material for the *surgical* history and the surgical branches of the museum collection. Both these officers prepared reports on the material available, which were published as "Circular No. 6" of the Surgeon General's Office, on November 1, 1865. The second half of the circular, which was very favorably received by military surgeons, was prepared by Dr. Woodward. In the following year he made a report, "On Epidemic Cholera in the Army of the United States during the year 1866," which appeared as "Circular No. 5," Surgeon General's Office, on May 4, 1867, and which was followed in June, 1868, by "Circular No. 1," "Report on Epidemic Cholera and Yellow Fever in the Army of the United States during the year 1867."

In the meantime, the collection of the material for the Medical History of the War had progressed rapidly, and, on November 12, 1870, he issued his first volume of 726 pages, quarto, containing a series of statistical tables, presenting a summary view of the facts embodied in the monthly reports made to the Surgeon General with regard to the sickness of the Army, the deaths and the discharges on surgeon's certificate of disability.

After the close of the war, he had returned with increased fervor to his investigations in microscopy, and especially in photo-micrography, and the results of his experiments, which made him famous in Europe as well as at



Joseph Janvier Woodward

home, were given in a series of publications which followed each other in rapid succession:

Report on the Magnesium and Electric Lights as applied to Photo-micrography, 6 pp., 4to, 11 photographs. Surgeon General's Office, 1870.

Report on the Oxy-calcium Light as applied to Photo-micrography. 3 pp., 4to, 2 photographs. Surgeon General's Office, 1870.

Report on an Improved Method of Photographing Histological Preparations by Sunlight. 10 pp., 4to, 11 photographs. Surgeon General's Office, 1870.

Report on the Histology of Minute Blood-vessels. 8 pp., 4to, 11 photographs. Surgeon General's Office, 1871.

Report on the Minute Anatomy of Two Cases of Cancer. 10 pp., 4to, 2 photo-lithographs. Surgeon General's Office, 1872.

Memorandum on Pleurosigma Angulatum and Pleurosigma Formosum. 4 pp., 4to, 8 photographs. Surgeon General's Office, 1871.

Memorandum on Surirella Gemma. 1 p., 4to, 2 photographs. Surgeon General's Office, 1871.

Memorandum on the Test Podura. 3 pp., 4to, 5 photographs. Surgeon General's Office, 1871.

Memorandum on Amphipleura Pellucida. 1 p., 4to, 2 photographs. Surgeon General's Office, 1871.

Memorandum on the Nineteen-band Test-plate of Nobert. 4 pp., 4to, 9 photographs. Surgeon General's Office, 1872.

Four letters to the Surgeon General, accompanying Photographs of the Mosquito, certain Parasites, the Proboscides of certain Flies, and Miscellaneous Photographs of Insects and Parts of Insects. 8 pp., 4to, 35 photographs. Surgeon General's Office, 1872.

Remarks on Photographic Micrometry in Trans. of the American Medical Association, 1876; Application of Photography to Micrometry, with Special Reference to the Micrometry of Blood in Criminal Cases. *Ibid.*

In 1876 he had charge of the representation of the Medical Department of the United States Army at the International Exhibition at Philadelphia, and the success of the medical exhibition was mainly due to his energy and activity. During the succeeding two years he devoted himself entirely to the preparation of the second *medical* volume, which appeared on March 25, 1879. The work treats on the "Alvine Fluxes" and will be a lasting testimony of the zeal and indomitable perserverance of the author. In careful and painstaking research of the literature of the subject, it surpasses all previous publications. The anxious pursuit of this work had gradually undermined his constitution, never very robust, and in the summer of 1880, he went to Europe to regain his health. He returned in a somewhat improved condition, but unfortunately was again prostrated by a fall from his horse in the spring of 1881, by which he fractured his leg. Barely recovered from this accident, he was called, in July, 1881, to the bedside of the lamented President Garfield. His quiet and patient services met frequently a flood of abuse from professional as well as non-professional journals. We will only here state that he kept full notes of daily, even hourly observations of the patient's condition, which he intended to publish as a defense, if such were needed, at some future time. Sickness and death have despoiled him of this opportunity.

Time and space have allowed us to refer only to a few of the numerous publications of this indefatigable worker. A close observer, tenacious in his convictions of what he conceived to be right, writing to the point without flourish or embellishment, fearless in the expression of his views, courting rather than avoiding criticism, he was ever ready to enter the lists in defense of his opinion.

Dr. Woodward's last editorial work was his obituary notice of his colleague Otis, which appeared in the *American Journal of the Medical Sciences,* July, 1881.

"For faithful and meritorious services during the War," Dr. Woodward received the brevet ranks of Captain, Major, and Lieutenant Colonel, and on June 26, 1876, was promoted to the rank of Major and Surgeon. He became a member of the American Medical Association in 1865, and in 1881 was chosen its president, being the first medical officer from the Army to whom this honor was accorded. He was a member of the College of Physicians of Philadelphia, of the National Academy of Sciences, of the Philosophical Society of Washington, and honorary member of the Royal Microscopical Society and of the Quekett Club of London, of the Liverpool and Belgian Societies of Microscopy, and of many other societies in this country as well as in Europe.

His decease added another name to the list of prominent men connected with the Surgeon General's Office, whom death removed within the brief period of three and a half years. On February 23, 1881, occurred the death of G. A. Otis, the famous author of the history of the surgery of the war. Two years later, on April 5, 1883, Brig. Gen. J. K. Barnes, who had watched by the bedsides of two martyred Presidents, passed away, and, on October 10, of the same year, Surgeon General C. H. Crane, at whose suggestion both Woodward and Otis had been selected as the compilers of the Medical and Surgical History, died after a brief illness.

Chapters XV, XVI, XXVII, and XXVIII give illustrations of great services to Medical Science that have been rendered by members of the Medical staffs of the U. S. Army.

XXVIII

WILLIAM BEAUMONT

AND THE BEGINNINGS OF MODERN PHYSIOLOGY OF DIGESTION

WE form the habit of thinking of great men as having appeared only at long intervals, and yet we know that great crises always discover great men. In William Beaumont we not only have an American surgeon of no small note and reputation, but in him we have the first great American physiologist. It was one of these great crises that placed this man in the fore-ranks of his profession; but still more it was the all possessing power, ability, and seriousness of purpose which enabled him to seize and make the most of opportunity when it rapped at his door. In the annals of French and English history we find the name of Beaumont very prominent. When the Huguenots were driven out of France to find refuge on foreign soil, the name of Beaumont went with them and took up its abode in England, later to find its way to the distant shores of the new America. From this grand old stock, no doubt, came the distinguished family from which descended the pioneer physiologist of the United States, who was born at Lebanon, Connecticut, November 21, 1785, the third child of Samuel and Lucretia Beaumont.

At this time the little colony of Lebanon, largely made up of farmers, was just recovering from the Revolution; there was lack of money and of higher education; the New England method of farming was still crude; and the living was of the most plain and simple type. Under this environment, therefore, this youth grew to manhood, assisting his father in the tilling of the soil in those days of oxen and the wooden plow, when the grain was cut with the old-fashioned scythe and threshed with the old-fashioned flail. His clothes were homespun; and pork and beans and Indian puddings comprised his daily rations. He spent the long winter evenings in front of the great fireplace and by the dim light of the candle read a few old books, or perhaps now and then a newspaper that strayed into the home; and on the Sabbath he attended church as it was customary for everyone to do in those early days. Little is known of his boyhood tendencies, except that he was courageous and fearless; for he did not keep a diary of the events of his boyhood as he did later of his adult life. One fact only is recorded of this period: that he "took a dare" as to who could stand nearest to a cannon fired, and acquired in this way a defect of hearing, which gradually increased and may explain to some extent his intense concentration upon his experimental work. He had the benefit of the excellent common schools of the progressive little town, and at the opening of the new century was a well-educated studious lad of fifteen. Newspapers and periodicals, which were rapidly increasing in numbers in New England, conveyed to his receptive young mind a knowledge of the vast progress of the world outside the little sphere of Lebanon, and he began to look forward with restlessness to the day when he would become his own master and could leave his paternal home and go out into that world to make a name for himself. At last it arrived, and declining a fine farm in the neighborhood which his father offered him, he set forth in the winter of 1806–7 to face the world, equipped with an outfit consisting of a horse and cutter, a barrel of cider, and a hundred dollars of hard-earned money. He wandered northward through the western parts of Massachusetts and Vermont, as yet sparsely settled, and in the spring arrived at the little village of Champlain, in New York, near the Canadian frontier. He liked the thriving little place, with its thousand inhabitants, mostly farmers; and, as he did not care to farm, applied for the position of village school teacher, which he obtained without difficulty, as New York was much behind New England in education. He successfully taught the school for about three years, developing the methodical habits which marked his later work, and then, either from natural restlessness or because his school teaching had been merely a means of saving money for it, turned toward the study of medicine. While teaching, he had used his leisure hours for reading the medical works in the library of Dr. Pomeroy, a prominent physician and surgeon in Burlington.

In the fall of 1810, he moved across Lake Champlain to St. Albans, Vermont, where he was successful in obtaining as his preceptor Dr. Benjamin Chandler, the most prominent physician of that locality. He was taken into the doctor's home as an apprentice and a fellow pupil with his son, John, who was reading medicine with his father. This choice was a wise one, if a tree may be judged by its fruits. After the diligent completion of the prescribed two years of apprenticeship, in which he never lost an opportunity of dissecting an arm or performing a post-mortem, he was granted a license by the Third Medical Society of the State of Vermont, which after examination did "willingly recommend him to the world as a judicious and safe practitioner in the different avocations of the Medical Profession."

There is still preserved, probably among the papers presented by his granddaughter to the Washington University Medical School, a notebook in which he recorded interesting cases and precepts noted down at this period, showing a growing habit of accuracy in observation and faithfulness in recording which indicate a master mind in the formative process, and explain why his great physiological opportunity alone, out of all those cases of gastric fistula on record up to that date, was seized and built into the foundations of the coming science.

He had in his two patrons, Pomeroy and Chandler, men of ability and broad medical interest, who doubtless had upon their shelves the best accessible medical works. The notebooks and letters of their pupil indicate that medical study and practice under their guidance became an all-absorbing passion. The old-fashioned "preceptorship" when it involved daily rides to see cases, with long talks and questionings, enabled the older man to confide the best of himself as well as of his experiences to the younger, and is not yet wholly replaced by the modern hospital clinic.

There is one letter of Beaumont to his parents which shows that he was taught during his student years something of the coming importance of Physiology and Physiological Chemistry. "My situation affords me a very pleasant life, though it requires my utmost diligence and perseverance in the pursuit of a medical profession by which I hope to exhibit proficiency in an art which, in this enlightened age of reason and under the modern improvements of Chemistry and Physiology, bids fair to rise with healing on her wings."

He says in the same letter that Dr. Chandler is the hottest Federalist he ever knew and daily threatens to turn his pupil out of doors as a Republican.

After receiving his license, Beaumont remained with his preceptor about three months,—until the war of 1812 began; then he again crossed Lake Champlain, and went to Plattsburgh, where a portion of the Army of the North under General Dearborn was encamped. Here was an opportunity for him not only to be patriotic but to put into immediate practice the art of his chosen profession; so he at once presented his credentials to the proper authorities, and on September 13th was received into the army as surgeon's mate in the "Sixth Regiment Infantry on brevet from General Bloomfield." On December 2nd, 1812, his commission was issued by President Madison; and on December 12th he was transferred to the Sixteenth Infantry. As there was very little activity on the part of the army in which he had enlisted, he retired from duty January 1st, 1813, and began the private practice of medicine in Plattsburgh. A little later, on the return of activities, he reëntered the army, and, on February 15th, was transferred back to the Sixth Regiment. Soon, this division of the army moved on to Sackett's Harbor, and here he administered to the wants of the suffering soldiers, and endured with them many of their hardships. He was more or less active throughout the campaign, and in August, 1814, took part in the battle of Plattsburgh under General Macomb, who defeated the British, although they had twice the number of men and more vessels. A few months later, the war came to a close; and in March, 1815, President Madison, through his Secretary of War, issued orders that the army be reduced to ten thousand men, and those officers retained who had won the highest merits. On this basis Beaumont was retained in May, 1815, in the Sixth Infantry, while hundreds higher in rank and older in service were dropped.

There being little opportunity for advancement in the service, and little experience in a healthy garrison, his ambitions soon led him to resign; and returning again to Plattsburgh, he entered into partnership with Dr. G. Senter, another army surgeon, for the practice of their profession. They also opened a general store for the sale of drugs, medicines, groceries, etc. They continued in partnership only a short time, for Dr. Senter soon went South, and the firm became known as Beaumont and Wheelock. After conducting this store for a year he sold out and devoted himself entirely to the practice of Physic and Surgery. In 1818, he was made a member of the Clinton County Medical Society, an honorable body founded in 1807 by eight physicians and surgeons. On April 6th, 1819, he was commissioned surgeon of the Fifteenth Regiment of the cavalry of the New York militia by Governor DeWitt Clinton, and in December discontinued practice in Plattsburgh. About this time the medical corps of the army was being reorganized, and his friend and military comrade, Dr. Joseph Lovell, was chosen Surgeon-General. Through him he was offered a thousand dollar clerkship, which he at first accepted, but later declined, for the reason, probably, that clerical work was not entirely to his liking. On March 18th, 1820, he was commissioned as post surgeon of the army to take rank from December 4th, 1819, and was at once ordered to Fort Mackinac, on the northwestern frontier, where he was to report once more to General Macomb. The village of Mackinac, in which was located the fort, consisted normally of some five hundred inhabitants, the greater part of whom were French Canadians and half-breed Indians, whose chief occupations were hunting, fishing, and trading in furs. It was also the headquarters of the American Fur Company which had been established a few years before by John Jacob Astor. Here it was that, during the summer months, the clerks, traders, and voyageurs, employed by the fur company, assembled and spent their hard-earned money, while the pelts were being assorted and packed for shipment. The Indians from the North also brought in their furs, and the population swelled to four or five thousand for a short period.

It was at this time that Beaumont arrived, greeted by this shiftless and roving populace and by the fights and whoops of drunken Indians. Fortunately, he had permission to engage in private practice, and as he was the only physician in the village, naturally surgical material was abundant at this season of the year, offering him rich experiences. In March, 1821, he applied for a furlough, and in August left Mackinac for Plattsburgh, where he was married to Mrs. Deborah Platt, the widowed daughter of "Friend Israel Green, innholder in Plattsburgh." She was a beautiful and intelligent young woman, of good family, who had become prominent through administering to the wants of the injured in the War of 1812, and was well fitted to be a helpmate to Beaumont. He returned soon to Mackinac with his bride, and she became matron of the hospital there.

In addition to her duties as hospital matron, Mrs. Beaumont, on the lonely island with its ten or twelve white women, kept open house for her husband's fellow-officers and made her home a rendezvous of those who sought the best and highest in the social life of the military post; at the same time, doubtless, holding Dr. Beaumont to the best possible use of his opportunities.

THE GREAT OPPORTUNITY

In the early summer of 1822, Indians and voyageurs were returning to Mackinac with their winter catch. The little village awakened from its long winter sleep. The beach was again crowded with tents and wigwams. Situated at the junction of Lakes Huron and Michigan, with Lake Superior near by, it possessed a store and trading center of the American Fur Company. Pelts were being unpacked and appraised by the Company's officers, and the store was crowded with the new arrivals. Here, on June 6th, occurred the peculiar accident which was not only to live throughout the annals of medical history, but to make the name of Dr. Beaumont ("Le bon doc-teur who mak' de mer-a-cle") famous for ages to come. A gun was accidentally discharged in the store, and its load of shot entered the body of Alexis St. Martin (or Bidigan), a Canadian voyageur about 19 years old, standing three feet away. St. Martin fell, with a cavity in the left abdomino-thoracic region as large as a man's palm and an opening into the stomach cavity large enough to admit a forefinger.

Beaumont dressed the wound and stated that the man could not live thirty-six hours. He was, however, so much better a few hours later that he was taken to the hospital of the post. The treatment extended over a period of two years, and the completeness of Beaumont's notes would be a credit to any modern hospital. It has been alleged that he made no effort to close the wound, but purposely kept it open for the sake of experiments on the stomach. This, however, is not authentic; for when other methods failed he suggested incising the edges of the wound and bringing them together with sutures; but St. Martin would not submit to this treatment and operation, and so the fistulous opening remained and he became the object of curiosity as the man "with a lid on his stomach."

In 1824, Beaumont sent a complete report of his case to Surgeon-General Lovell, and with his approval it was published in the Medical Recorder early in 1825 (ascribed through error to Dr. Lovell). The Medical Society of Michigan Territory was the first to recognize it by making him an honorary member of the society.

Early in 1825, he conceived the idea of conducting a series of experiments on the stomach of St. Martin. Already he had ascertaind to some extent the time necessary for the digestion of certain foods, and had even watched the process of digestion itself through the fistulous opening (large enough to admit a finger) where the bits of food had been introduced. He could also collect the gastric fluid which frequently flowed out spontaneously from the glands into the stomach cavity and experiment with it. Moreover, on removing the dressings, he would sometimes find the stomach everted to the size and about the shape of a half blown rose, giving a view of the glands as they secreted.

The facilities for research at Mackinac (even if he received the "book of experiments" which Dr. Lovell tried to obtain for him, which is doubtful), were of the poorest sort yet it was under these discouraging conditions that he began his remarkable investigations. A little later, however, he was transferred to Fort Niagara, where conditions were more favorable for the series of experiments, which he continued until August, 1825. Then most suddenly and unexpectedly they were terminated, through the disappearance of St. Martin.

All attempts to locate the latter proved fruitless, and Beaumont became broken-hearted over his loss; yet consoled himself with the thought that he might fall into the hands of some one more capable than himself of working out the problems of gastric digestion. However, he decided to give to the world the results of the few experiments that he had made; and although these were incomplete they assured him that the old theories of maceration and putrefaction were insufficient to explain the processes he had observed, and that the gastric secretion possessed innate solvent powers.

About the middle of the year 1826, when the Indians became hostile in the northwest, Beaumont was transferred, with the troops from Sackett's Harbor and Niagara, to Fort Howard near Green Bay; and a little later he received orders to join the Fifth Regiment at Fort Crawford, also in Michigan Territory and presenting much the same conditions.

He had now heard nothing from St. Martin for fully two years, and had it not been for the interest which certain employees of the fur company took in locating him, this rare case would have been lost to science. It was now reported that he was living near Montreal, Canada, had married, had become the father of two children, and was working in great poverty for the support of his family. After two more years, with the assistance of officers of the fur company, Beaumont succeeded at great expense in engaging his services, and St. Martin and his family were transported from lower Canada to Fort Crawford, at Prarie du Chien, upper Mississippi, a distance of nearly two thousand miles.

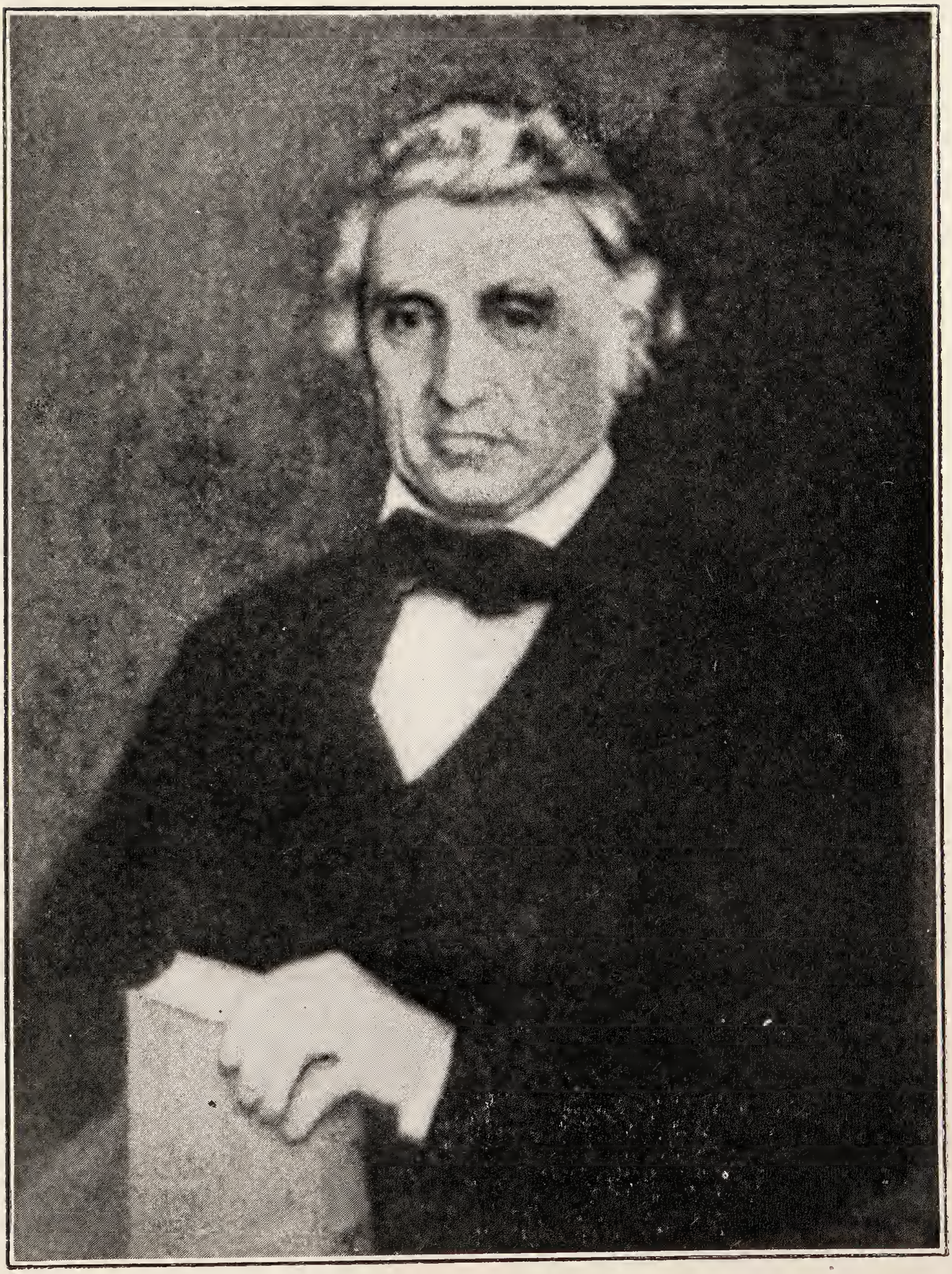
Beaumont without any delay took up his line of experimentation on the stomach of St. Martin exactly where he had left off four years before. At this isolated military post, 3,000 miles away from any physiological laboratory, with no laboratory equipment except a thermometer, a few open mouth vials and a sand bath he conducted a series of observations on digestion which have not been excelled even under most favorable conditions and surroundings. In this series, of fifty-six experiments, he fully demonstrated the solvent properties of the gastric juice and that its operation is that of a chemical agent, breaking up and dissolving the heterogeneous articles submitted to its action into a uniform, homogeneous semi-fluid. At the close of these experiments, covering sixteen months, St. Martin returned with his family to Canada; this time with the consent of his benefactor; for Beaumont felt confident that he could obtain his services again for further experimentation. He was convinced, too, that he had gone as far as he could without the assistance of others, and determined to get in touch with men trained in chemical investigations, especially in physiological chemistry.

On May 23, 1831, Beaumont obtained a furlough of one year for the purpose of going abroad with St. Martin in order to study there some of the unsolved problems of gastric digestion with the European scientists; for at that time the best chemists, in fact, the only physiological chemists, were to be found in Europe. Disappointment, however, soon became his lot for the furlough was recalled—due probably to the outbreak of hostilities which led up to the Black Hawk Indian War—and he was transferred with the troops to Fort Dearborn.

The troops who were at this time rushed from New York to the scene of battle brought with them a worse enemy, the Asiatic cholera, which was then first introduced here. In the summer of 1832, the war closed, and Beaumont returned to Fort Crawford. He had had wide experience not only in an Indian war but in Asiatic cholera and intermittent fever as well. Although the malarial parasite was as yet undiscovered, he showed by his notes that he had a clear insight into some of the causative factors of malaria. On August 22, with the cholera epidemic under control, he was given leave of absence of six months with permission to visit Europe, and immediately took his departure from the northwest for Plattsburgh.

Here St. Martin, for once true to his promise, met Beaumont, and on October 16, 1832, a legal contract was drawn up between the two parties, the like of which has never been duplicated in history, whereby the said St. Martin for the consideration mentioned "shall for a period of one year serve, abide, and continue with the said Beaumont wherever he may travel or reside in any part of the world," etc. He now went with St. Martin to Washington and for some reason gave up his intended trip abroad and devoted his time to the study of the literature on the physiology of digestion.

In order to lighten Beaumont's burden financially, St. Martin was made Sergeant of a detachment of orderlies in the War Department at Washington through the Surgeon-General, and Beaumont began his third series of experiments, 116 in number, which lasted from December 1, 1832, until March 1, 1833. He studied the temperature of the stomach under different conditions; he attempted to imitate the gastric juice with solutions of muriatic and acetic acids; he found that when disease affects the coats of the stomach and the tongue, digestion is protracted; he found that the sense of hunger resided in the stomach and that it was as well allayed by putting the food directly into it, as when it passed in the usual way via mouth and esophagus (but "there is no indication that St. Martin was ever satisfied to receive his whisky in this manner, but took the position that this was one appetite which could not be appeased by pouring it through the opening in the stomach"); he found that the sense of taste stimulated the flow of the gastric juice. It is interesting to note that in many of the experiments on artificial digestion in which body temperature was necessary, St. Martin was made to carry vials in the axilla by the hour, with the evident idea that there were certain forces in animal heat which could not be duplicated on a water bath.



William Beaumont

In the midst of these researches he called to his aid two of America's leading scientists, Robley Dunglison, professor of physiology in the medical department of the University of Virginia, later called to the University of Maryland, and Benjamin Silliman, professor of chemistry in Yale University. What Beaumont most desired was a chemical analysis of the "solvent ingredients" of the gastric juice which up to that time had baffled all chemists. This was the task of Professor Dunglison who thought he had separated out certain constituents of the gastric juice, yet there remained certain unknown constituents which he thought might never be accurately determined.

Beaumont now called upon Professor Silliman, of Yale, and through him he was brought into touch with Professor Jacob Berzelius, of Stockholm, Sweden, and a bottle of gastric juice together with a history of the case was forwarded to the latter for analysis. After waiting for more than a year in high expectation, he received an utterly disappointing report from this great chemist, *whose analytical results were most unsatisfactory and incomplete and added practically nothing to the knowledge Beaumont already possessed.*

In the meantime Beaumont had been ordered to New York City, his furlough having expired, and here he probably thought he would be able to gain additional knowledge from the scientists of that city. Residence there, however, was distracting and barren of results, and serious study of his case was not resumed until he was assigned to Plattsburgh on recruiting duty. St. Martin, for the sake of science, was first furloughed by the authorities at Washington and then placed on duty with Beaumont, subject to his orders. While St. Martin was absent in Canada, on account of the death of one of his children, Beaumont began the compilation of his notes and experiments, receiving valuable assistance in this from his cousin, Samuel Beaumont, a physician of good standing in Plattsburgh, who before he studied medicine had "followed the business of printing and publishing." They first wrote out "a rough MS., sufficient with the experiments to make about 300 printed pages, octavo," and then spent several weeks "correcting and copying it for the type." The great surgeon who had spent his life among hardships, learned now to respect literature—"I find it an immense job to make a book, and I heartily wish it were done," he declares.

On the return of St. Martin, he conducted a fourth series of experiments, 62 in number, from July 9 to November 1, 1833, which were devoted chiefly to the time of digestion of different articles of diet. His fame in connection with gastric digestion had already become widespread, and on March 6 he had been granted the honorary degree of Doctor of

Medicine (*Doctorem in Arte Medici*) by the Columbian College of Washington, D. C., and a little later made an honorary member of the Connecticut Medical Society. During eleven years of ardent labor and untiring study, Beaumont had conducted, in the four series, 238 experiments, which were set forth in the first edition of his book, which appeared in 1833, entitled "Experiments and Observations on the Gastric Juice and the Physiology of Digestion," from the press of F. P. Allen, of Plattsburgh,—a book which to-day is a prize package on the shelves of any library.

Up to this time, the knowledge of scientific men concerning digestion had been of the most meager sort. The experiments of Réaumur on buzzards and Stevens on the human being, during the middle and latter part of the eighteenth century, had simply revealed that there was a process of solution in the stomach, as shown by the fact that substances contained in perforated tubes and balls were dissolved and liberated. Spallanzani, in 1783, proved the real solvent powers of the gastric juice, within and without the body by means of fluid obtained by the swallowing of sponges attached to strings. He gave the fluid the name of gastric juice, and established the theory of its chemical powers. Werner, Hunter, and others showed the gastric juice to have an acid reaction; but it remained for Prout, in 1824, to claim with great positiveness, after most painstaking experiments, that the acid in question was hydrochloric acid (HCl).

WILLIAM BEAUMONT AND THE DISCOVERY OF THE HYDROCHLORIC ACID OF THE GASTRIC JUICE

It has been asserted by Osler and others, that Beaumont knew positively that the gastric juice contained free HCl. This is evidently an expression of profuse admiration of our first American physiologist, for neither his published work nor his correpondence with Berzelius nor that with Robley Dunglison gives the chemical evidence proving such knowledge. Over and above all this, the chemical demonstration of pure HCl was in those days a matter of impossibility, because no method existed for the demonstration of free HCl which would to-day be considered unassailable from the standpoint of modern chemical technique.

It was not till 1852 that Bidder and Schmidt (Leipzig, 1852) proved by a chemical method, which was above criticism, that the gastric juice did contain free HCl.

The discovery of this acid in gastric juice has usually been attributed to Prout ("Philosophical Transactions," 1824, p. 47). I will insert Prout's own statement concerning his method of chemical analysis, and it will be seen that even his method would to-day be considered no absolute proof, because his very first step, where he precipitates the muriatic acid by nitrate of silver to determine the proportion of HCl with fixed alkali, is open to criticism. Nevertheless, Prout's method constitutes an admirable piece of research work, and the best chemical technique of his day is represented by it.

> The contents of the stomach of a rabbit, fed on its natural food, were removed immediately after death and repeatedly digested in cold distilled water until they ceased to impart anything to that fluid. The whole of these different portions of fluid, which always exhibited strong marks of acidity, were then intimately mixed together, and after being allowed to settle, were divided into 4 equal portions.
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> 1. The first of these portions was evaporated to dryness in its natural state and the residuum burnt in a platinum vessel; the saline matter was then dissolved in distilled water, and the quantity of muriatic acid present determined by nitrate of silver in the usual manner; and proportion of muriatic acid in union with a *fixed* alkali was thus determined.
>
> 2. Another portion of the original fluid was supersaturated with potash, then evaporated to dryness and burnt, and the muriatic acid contained in the saline residuum determined as before. In this manner the *total* quantity of muriatic acid present in the fluid was ascertained.
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> 3. A third portion was exactly neutralized with a solution of potash of known strength, and the quantity required for that purpose accurately noticed. This gave the proportion of *free* acid present, and by adding this to the quantity in union with a fixed alkali, as determined above, and subtracting the sum from the *total* quantity of muriatic acid present, the proportion of acid in union with *ammonia* was estimated.
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> But as a check to this result, the third neutralized portion above mentioned was evaporated to dryness, and the muriate of ammonia expelled by heat, and collected. The quantity of muriatic acid contained in this was then determined as before; and was always found to represent nearly the quantity of muriate of ammonia as before estimated; thus proving the general accuracy of the whole experiments beyond a doubt.
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> 4. The remaining fourth portion of the original fluid was reserved for miscellaneous experiments, and particularly for the purpose of ascertaining whether it contained any other acid besides the muriatic. The experiments above mentioned seemed to preclude the possibility of the presence of any *destructible* acid; and the only known *fixed* acids likely to be present were the sulphuric and the phosphoric; the muriate of barytes, however, neither alone nor with the addition of ammonia, produced any immediate precipitate,[1] showing the absence of these two acids in any sensible quantity, and still further confirming the results as before obtained. In this manner the three follow-

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[1] Ammonia after some time gave a flocculent precipitate, of earthy phosphate in union with vegetable and animal matter, and after combustion traces of sulphuric acid, the result of that process, were very perceptible; but the experiment proves that neither existed free previously in the orig. fluid.

ing results selected from a variety of others of a similar nature, were obtained:

	No. 1. grs.	No. 2. grs.	No. 3. grs.
Muriatic acid in union with a fixed alkali [2]	.12	.95	1.71
" " " " " ammonia	1.56	.76	.40
" " in a free and unsaturated state	1.59	2.22	2.72
Total	3.27	3.93	4.83

These results then seem to demonstrate that free, or at least unsaturated muriatic acid in no small quantity exists in the stomach of these animals during the digestive process; and I have ascertained, in a general manner that the same is the case in the stomach of the hare, the horse, the calf, and the dog. I have also uniformly found free muriatic acid in great abundance in the acid fluid ejected from the human stomach in severe cases of dyspepsia, as the following examples show. The original quantities of the fluids operated on of course were various, but for the sake of comparison they are reduced, in the following table, to one pint or 16 fluid ounces, which quantity, in 3 instances (selected from many others) was found to contain:

	No. 1. Grs.	No. 2. Grs.	No. 3. Grs.
Muriatic acid in union with a fixed alkali [3]	12.11	12.40	11.25
" " " " " ammonia	0.0	0.0	5.39
" " in a free or unsaturated state	5.13	4.63	4.28

CONCEPTIONS CONCERNING ACIDITY

Even at the present day the best authorities differ as to what really constitutes an acid. The definition may be attempted from either of two viewpoints: *first,* from the *purely chemical* viewpoint, *secondly,* from the *physico-chemical* viewpoint. A chemist conceives an acid to be a combination in which hydrogen atoms can be replaced by metallic atoms. By the entrance of the metallic atoms the acid character is neutralized. The degree of acidity can therefore be determined by the quantity of decinormal solution of alkaline hydroxide which is necessary to replace the hydrogen atoms. The physico-chemist, on the other hand, defines an acid as a chemical combination which, when dissolved in water, dissociates free hydrogen atoms (H–). According to the degree of this dissociation we can distinguish between strong and weak acids. In a certain definite volume of a strong, and of a weak acid, the weak acid will be less dissociated than the strong.

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[2] For the sake of analogy, the Cl in union with the bases of the *fixed* alkali is reduced in this next table to the state of HCl.

[3] In No. 3 of the above table only, could I detect ammonia in the fluids ejected from the human stomach. Sir Astley Cooper, who gave me the fluid, for examination, said the patient often took ammonia as a medicine.

The difference between these two viewpoints will become immediately clear when we determine the acidity of a 1/32 normal HCl solution and a 1/32 normal acetic acid solution. From the standpoint of the chemist both these dilute normal solutions of acids should have the same acidity when taken in exactly the same quantity. From the standpoint of the chemist 1 liter of 1/32 normal HCl and 1 liter of 1/32 normal acetic acid require the identical amount of deci-normal solution of sodium hydroxide (NaOH) for their neutralization. But, for the physico-chemist the acidity of 1/32 normal HCl is forty (40) times as great as a 1/32 normal acetic acid solution, because the stronger acid dissociates more than the weaker acid. The degree of dissociation of 1 liter of HCl in a dilution of 32 liters of water is equal to 0.97, but that of the weaker acetic acid is equal to only 0.024.

Concerning the method of determining the acidity by titration, the chemist determines the *dissociated* H ions, as well as the *combined* H ions. The latter, which in certain dilutions are not yet dissociated, are called the "potential" H ions, while those that are dissociated are called the "actual" H ions. The physico-chemical chemist determines only the "actual" H ions; the chemist determines both. For the physico-chemist the urine generally reacts neutral or very faintly acid. There is no direct dependence between the H ion acidity and the acidity determined by titration. Concerning the technical difficulties involved I may quote the following letter:

The Johns Hopkins University, Department of Chemistry.
November 6, 1919.

To Dr. John C. Hemmeter: Outline of Method for Determining the Presence of Free HCl in a Mixture of NaCl and Organic Acids:—

Theoretically there must always be a certain amount of free hydrochloric acid in a mixture of sodium chloride and organic acid. It is true that the amount of free HCl will be small, but nevertheless it must be there, and furthermore its concentration can be calculated provided the strength, i. e., the dissociation constant of the organic acid is known and the concentration of NaCl and the organic acid. The method of attack is that of determining the distribution of the base between two acids of different strengths, in this case HCl and an organic acid.

Practically the problem in the present case is not capable of calculation because of the fact that a mixture of organic acids is present. However, the presence of a free HCl could be shown by the following experiment. Pass a slow current of air through the gastric juice, absorbing the saturated vapors as they emerge from the solution in a known volume of standard alkali.

At the end of the experiment, titration of the alkali would give a measure of the total acid carried over by the air stream. A chloride determination of this titrated solution would now tell us the amount of HCl carried over. The only possibility of this method not giving conclusive proof of the existence of free HCl would be the pressure of volatile organic acids containing chlorine.

Sincerely yours,
Walter A. Patrick,
Associate Professor Phy. Chem.

CHEMISTRY EXPERTS CONSULTED BY BEAUMONT

It will throw an important light on the conditions which surrounded the physiological and chemical work of William Beaumont if I enter very briefly into the history of Robley Dunglison (1789–1869), Benjamin Silliman, Sr. (1779–1864), and Jons Jacob Berzelius (1779–1848). Silliman's biography is given by Daniel Coit Gilman in "Leading American Men of Science," edited by David Starr Jordan, page 89. Doctor Gilman, who was the first president of the Johns Hopkins University and who called Ira Remsen to the chair of chemistry at that University, describes Silliman's first lectures in chemistry (l. c. page 96). It was in 1801 that the Yale College had resolved to establish a professorship of chemistry and the questions are significantly asked, where should the prospective professor of chemistry fit himself for the post to which he was unexpectedly called (meaning Benjamin Silliman)? Where could he turn for instruction? Whom could he consult? Philadelphia was then the seat of science in America and Dr. James Woodhouse was the lecturer on the subject of chemistry and some éclat was given to his instruction by the fact that he had been in London and heard and observed Sir Humphry Davy. Silliman's picture of the situation in Philadelphia is not altogether flattering. He describes Woodhouse as being in advance of his time, for he ridiculed the idea that the epidemic of yellow fever in Philadelphia was a visitation of God for the sins of the people. The lecture rooms were crowded, the apparatus was of the most humble kind, and there was no assistance, but the experiments were numerous and made a strong impression on Silliman. Silliman's own lecture course was given in a subterranean lecture room at Yale College which had been fitted up as a laboratory. But it was so inconvenient that the young chemist was obliged to get several members of the corporation into the gloomy cavern fifteen feet below the surface of the ground, before they could be persuaded to improve his faulty location. In this deep-seated laboratory, Silliman worked during fiften years of the best years of his life, and he has left very interesting and particular descriptions of the extremely simple apparatus which he used. He was much encouraged by a description of Priestley (see article on Lavoisier) concerning the defective equipment which this discoverer of oxygen had. "With Florence flasks, cleaned by sand and ashes, and plenty of glass tubes, vials, bottles, and corks, a tapering iron rod to be heated and used as a cork-borer and a few live coals with which to bend the tubes, a good variety of apparatus might be fitted up. Some gun-barrels, also," he said, "might be of much service." Silliman adds, "I had brought from Philadelphia an old blacksmith's furnace which served for heating the iron tube." Some idea of the easy chemical conscience of those days is given us by the remark that sand and bran with soap would make the hands clean and that there was no sin in dirt. Imagine a modern chemist with balances sensitive to an almost incredible degree saying there was no sin in dirt.

Dr. William H. Welch refers to Silliman in his essay on the *Relation of Yale to Medicine,* address at the 200th anniversary of the founding of Yale College, New Haven, Connecticut, October 21, 1921.[4] Welch remarks that Silliman in attending lectures in Philadelphia and Edinburgh, came under the influence of such famous medical teachers as Wistar and Barton in Philadelphia, and James Gregory and John Barklay in Edinburgh. It appears that he did not receive instruction from Wistar and Barton, but simply met them, as well as the illustrious Joseph Priestley and Benjamin Rush.

The equipment of Robley Dunglison as well as his personal qualifications for exact quantitative chemical analysis, were almost inferior to those of Silliman. It is clear that in those days quantitative determinations for free HCl could not be carried out in the laboratories of either of these teachers of chemistry, and they were the best that William Beaumont could consult in this country.

> "It happened that when old Thomas Jefferson was organizing the University of Virginia in 1824, he, failing to find a man for the chair of anatomy, physiology, materia medica, and pharmacy, wrote to London to a learned young man only twenty-six but one who had already written a 'Treatise on Children's Diseases,' and was editing the 'London Medical Repository,' to come on. This man, Robley Dunglison, born in Keswick, England, in 1789, was destined to be a merchant, but fortunately a rich uncle, one Joseph Robley, died and left him enough to become a doctor. So when seventeen, after a good education, he began to study medicine under a village physician before attending courses at Edinburgh, Paris, and London, taking his surgical degree at the Royal College of Surgeons, London, 1819, and his medical degree at the University of Erlangen in 1823.
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> "In 1825 Yale conferred on him her LL.D. . . . For nine years he stayed at the University of Virginia going afterwards as professor of materia medica and medical jurisprudence to the University of Maryland. Thereafter he was

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[4] This address is found in *Papers and Addresses by William Henry Welch,* Vol. III, page 243.

made professor of the institutes of medicine in Jefferson College, which appointment he held until 1868.

"During the nine years in Virginia his industry was amazing. The 'Human Physiology,' rejected by Philadelphia publishers, came out at Boston in 1832, and went through eight editions and became at once the book for students. S. D. Gross says, 'What Haller's great work accomplished for surgery in the eighteenth century, Dunglison accomplished for it in America in the nineteenth.' The book is rich in learning, accurate and logical in its statements of facts. His big 'Medical Dictionary,' 1833, a work of profound erudition, earned him a world-wide reputation; 55,000 copies were sold during his life-time, and in 1897 it had reached twenty-three editions. These books were followed in rapid succession by treatises on 'Materia Medica,' 1843; 'Hygiene'; 'The Practice of Medicine,' 1842, and 'New Remedies'; yet this systematic and persistent writer found time to edit 'Forbes' Cyclopedia of Practical Medicine' and several foreign works. He founded and edited for five years the 'American Library and Intelligencer,' and with one William Chapin issued a celebrated dictionary for the blind in three folio volumes, and all this besides innumerable articles to the medical and lay journals." (From an article by Davina Waterson in Howard A. Kelly's "Cyclopedia of American Medical Biography," page 264, Vol. 1.) [5]

Berzelius, who had studied medicine and chemistry in Upsala, devoted himself in 1807 to the development of Dalton's atomic theory. He elaborated the analytic methods for the determination of the elements so carefully that a reliable determination of the relative atomic weights became possible. He also established a system of atomic weights which has recently been revived and come to honor again.

Beaumont sent some of the gastric juice obtained from the stomach of Alexis St. Martin to Berzelius, but on the last page of his book, p. 276 Plattsburgh edition, he states, "I regret, exceedingly, that I have not been able to obtain returns from Professor Berzelius, to whom I transmitted, about seven months ago, a bottle of gastric juice for chemical examination. I could not, however, consistently with the expectations and wishes of my friends, further delay the publication of these experiments." Finally, on July 19, 1834, an answer from Berzelius reached Professor Silliman. This was in reply to the material and a letter sent April 17, 1833. One should not be too critical at this date, January, 1925, of almost perfect analytic methods, in judging the letter of Berzelius. There are two weak points about it, however, that should not be suppressed. He was requested to throw light on chemical composition. All he did in that way was to evaporate 266.76 grams of the gastric juice in vacuo at room temperature, until he had a residuum of 3.385 grams. But he said it was filled with crystals of sodium chloride. In a chemist of his brilliant ability, it strikes one as astonishing that this abundance of crystals of sodium chloride did not suggest to him the question of their derivation. The next most striking thing about the letter of Berzelius is that he asks eight questions, mostly not chemical, but physiological. It is for this reason that the reply of Berzelius was so disappointing. How much different and exact and confidence-inspiring are the analyses of Bidder and Schmidt, published at Leipzig, 1852, eighteen years after. Berzelius had every laboratory equipment to determine that there were not enough of bases contained in the gastric juice to combine with the entire quantity of chlorine that was determined, and therefore as the gastric juice did not contain free chlorine, this substance could only have been HCl. One of the best treatises on this subject is that by Martius and Lüttke, entitled *Die Magensäure Des Menschen,* published at Stuttgart, Verlag von Ferdinand Enke, 1892.

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[5] From the very breadth of Dunglison's interests it must be evident, that he could not possibly have been a competent analytical chemist. Personally I have not succeeded in finding the evidence that Robley Dunglison demonstrated any definite constituent in the gastric juice that Beaumont gave him.

In my clinical practice I have been almost daily called upon to make analyses of gastric juice, and whilst clinical methods cannot claim to possess the exactness required by the technical analytical chemist, nevertheless the routine analysis of gastric juice year in and year out has of necessity made me familiar with the exactness of the methods, and to the present day I know of no more dependable method when quantitative results are desired, than that of Bitter and Schmidt. With practice, this method yields as satisfactory results as any. Berzelius does not state that he made any effort at chemical analysis, but simply aimed to determine the solid residue and then preserved it under alcohol.

The relation of William Beaumont to the American chemists Dunglison and Silliman, and his disappointment with Berzelius, is introduced into this biography to throw an interesting sidelight on the state of chemistry at that period, only ninety years ago. The chapter on Lavoisier (XXI) will demonstrate how much more exact analytical methods had become under this great French physiologic chemist. The difficulty appears to have been with the facility for expansion of new chemical knowledge. If a new method was originated in Paris, it required sometimes a year before it was even known in Stockholm. Other difficulties arose because of the lack of proper journals for the dissemination of chemical knowledge and the rarity of international conventions of chemists, physiologists, and clinicians. These factors may explain the differences in the degree of perfection in chemical analysis which are found in the various universities of Europe about a hundred years ago. All the more we are compelled to admire the scientific enthusiasm, the genuine tenacity and perseverance of a true investigator in William Beaumont who did not shrink from his very laborious and frequently very disappointing work, associated with many embarrassments and unexpected difficulties. Although an army surgeon stationed in a lonely post in the wilderness, in what was then called the territory of Michigan, more than 4,000 miles distant from anything like a chemical or physiological laboratory, and more than a thousand miles from any university, he still produced a work which will be counted as one of the classics of physiology as long as the history of that science will be written.

If now we compare the work of Beaumont with that of recent workers on the physiology of digestion, we will find that he anticipated some of their best results, notably the researches of Pavlow on the work of the digestive glands, of Cannon on the mechanics of digestion, etc. To quote the words of Vaughan, "He made such an exact study of the physical and chemical natures of the gastric juice that, with the exception of the discovery of pepsin, the closest research of modern times has added but little to the work done by him."

Although Beaumont had not succeeded in his efforts to find a publisher for his book in London, and was more or less disappointed with its recognition at home, yet he had become an international character. Reviews were given in a number of foreign journals, and more important still a German edition was issued: *Neue Versuche und Beobachtungen über den Magensaft und die Physiologie der Verdauung. Auf eine höchst merkwürdige Weise, während einer Reihe von sieben Jahren, an einem und demselben Subjecte angestellt, von Dr. Wilhelm Beaumont, Wundarzt in der Armee der Vereinigten Staaten Nordamerika's. Aus dem Englishen übersetzt von Dr. Bernard Luden, prakt. Arzt in New York. Mit drei lithographirten Abbildungen. Leipzig, 1834 Christian Ernst Kollmann.* A little later Sir Andrew Combe published an English edition of Beaumont's work.

During the publication of his book, Beaumont had spent a great deal of time and energy in the effort to induce the Government to reimburse him for the expense already incurred and to enable him to continue his experiments on St. Martin in this country or abroad, and after receiving encouragement from his friends he finally made application to the Twenty-third Congress for this reimbursement. However, in consequence of the non-scientific attitude and lack of proper appreciation of the work by members of this Congress the bill was lost by a vote of 129 to 56. This naturally was a great disappointment to Beaumont and his friends. When the Twenty-fourth Congress convened, he was on the field again with a new application, this time asking only for compensation for the services he had rendered St. Martin, but the bill was this time "pigeonholed."

The contract between Beaumont and St. Martin having expired, a new one had been executed, this time for a period of two years, beginning November 7, 1833, for a consideration of $400 to be paid in instalments.

In the early part of 1834, Beaumont was ordered on a tour of inspection of the hospitals at Forts Trumbull, Walcott, Preble, Constitution, and Sullivan, and Hancock Barracks, and it is interesting to note that the ideas which he embodied in his report to the Surgeon-General were a half century ahead of their time and compare very favorably with modern ideas. Upon his return he made a short trip with St. Martin to New Haven and Boston, where he exhibited him before medical societies. St. Martin was then permitted to make a short visit to Canada, promising to meet Beaumont in Plattsburgh by June 15 and go with him to his new post at Jefferson Barracks, twelve miles south of St. Louis. When the time arrived, St. Martin failed to meet him and, disappointed again, he proceeded alone to the Barracks.

From this post, after serving during the summer and fall of 1834 and enjoying a three months' leave of absence in Plattsburgh and Washington, Beaumont was transferred to the Arsenal at St. Louis, where he arrived August 1, 1835, accompanied by his family. At this post he was to have the privilege of engaging also in private practice, and within a year after opening his office he had a very large and lucrative practice. About this time, too, he received an offer of the chair of surgery in the new Medical department of the St. Louis University, which was the first medical department established west of the Mississippi. This offer, after some hesitation, he accepted subject to permision from Washington, but for some reason the University was not opened till several years later, and there is no evidence that he ever delivered any lectures there.

In the meantime, Beaumont's old friend, Surgeon-General Lovell had died and Dr. Thomas Lawson had been appointed his successor. Friction soon appeared between Beaumont and the new Surgeon-General, and on November 15, 1839, he was ordered by him to Fort Brooks, Florida. Unable to secure a reconsideration of this humiliating order, Beaumont resigned from the service, and on December 31, 1839, his resignation was accepted. Thus ended his brilliant and honorable military career of more than twenty-five years.

Beaumont now purchased a country home, and entered into partnership with a young doctor named Sykes, who was to relieve him of a part of the burdens of general practice. After two years this was dissolved through disagreement. Several years later he formed another partnership with a young doctor named Johnson, but in a short time it ended in the same way. Soon after his arrival at St. Louis, he became a member of the Medical Society of Missouri; he served as its president in 1840, but his administration was not a successful one. A new edition of his work appeared in May, 1847, revised by his cousin Dr. Samuel Beaumont, of Plattsburgh, N. Y.

During all these years Beaumont had not lost interest in St. Martin, for he was continually on the lookout for him; always hoping to carry out more experiments on the gastric fluid. English scientists, as well as those in our own country, were also after him. He was ever disappointing and evading his benefactor; going off on sprees and always demanding money; never to be relied on.

Beaumont even went so far as to send his son into Canada to obtain St. Martin in May, 1846, but he would not consent to leave, although living almost in poverty,—another great disappointment. In the latter part of 1850, having sunk to the very depths of poverty, he finally consented to leave Canada and come once more to his benefactor, but after starting on his journey he fell sick and returned home; after which all attempts to persuade him to leave again were of no avail, and Beaumont's wishes were never fulfilled.

On April 25, 1853, Beaumont died from general infection that was initiated by a carbuncle on the neck. His good wife, Deborah, survived him many years. St. Martin lived to the age of eighty-three, dying in poverty in Canada.

A modest marble slab in the beautiful Bellefontaine Cemetery of St. Louis marks the resting place of the great pioneer physiologist; the medical profession of Michigan has erected a memorial tablet at the site of his first experiments; medical schools bear his name; but the greatest memorial of all of this modest scientist is the undying memory of his achievements treasured in the literature of many lands.

ESTIMATION OF BEAUMONT'S WORK

In an address at the Washington University Medical School, of St. Louis, Missouri, Joseph Erlanger, Professor of Physiology in that institution, after quoting Beaumont thus: "My opinions may be added to, denied or approved, according as they conflict or agree with the opinion of each individual who may read them; but their worth will be best determined by the foundation on which they rest—the incontrovertible facts," presents a list of these facts which may form a fitting conclusion to this chapter.

Beaumont proved that the juices of the stomach are formed by the walls of the stomach, by actually watching, with the aid of a hand lens the collection of the juice at the gland orifices in the mucous membrane.

He showed that this juice is formed through contact of food materials with the wall of the stomach; that foreign bodies which are not foods do not supply an adequate stimulus for the secretion of the gastric juice. That the small amounts of fluid contained in the stomach empty of food represent swallowed saliva, and mucus which is formed on the surface of the mucous membrane for the purpose of protecting it, is not to be regarded as digestive juice.

He settled the moot question as to whether or not the gastric juice is acid in reaction, by proving, by the sense of taste, that the acid juice is formed only when food is taken into the stomach, while the slight secretion found in the stomach at other times is not acid, and indeed, as has just been said, is not gastric juice at all (thus confirming Tiedemann and Gmelin (Jesse S. Myer, Life and Letters of William Beaumont, St. Louis, 1912, p. 181).

He settled conclusively the question as to the nature of gastric digestion in man by proving it to be purely chemical rather than a mechanical or so-called vital process.

He obtained presumptive evidence of the existence in the gastric juice of another digestive principle beside the acid.

His experiments imply that he believed that there is a definite relation between the quantity of gastric juice formed and the amount of food ingested.

He made extended observations and reports on the time required for the completion of the gastric digestion of different diets.

He noted that fluids pass from the stomach very soon after they are ingested.

He noted that oils and fats retard the emptying of the stomach.

He observed that starches pass from the stomach more rapidly than any other foods.

He noted that certain foods which are not acted upon by the gastric juice (raw cabbage, for instance) may very quickly be passed on into the intestines, while at the same time digestible materials may be retained in the stomach.

He showed that bone dissolves in the gastric juice, provided a sufficient quantity of juice is supplied.

He observed that the emptying of the stomach into the intestine begins very early in the process of digestion, but that it is accelerated toward the close of digestion. The emptying and its acceleration he by implication ascribes, at least in part, to the presence of the gastric juice, for he says: "Toward the latter stages of chymification the chyme becomes more acid and stimulating, and passes more rapidly from the stomach."

He reports the frequent finding of bile in the stomach some time after it has become empty of food.

He finds bile in the stomach after the ingestion of a meal rich in fats. And shows that fats are digested better by bile-tinged gastric juice than by colorless gastric juice, while the gastric digestion of other than oily food is retarded by the admixture of bile. And states clearly that bile is called into the stomach only for the chymification (emulsification) and solution of oil by the gastric juice. And also shows that the addition of bile-stained gastric juice to the digested contents of the stomach causes a precipitate to be formed.

There are indications that he noted an interruption of gastric movement by the act of swallowing; and that the stomach contracts on its contents (latter also observed by Magendie).

By the use of the stem of a thermometer he followed the gastric movements, probably incited by irritation of the pyloric region of the stomach. And noted, toward the end of digestion, contractions of the pyloric region that recurred more or less rhythmically every two or three minutes.

He confirmed Magendie in showing that, when the stomach is empty, noises caused by the motion of the air in it almost always occur; thus laying the foundation for the recent conclusive demonstration of hunger contractions of the stomach.

He showed that in man the sense of hunger subsides immediately, when food is introduced directly into the empty stomach; and inferred that the sense of hunger resides in the stomach.

He showed that anger and impatience interrupt the process of digestion; and found that no gastric juice is secreted during fever.

He noted that the temperature of the pyloric region of the stomach is usually higher than that of the fundic region; which is now explained by the proximity of the liver.

Only by reading his book and manuscripts and letters, given into the custody of the Washington University, can the real man be discovered . . . an investigator who worked with no other incentive than "an honest desire to contribute a mite to Medical Science" . . . whose pioneer work marked a new era in medical progress for the New World.

XXIX

GOETHE AS A NATURALIST

SYNOPSIS OF BIOGRAPHY

TO give a detailed account of the literary and scientific productions of Goethe is altogether impossible within the limits of the present work; much less can we attempt any detailed criticism of these works. The best source of information to one acquainted with English alone is the biography of the poet by George Henry Lewes, along with which may be taken Goethe's interesting conversations with Eckermann, translated at Oxford; also Peter Hume Brown on Goethe. The best German sources of information are Albert Bielchowsky: "Goethe, Sein Leben und seine Werke," München, 1907; and Paul Burg: "Ein Goethe-Roman" in 5 Bänden. The work of Heinrich Düntzer, "Goethes Leben," and that of Rudolf Magnus, "Goethe Als Naturforscher", as well as "Goethe Als Naturforscher" by Rudolf Virchow, are classics of their kind, bearing on the life and work of this great man.

On the general character and literary position of Goethe however, a few words are necessary. It is as a poet, no doubt, that this remarkable man is generally known and recognized in America; but it is not as a poet only that a just estimate can be made of his intellectual caliber, or his significance to European civilization. It is as a poet, thinker, critic, and original observer of nature, all combined in admirable harmony, for which he is so remarkable. We do not find in literary history any intellect that can fitly be placed on the same plane with Goethe; that presents, with such wonderful breadth and yet such grace, so much severe thought, combined with so much luxuriant imagination; so much accurate science with so much playful fancy; so much simplicity with so much art; so much freshness and originality of productive power, with so much fairness and comprehensiveness of critical judgment.

"Faust," a magnificent poem, is in fact a great dramatic poem, for it is full of dramatic scenes, full of the living current of dramatic action. "Faust," although essentially a German poem, is yet a poem whose appeal and significance is universal. It is the great drama of that moral and metaphysical questioning which thoughtful minds must go through in all times and places, but which has received the fullest and most fruitful development in modern Germany. Of the other poetical works of Goethe, "Iphigenie," "Hermann and Dorothea," and "Tasso," are those which most strongly bear the type of the ripe manhood of the author. The form and style of these classical works are characteristically Greek; by which we mean they are chiefly remarkable for profundity of thought and truth of feeling, expressed in the most simple, graceful, and unpretending manner. In soul, however, they are essentially German, and the most deep-thinking of the Germans are always the first to claim Goethe as the most German of all German poets in spirit, though very few great German writers have so carefully avoided the most characteristic German defects of style. In the extraordinary value which he attaches to "the form," Goethe authenticates himself everywhere as at once a great modern Greek and a great artist. This inherent, deep appreciation of form is also a fundamental guiding principle in the amazingly original research into pioneer territories of botany, where he actually became the author and founder of a new science, the morphology of plants, but also in his contributions to comparative anatomy and physiology. These special researches were conducted with a thoroughness, systematic objectiveness, and logical sequence which even in the light of the most modern methods of research, command the admiration of the most distinguished savants and investigators.

Johann Wolfgang von Goethe, the acknowledged prince of German poets, one of the most highly gifted and variously accomplished men of the eighteenth century, was also a great naturalist. He was born in the year 1749 at Frankfurt-on-the-Main, where his youthful years were spent. His father, Johann Kasper Goethe, was an imperial councillor, in good circumstances, and in a respectable position. His grandfather on his mother's side was the President of the Supreme Court of the City. From his earliest youth, Goethe enjoyed all the advantages of a comfortable home and the stimulating influence of a great and prosperous city according to the standards of the time.

In his autobiography, "Dichtung und Wahrheit," he tells us among many other things affecting his youth, how he witnessed the pomp and ceremony of the coronation of the Emperor Joseph II. For Frankfurt since ancient times enjoyed the privilege of being the place where the Emperor was elected and crowned.

In the year 1765, he went to the University of Leipzig, of which Ernesti and Gellert were then the most notable ornaments. No place in Germany at that time enjoyed a higher reputation for culture and refinement. His own special study of jurisprudence, which he pursued in accordance with the wishes of his father, did not attract him as much as the courses in literature offered by the University and the opportunity to study art, more particularly the treasures of Roman and Greek antiquity. Disregarding the dictates of careful living, the youth brought upon himself an illness of a serious nature which forced his return to the parental home in 1768. This illness had given to his mind a serious turn and engendered an interest in the pseudo-scientific efforts of earlier alchemy.

As a student, he was avowedly concerned with legal studies, but his real studies were in the wide domain of literature, philosophy, and above all a contemplation of life, in all its manifestations. In the year 1770, he went to Strassburg to finish his law studies; but here also, anatomy and chemistry, Shakespeare, Rousseau, and architecture—anything rather than the statute books—occupied his time and exercised his soul. But more important than all this, he made the acquaintance of Herder, to whom, although he was only five years his elder, Goethe looked up as a disciple to his master. Herder had already attained an assured place in the hearts of the younger generation by his critical writings that loudly proclaimed the right of the individual on the basis of national peculiarity, and glorified Homer's natural art and Shakespeare's genius with a fervor that had not been witnessed before.

Here it was that one of the earliest, certainly the most famous, of those youthful love adventures took place, which, in his biography, as in that of Robert Burns, play such a prominent part—the well-known affair of Frederica Brion of Sesenheim. This passion found expression in verses that revealed for the first time the latent genius of Germany's greatest poet. With regard to Goethe's various love affairs, it may be said in this connection that he was more readily moved to love, than intense in love, and that the objects of his admiration generally seem to have had more reason to boast of his complaisant susceptibility than of the perseverance of his devotion. How much or how little there was of what is commonly considered culpable in these connections, will always be an open question. Some find it difficult to forgive the man who was so facile to lend his heart and so loath to give his hand; and present-day precepts of morality will always be inclined to pass severe judgment on the man who, professing the profoundest submission to law and order in everything else, seems to have felt no scruples in waiving all restraint in matters pertaining to the tender passion, shrinking from the golden clasp of legitimate marriage as from some conventional shackle. In the year 1771, the young poet, now twenty-two years of age, took his degree as Doctor of Laws, and went for a short while to Wetzlar on the Lahn, which was the seat of the imperial council of the German empire at that time, and which afforded peculiar facilities for young men engaged in the study and practice of the law. Here, however, as in other places, his knowledge of the human heart and character altogether outgrew his professional studies; and Wetzlar became to him the scene of the famous "Sorrows of Werther" (*Werthers Leiden*), a glowing leaf from the life of the human soul, full of interest and beauty at all times, but which, with the state of European thought and feeling as it was then, aroused the whole literary mind of Europe like a storm raging through a forest. The book was not published till 1774. After returning from Frankfurt, Goethe spent some years in his native city, engaged chiefly in literary productions. His first great work was "Götz von Berlichingen," (which has been translated into English by Sir Walter Scott), published at Frankfurt in 1773, which at once liberated the Germans from the painful constraint of French and classical models and permitted them to take up that career of bold originality, which they have since prosecuted in so many departments of literature, learning, and philosophic speculation. In the year 1775, Goethe, who had the fortune to gain the good will of Karl August, Grand Duke of Saxe-Weimar, accepted an invitation from that prince to settle in his little capital, since become so famous as the Athens of the great formative period of German literature. Here the poet became a statesman; and busying himself in various ways in the service of his benefactor, passed quickly through the stages of court advancement, till in 1779, he became "actual privy-councillor," at the age of thirty, holding the highest dignity that a German subject could then attain, a distinguished, rich and an influential man. In 1782, he received a patent of nobility; and in the following years, till 1788, traveled much in Switzerland and Italy, the fruits of which last journey were embodied in such beautiful works as "Iphigenie," "Egmont," "Tasso," and the "Venetian and Roman Elegies." Of this last work, thoroughly German both in form and feeling, the heroine was Christiana Vulpius, a highly attractive though not a highly gifted woman, who bore him a child—his eldest son—in 1789; but whom, though he always treated her as his wife, he did not formally marry till 1806. In 1792, he took part in the German campaign against France, of which he has left a memoir.

The period of Goethe's friendship with Schiller may be regarded as the climax of his rich life. Schiller was Professor of History at the University of Jena, which Goethe had fostered with special care. This friendship, beginning in 1794, lasted until Schiller's premature death in 1805. In their correspondence, which has been preserved, the accord of these two great minds and souls finds expression, and the stimulus Goethe received is revealed in the maturity and perfection of the work of this period.

It is largely owing to Schiller's friendly interest that Goethe completed the first part of "Faust" (1808), which with the second part, only finished in 1832, the year of his death, represents in the universality of its range and its poetic worth what may justly be regarded as the greatest contribution to literature since Shakespeare's day.

In the year 1815, he was made minister of state. After the death of the grand duke, in 1828, he lived much in retirement, occupied occasionally with poetry, but much more intensely and constantly with the study of nature and the fine arts, which from his earliest years had possessed the strongest attractions for him.

In his later years, Goethe was the acknowledged dean of European letters. With the calm balance of mind he viewed the affairs of man and followed the progress of science. He was not only one of the greatest poets, but a man whose range of interests exceeded that of any other man of letters.

Goethe died March 22, 1832, at Weimar, in his eighty-fourth year.

DISCOVERIES IN SCIENCE

The peculiarity of Goethe's personality rests in the final analysis, upon the inward harmony between his study of nature and his artistic life. The two directions of his creative activity, the artistic and scientific, sprang from the same source, and each permeated and deeply affected the other. It is only from this point of view that we can understand why he should have devoted more than fifty years of his precious life, with hardly an interruption, to the science of nature.

Goethe, himself, tells us what occasioned him to take up his various studies of nature, but we may assert with confidence that the occasions were merely accidental and not in themselves determining factors; that, moreover, he would have become a naturalist under any circumstances, for he had been led to nature in a most individual way and by his own most characteristic impulses. As he tells us in his autobiography, "Dichtung und Wahrheif" (Fiction and Truth), he had from his earliest years felt an impulse to investigate natural things. That this is "truth" and not "poetry" we know from the fact that the young friend of the liberal arts and belles-lettres, and student of law, evidently took the greatest interest in scientific lectures while at Leipzig, and still more so while at Strassburg, where he studied anatomy and even attended a course of lectures on the clinic of midwifery. Animated by an insatiable desire for knowledge, he was further encouraged in these efforts by his associates, both in Leipzig and Strassburg, who for the most part were students of medicine; and he pursued these studies with the greatest industry since he thought thereby to retain the respect and confidence of his Strassburg "society" which he had immediately won by his "strange rudimentary learning, or rather, his overlearnedness."

These studies prepared him for collaboration on Lavater's "Physiognomische Fragmente" (Physiognomic Fragments), which became a determining influence in his life in so far as it introduced him to that field of knowledge in which he was destined to make discoveries of most fundamental importance, viz., anatomy and more especially osteology. In physiognomy Lavater urged the necessity of giving special consideration to the solid parts of the human organization, the bone formations, and in his contributions on animal skulls (1776) Goethe expressed his conviction that one can see most plainly by the difference between skulls "how the bones are the foundations of formation and embrace the qualities of a creature. The movable parts are formed according to them, or, to be more exact, with them, and perform their functions only in so far as the solid parts permit them."

Without these preliminary studies how would it have been possible for Goethe, even though he was able to "grasp much in a few days," to gain in a week such a mastery of osteology and myology—Loder, professor of anatomy, began to demonstrate the subject to him at the end of October 1781—that shortly afterward from a pupil, he developed into a teacher, able to deliver lectures on the human skeleton at the Academy of Design? This fact leads us to surmise that he may have been guided in his osteological studies chiefly by artistic interests and aims. But the more profoundly he grasped the subject, and the more familiar the knowledge became to him through conversation and correspondence with the most learned anatomists of his day, the more absorbing became his interest in osteology from the scientific side. In his understanding of this branch of anatomy, he was aided particularly by Merck, who, though but an amateur, possessed a rare knowledge of the subject, stood high in the estimation of specialists, and, like Goethe, was an enthusiastic and fortunate collector of specimens. In the spring of 1784, Goethe discovered a little bone in the upper jaw of a human skull which scholars asserted did not exist there, and this successful outcome of his investigations, gave him so great joy that "it sent a thrill through every fiber of his being." He wrote to Herder: "In accordance with the teaching of the Gospel, I must hasten as quickly as possible to inform thee of the good fortune that has come to me. I have discovered—neither gold nor silver—but something that gives me unspeakable joy—the *os intermaxillare* in man!"

The intermaxillary bone was a brilliant example by which Goethe was first able to illustrate the great self activity of his own nature, as he was again, a few years later, by the metamorphosis of plants. In his "specimen," as he called it in a letter to Merck, December 17, 1784, he not only proves the existence of this bone in man: he also shows how its shape varies according to the shape of the animal, the formation of the teeth, and the kind of food, extending forwards in some and backwards in others, and finally in the noblest creature, man, "modestly hiding itself for fear of betraying animal voracity."

The discovery was not an easy one to make, otherwise it would not have remained a moot question for centuries. The difficulty of recognizing the real truth lay in the fact that in full-grown skulls the bone is completely grown together with the adjacent bones, and it is only in young specimens that the attentive observer is able to see sutures along the side. Goethe arrived at his discovery by the comparison of animal and human skulls of different ages, and this method of comparison, which, instead of confining itself to the exterior, enters into the structure and contexture of the forms under investigation, is a further feature of the discovery that is of fundamental importance. The bone could not be wanting; it had to be present; it was required to complete the harmony of the whole. A similar method of reasoning, based on his contemplation of the great Strassburg Cathedral, had revealed to the young student Goethe the original plan of the architect, that the tower of the edifice should end with a five-pointed crown.

All his efforts to obtain recognition of his discovery among specialists failed, except in the case of his teacher, Loder. For a long time it was not granted to the poet to "legitimize" himself in the "learned body" of anatomists by means of his "inaugural dissertation." It was sent first on December 19, 1784, to Darmstadt, to Merck, then to Cassel, to Sömmering, and finally to Stavoren, Holland, to Camper, the most celebrated anatomist of the time, who did not receive it till the middle of September, 1785, nine months after it had been started on its rounds. Most carefully prepared and very distinct drawings of the skulls investigated by Goethe were intended to demonstrate the difference in form in different animals of the bone wedged in between the two halves of the upper jaw, and to show its existence in man. They also contained among their number different animal skulls in which the bone was either partly or wholly grown together with adjacent bones. The author's name was not mentioned, and Camper in all honesty subjected the treatise to a thorough test, making a new investigation of skulls of various ages; but he held fast to his old view that man has no intermaxillary bone. In other respects he confirmed all of Goethe's observations, even that concerning the walrus, in which the bone had not been recognized because of its compressed, misshapen form, and of which it had also been said that it had no incisor teeth. Goethe remarked that, judging by the form of the intermaxillary, one must ascribe to the walrus four incisors. Camper considered this remark likewise correct and wrote to Merck concerning the intermaxillary: "Your friend—Mr. Goethe, I presume—has set us to seeking and examining a bone which would have remained unknown in the walrus, if we had not had these explanations." But Camper continued to deny the very thing about which Goethe cared most: he still asserted the intermaxillary bone does not exist in man. From Sömmering, Goethe received, as he wrote to Merck, "a very trifling letter. He even wants to talk me out of it. Humph!"

With such opposition on part of the specialists, Goethe lost all desire to publish the treatise. Loder announced the discovery to the scientific world in 1788 in his "Anatomisches Handbuch." Sömmering and Blumenbach gradually became converted, but it was almost forty years before Goethe's discovery attained full recognition. He himself did not publish the little work till 1820, 26 years after the discovery was made when it appeared with important additions in one of the numbers of his periodical *Zur Naturwissenschaft,* and it was not until a year before his death (1831) that he experienced the joy of seeing it reprinted, together with the drawings, in the *Verhandlungen der Kaiserlich Leopoldinisch-Karolinischen Akademie der Naturforscher,* Halle.

Goethe was, however, not disconcerted; he knew beforehand that he was on the right path, or as Herder put it, on the true path of nature, and that from now on he would lose nothing. His scientific activity broadened from day to day, but the vegetable kingdom especially engrossed his attention.

Immediately upon his arrival in Weimar his interest was aroused in the plant world, partly because his official duties turned his attention in that direction. In nature's open work shop, in meadow and field, in forest and game preserve, he began his studies, which found rich nourishment in the laying out of gardens for the Duke and in the desire to beautify his own garden out of his own resources. Even as early as 1788, we find him occupied with observations on mosses; not until later did he turn to books, for it was not in his nature to learn anything from them, and it was only after he had looked about him for a long time in nature and had discovered some of the secrets of her workings that he knew how to use books. From 1785 on he was wholly absorbed in the plant world, and "in botany he had soon made very admirable discoveries and combinations which corrected many errors and threw light on many points." But he was not seeking to find out isolated facts; it was his aim here as everywhere to discover a general, fundamental law to which individual phenomena can be reduced. Upon this was centered the "productive passion" which he had conceived for the natural sciences.

The anticipation of his discovery of plant metamorphosis, which at that time hovered before his mind under the conception of a hypothetical *Urpflanze* (primitive or primordial plant), accompanied him across the Alps. In Italy, so rich in forms, he saw, side by side, beneath the open sky, a fullness and variety of throbbing life such as was hardly to be found, scattered, in the small hot houses of his northern home; he found here everything more unfolded and further developed, and many things, which he had previously only surmised and had sought with the microscope, he here saw with his naked eye as an indubitable certainty. In Palermo he went to the public gardens to think over the plot of *Nausikaa* more fully, but the thoughts which the wealth of plants suggested to his mind disturbed his poetic plan: "The garden of Alcinous had vanished and a world garden appeared before me." He had seen and reflected enough in the world garden; he was now able to pluck the ripened fruit. It did not fall into his hands without some effort on his part; in fact, in later years he insisted that this was truth about all his works. Of this particular fruit of labor he said: "What a long chain of observations and reflections I had to carry out before the idea of plant metamorphosis dawned upon me!"

In this "epic of the coming into being of higher plants," as Alfred Kirchhoff aptly calls the little treatise which appeared in 1790 under the title *Versuch, die Metamorphose der Pflanzen zu erklären,* Goethe revealed to the scientific world the idea of creative power in continuance of operation, not ceasing at apparently complete forms and then withdrawing from activity. He sought in this way to reduce "the manifold specific phenomena of the glorious world garden to a simple, general principle," and it may be said that the poet was the first man to raise botany, and at the same time zoölogy, to the rank of real sciences. Hitherto these branches of knowledge had consisted solely in empirical descriptions, in collecting and arranging, and in distinguishing and separating. To draw an illustration from botany, prior to Goethe the plant in its totality, and each organ of it, was considered only as a complete object, a finished thing distinguished from all other things. Now Goethe had studied comparative anatomy and comparative osteology, under the best masters of his day and in this way had the good fortune to make many splendid discoveries; what could have been more logical than that as soon as he turned to this field, he should study comparative botany—that he should observe the relations of different plants to one another, and those existing between the organs of a single plant? Hence it was necessary for him to watch the plant in its germination and growth, in "its development out of the seed and all the way to the formation of new seed," and with the eye of a genius he recognized that cotyledon, stem, anther, leaf, sepal, petal, filament, stamen, in short, all appendages, or lateral organs, of the plant axis are only transformed or metamorphosed leaves—[1] in other words, all formations in- and on the *higher* plants, for it is only with such that Goethe's doctrine of metamorphosis deals may be reduced to a primordial organ, which he calls "leaf" (Blatt). Accustomed to view every manifestation of nature in its relation to her other phenomena, in the conviction that only in this way is it possible to entice from her, her secrets, he directed his attention to formations deviating from the normal to certain monstrosities, as, for example, double flowers, in which "instead of stamens (filaments) and anthers, petals are developed,"—that is to say, a petal is formed where under ordinary circumstances a stamen appears—and from these facts he deduced the inward relationship of these organs, their similar origin, and their predisposition to assume the same form. Such phenomena of abnormal or retrogressive metamorphosis aided him in his investigation of the normal course of plant development.

It is worthy of note in this connection that Goethe did not see in the leaf as the fundamental organ the final, simple element to which the plant form may be reduced. He chose this designation for lack of a better. Modern science employs the term "leaf-organ." In order to have gone back to the beginnings of plant growth he would have had to possess a knowledge of the biologic significance of the elementary unit of the organism, the cell. But that Goethe's genius had divined the truth clearly and with surprising accuracy is apparent from his words: "Every living thing is a multiple, not a single being; even in so far as it seems to us an individual it remains nevertheless an aggregation of independent living beings, which in idea or fundamental plan are homogeneous, although in appearance they may be homogeneous (similar) or heterogeneous (dissimilar). They separate and then enter into new unions, thus securing an endless production in every way and in every direction."

After he had gone to Sicily and returned to Rome it was no longer a surmise, it had become with him a "Columbus' egg"; he had not only found the clew, he had the "master key," and was in a position to declare "that the great works of art (speaking of the remains of classical art) are at the same time the highest works of nature, produced by man in accordance with true and natural laws; everything capricious and imaginary falls to ground; here is necessity, here is God." He was able to look into the depths of art with all the greater joy as he had accustomed his sight to the depths of nature.

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[1] I suppose Goethe considered filament and anther separately which would eliminate stamen as such.

The word *Morphology* was coined and introduced into the nomenclature of science by Goethe.

Called soon afterward to the seat of war in Silesia, during his sojourn in Breslau, he devoted himself chiefly to comparative anatomy. On the thirty-first of August, 1790, he wrote Friedr. v. Stein, "In the midst of all this turmoil, I have begun to write my treatise on animals."

Passing from the wealth of material in Goethe's *Morphologie,* we must mention here one more discovery, the so-called "vertebrate" theory of the skull. As a result of his faithful and diligent study of vegetable metamorphosis, Goethe says the year 1790 had in store for him a new view concerning the animal organization which pleased and satisfied him. It was an idea analogous to the metamorphosis of plants, that in the higher animals the skulls are composed of modified segments of the vertebral column. He had recognized the vertebral form of the occipital bones earlier, but it was not until 1790, during his sojourn in Venice, that, as a result of a happy accident, he thought he perceived that some of the bones of the face are likewise modified vertebræ. In spite of the fact that the latter reference has proved to be at least partly erroneous, and that Goethe did not go more deeply into the question of the vertebral character of the occipital bone, a theory which is accepted as a fact, nevertheless the idea itself has been extraordinarily fruitful in its influence on the investigation of the bones of the head.

Goethe's earliest scientific activity was in the field of mineralogy and geology. Soon after his arrival in Weimar he prepared himself, on his wanderings through Thuringia, while "living in chasms, caves, and forests, in ponds and under waterfalls, with the subterrestrials," for serious scientific work, to which he added a practical interest when the plan arose of improving the old Ilmenau mines, and he was officially entrusted with the undertaking, to which he devoted much faithful effort. Mineralogy was for him, however, but an auxiliary science to geology, which he called the "skeleton of the earth." The investigation of the earth's crust in the region of Karlsbad and Bohemia was, up to the end of his life, very dear to his heart. In general he always held the view which he had early formed that granite is the solid foundation of the earth, as he asserts in his highly poetic essay *Über den Granit.*

What broad and unobstructed views Goethe revealed in geology are shown by his prophecy of the significance that geology would some day attach to fossils, which were then just beginning to be studied. He wrote to Merck, October 27, 1782: "All the remains of bones of which you speak, and which are found everywhere in the upper sand of the earth, are, I am fully convinced, from the most recent age, which, however, in comparison with our usual method of reckoning time, is exceedingly old. In that age the sea had already receded, but the rivers were still very broad. . . . At that time, elephants and rhinoceroses were at home with us on the exposed mountains and hills, and their remains could very easily be washed down by forest streams into those great river valleys or sea levels where, more or less impregnated with stony matter, they were preserved, and where we now turn them out with the plow or bring them to light in some other accidental way. . . . The time will come soon when fossils will no longer be a mass of confusion, but will be arranged to correspond in general to the ages of the world."

These are truly prophetic words, which have found their complete fulfilment in science. Petrifactions afford geologists the best means of distinguishing and determining rock strata and of systematizing the geological ages.

In *Meteorology,* Goethe was not so felicitous as in his ideas and works on the three kingdoms of nature. His interest in this science, which was at that time still in the rudimentary stage, was profound and was probably affected by his sensitiveness to the changes in the condition of the atmosphere. He suffered to an unusual degree under the inclemencies of the weather and he belonged finally, to "the few men who have an immediate feeling of the state of the barometer." He provided himself with barometer and thermometer and evidently began early the study of comparative meteorology. He wrote from Rome requesting that the record of the weather in Weimar during his absence be copied for him from the "Weather Observation Museum" of Dr. Siewar in Upper Weimar. But, as he says himself, it was impossible for his nature to grasp the whole complex meteorological data as they are represented in tables by means of figures and signs. Only after he had become acquainted with Howard's scientific nomenclature for the cloud formation which had interested him, did he feel that he had a fixed point of departure and he gladly grasped the offered thread that led him out of the labyrinth. He now compared the cloud-forms with the readings of the thermometer and from the latter was able to guess the former. To the terminology of Howard, which has been retained up to the present time, Goethe added a new designation, which he calls "paries"—wall, which was adopted by Kämtz in his *Lehrb. d. Meteorologie* (1831), but has not found its way into the more recent textbooks on the subject. It was entirely out of the question to accept with approval the hypothesis which Goethe set up in explanation of the variations of the atmospheric pressure, upon which, as we know, meteorological conditions essentially depend. He assumed that the gravitation of the earth is not constant, but changeable and pulsating, as a result of which the attraction on the atmosphere, and hence the pressure of the latter, increases at times and at times diminishes. This hypothesis, which Goethe first published in his *Italienische Reise* in 1816, and then often repeated in his meteorological essays from 1820 on, cannot well be made to harmonize with our physical conceptions.

Of Goethe's *theory of colors* it must be said that it was a life work with him in the highest sense. His writings on this subject fill more pages than what he wrote on all other scientific subjects taken together. No one of the products of his genius has he enveloped with warmer love and, if we are rightly informed, he valued this work far higher than his poetic writings. To no work did he apply himself with greater pains and in none did he show greater perseverance. After his *Beiträge zur Optik, Erstes Stück & Zweites Stück* had appeared in 1791 and 1792, respectively, it took no less than eighteen years of untiring, painstaking application, during part of which time he enjoyed the most devoted interest and encouragement of Schiller, the "unreplaceable," before his chief work, the two-volume treatise, was finally finished and in print. Even to his last years he followed every new phenomenon with the energy and freshness of youth and sought to bring it into harmony with his earlier work. See Helmholtz Vorhäge B. I s. 23 "Ueber Goethe's Naturwissenschaftliche Arbeiten."

In spite of the error contained in it, this work has created a new department of culture, not alone for the author himself, but for the scientific and artistic world as well. The opposition it met with was not because of the experiments recorded in it, which were never questioned as to their correctness and are unparalleled in their variety, but because of the physical interpretation of them. The error in the work has not retarded science; the truth in it has not only advanced science, it has even become the foundation of a new science, that of physiological optics, of which Goethe must be looked upon as the originator. He has opened our minds to a sphere of human observations hitherto but little considered. Scientists before him excepting Leonardo Da Vinci had hardly attempted to discover the laws of visual processes in their relation to light and color. Goethe was the first man to reduce to a scientific formula the phenomena of colorless and colored after-images, of successive and simultaneous contrast. The description of these delicate phenomena, their origin and gradual subsidence,—for which he coined the suggestive expression *Abklingen*[2]—the

[2] Abklingen—a gradual dying away of sound he applies in analogy to fading away of color. See Mangus, "Goethe als Natur—forscher."

theory of colored shadows, about which he wrote a separate treatise, and many other details which throw a great deal of light on visual phenomena, form the first part of the *Didaktischer Teil* of the work, to which he gave the subtitle *Physiologische Farben.*

The fundamental idea of this part of the work is that it is the nature of the eye to demand brightness when darkness is offered it, and to demand darkness when confronted by light. Likewise when a color is offered it, it demands the opposite color. For example, yellow demands violet, orange demands blue, purple demands green, and *vice versa.* These "demanded" colors are a product of the eye and belong to it entirely; there is nothing like or corresponding to them in the outer world. The discovery of this law of visual processes has made Goethe's name one of the most prominent in connection with the latest development of the physiology of colors, which is more and more taking the place of the Hering and the Young—Helmholtz theory. The new theory is based on the law of antagonistic colors, according to which there are four fundamental color sensations, which go together in pairs: yellow and blue, red and green. In addition to these, there is a black-white sensation, as Goethe had also maintained. To be sure, the colors are here and there differently designated, as a natural consequence of a certain difference of conception, but in essence Goethe's theory and the new one are the same. His classification included a third group, the chemical colors.

GOETHE'S SCHEME OF A HISTORY OF SCIENCE

Goethe had planned to treat the historical part of the *Farbenlehre* as symbolic of the history of all science, and although he finally gave it the modest title of "Materials for the History of the Theory of Colors," his contemporaries and succeeding generations have declared with delight and enthusiasm, that he did full justice to the exalted task which he set for himself. A light-bearer, Goethe leads us through thousands of years and lets us listen to the conversations which a sovereign genius holds with the great men of the long past and even with the inspirations from the maker of all things. He usually shows us the personalities on the historical background of their times, in order to give us a clear understanding of them. How felicitous the master is in conjuring up before our mind's eye with a few strokes, a picture of the intellectual nature of a Plato and an Aristotle! With what deep, wisdom-laden observations on the philosophy of history he fills up the "gaps!" And who has ever said truer or more beautiful things about the Bible than Goethe in his history of the theory of colors? "The spirit of true, deep humanity reigns throughout the work," wrote Knebel (Aug. 10, 1810). Everything in it is there, because of its substance; there is nothing in it for the sake of appearance, or effect. And thus in the end it leaves upon us the impression of reconciliation with the shades of Newton.

WAS GOETHE A FORERUNNER OF DARWIN?

The intellectual relationship between Darwin and Goethe may be stated in a general way to exist in their presentation of the doctrine of development (using development in the sense I have previously defined). The idea of evolution and development is really one of the oldest products of human thinking. We find unmistakable evidences of it in Heraclitus (513 B. C.) and the ideas of Empedocles (460 B. C.) have already been brought out in the preceding; but the thought obtained in all ancient systems of philosophy, particularly in that of Aristotle, and through him, also in the systems of the Middle Ages. This must be evident to the student of Thomas Aquinas and Dante.

We find it again in a more elaborate form in the systems of the Renaissance, the best known of which is that of Giordano Bruno. Then came the time of the more modern clarification, and here again we find the idea of development and evolution in the writings of Leibnitz. In his philosophy of the history of the human race, Herder has brought his ideas of nature, earth, and man, in the light of this conception of evolution. Frau von Stein wrote to Knebel in 1777, "Herder's new publication makes it probable that once upon a time we were plants and animals. What nature may further make out of us in future, will probably be unknown. Goethe is meditating deeply on these things."

In his essay on "Goethe's Naturwissenschaftliche Arbeiten", H. von Helmholtz attempts to solve the intricate problem of Goethe's versatility and raises the deep question of how the poetic talent could have brought a consciousness of power to become master also of nature (published in Helmholtz's *"Vorträge und Reden,"* Bd. I., p. 25 and Bd. II, p. 337), for with the consciousness to possess such power must inevitably come the endeavor to make use of it.

In this connection I wish to ask you to consider the following opinion of Johannes Müller ("Handbuch der Physiol." II. p. 567). At one place this great physiologist speaks of the influence of the idea on fantasy and in attempting a definition of the psychology of fantastic phenomena of vision he teaches that our fantasy works only within the limits of the form that were given to it by a preceding idea, meaning, no doubt, that there is no such thing as thought imagery "De Novo," i. e., starting by its own incentive, or expanding without definite boundary. Johannes Müller thinks that fantasy is always defined by some preceding conception and then he continues, "Whoever wishes to have a clear conception of this should read Goethe's masterful description of the rodent and its social, as well as morphologic relations to other animals. Nothing similar can be produced that could equal this projection, taken out of the central point of living organization. If I am not in error, this presentation contains the premonition of a distant ideal of natural history. Thus you may see the vertebræ develop into the skull, the leaf-organ evolve into the petal or stamen, the respiratory organs evolve as lungs or gills under the most manifold forms." And further on, Müller continues, "In the artist, as well as in the comparative naturalist, fantasy-life moves only within the sphere of concept. The naturalist pronounces the law of form development. He sees it only in the objective or realized in nature. The fantasy of the artist is also active in this same law, but it departs from that which is reality and objective and elevates itself above that which is real to that which is ideal. Now this is purposeful to itself and no longer an expression of inner functions, but yet it is limited by these inner functions. Let us not marvel, therefore, when one and the same man has achieved the greatest things in both directions. It was only through an imagination, so plastic, that it recognized all changes in living matter as conformative to a central idea, that Goethe discovered the metamorphosis of plants and upon this imagination are founded also his advances in comparative anatomy and his highly intellectual, even artistic conception of this science."

The language of the German physiologist which I have here translated, is perhaps not as readily comprehensible as that of John Tyndall, when he speaks of the scientific use of the imagination in his "Fragments of Science," but the central thought in the doctrines of both men are identical, namely, fantasy and imagination are not mental operations devoid of foundation and definite boundaries; they are started by precise conceptions and always operative within the boundaries of those conceptions.

Lavater's judgment on Goethe might be compared with these opinions of Müller in his "Sämmtliche Werke" (Bd. 22, p. 340). He says, "Your unalterable endeavor is to give a poetic form to reality. Others attempt to make real, that which is poetic and imaginative. This leads to nothing but nonsense (Dummes Zeug)".

Helmholtz distinctly asserts (l. c. p. 46) that Darwin's theory of the evolution of organic forms is preferably founded on the same homologies and analogies in the structure of animals and plants which the poet Goethe had attempted to represent to his incredulous contemporaries as "The First Discovered." Yet Helmholtz emphasizes that these analogies and homologies were at first only in the form of divining intuitions. Thus he assigns to Goethe the credit of the first discovery; to Darwin he assigns the merit of having traced the causative connection of these homologies and analogies in the types of the most manifold organisms and thus having brought the poetic premonition to the maturity of a precise concept.

Is this really true? Was Goethe the first discoverer of the theory of evolution as modern naturalists understand it? Was he the forerunner of Darwin? As his method of thinking was principally of the kind that we define as prophetic and poetic, it is difficult to describe in a meager definition, what was really the essential characteristic of Goethe's idea of evolution.

I will premise what I shall have to say by stating that I understand the evolution of Darwin and the evolution of Goethe to be two different doctrines and that Goethe is not a forerunner of Darwin. Indeed a thorough study of the thought direction, intellectual types and manner of observation of nature of these two men must recognize in them typical representatives of two opposing aspects of nature (See Chap. xxx and xxxi).

What Goethe means by his formation and transformation (Gestalten u. Umgestalten) is the *type* according to which the individual organisms are shaped. He means the architectural plan which underlies their development. He does not mean the transformation of the *living things themselves.* He never speaks of the real transformation of a lower genus of animal into a higher one. This is transformism in reality. It is a conception upon which everything depends, according to Darwin. This is an idea which was unknown to the old evolutionists.

In rare instances, it could combine itself with the evolutional thought to give its realization some external form. In his later years, Goethe incidentally touches upon such a hypothesis and we find traces of it in Kant's writings. But Darwin's idea of evolution stands in no necessary relation with the true inwardness of Goethe's idea. The particular and proper discovery of Darwin was not the doctrine of evolution, or even of transformism, for he found these already existing and makes no endeavor to introduce them into biology. The doctrine which is peculiar and proper to him is that of *natural selection through the struggle for existence* and *this doctrine constitutes in itself, almost the entire work of Darwin.* But this principle is absolutely strange to Goethe and diagonally opposed to his method of thinking. One can see this most clearly in the fact that the most genuine followers of Goethe, namely Schelling, Hegel, and others, who adhere strictly to the old evolutional thoughts, reject the doctrine of transformation as a hypothesis all too crude and nebulous.

The history of architecture shows—to cite a parallel analogy—that the Gothic dome was evolved from the old Basilica of the Roman church, and thus in the working of nature, the old evolutionists conceived that one type of its creatures was evolved from another type. But it was not a transformation. A Basilica remained a thousand or more years, always a Basilica. When the Gothic dome came into existence, it represented simply an objective realization of a new thought. The old Basilicas still existed and were still built when the Gothic domes were constructed.

Just as little as a Gothic dome was ever actually grown out of a Roman Cross Church, so little did the old evolutionists conceive that a bird could have gradually been transformed out of a reptile. Goethe would have conceived the effort to test the workings of nature by actual experiment as profane and arrogant. He speaks in this tone about the physicists who attempt to force from nature by levers and screws, that which she does not wish to reveal.

> Geheimnissvoll am lichten Tag
> Lässt sich Natur des Schleiers nicht berauben,
> Und was sie deinemGeist nicht offenbaren mag,
> Das zwingst du ihr nicht ab mit Hebeln und mit Schrauben.

It would not have suited Goethe's high flying aspects of nature that natural selection should dominate "Where at the rushing loom of time the living dress of the Deity is woven" (Am sausenden Webstuhl der Zeit, da der Gottheit lebendiges Kleid gewirkt wird).

Half of our difficulties in modern conceptions are due to errors of interpretation and errors in the use of words. We are frequently deceived when we think over the old and new evolutional ideas, by the fact that modern transformism also calls itself "Doctrine of Evolution." But if we ponder over the meaning of these words, we will become convinced that they are antagonistic. One can only speak of development and evolution if the higher thing is already contained potentially in the lower, for instance, as the blossom or flower is contained in the bud; so that the lower thing need only to be unfolded to become the higher thing. This is what the old evolutional idea meant. But this is diagonally opposed to transformism in the modern sense, which does not mean unfolding, but direct addition.

Evolution in Darwin's sense tends to demonstrate how new and different conditions, properties, and circumstances were added to the first and original properties of the lower forms of life, so that by natural selection, new and other properties developed. This contradicts the fundamental thought of Goethe, according to whom there is no such addition, particularly none that could come from trivial external circumstances.

According to Goethe, nature forms only from within by unfolding that which is always one and the same; by simple potentialization, never by addition of new qualities.[3]

According to Darwin, the living thing is an accidental aggregate, subject to continual slow changes by processes of adaptation to the changing environment, and by natural selection of those new qualities that are most conducive to perpetuation of the species.

Indeed, the signification *development* in the strict orthologic sense, is a misnomer when applied to Darwinism. It is known that in modern times some leading biologists have turned away more and more from the one-sided doctrine of natural selection and they assume hard and fast fundamental directions, along which the upbuilding of living things is assumed to have moved, and which are definitely prearranged with the aboriginal dispositions and factors "Anlagen" given to the first living thing.

It must be evident that such viewpoints much more readily fit into the old ideas of development. They are rather favorable to the contemplation of things under the form of "ideas." Here indeed we would be dealing with disposition and unfolding of original plans and therefore with the occurrence of things from within, outward. If the representatives of this trend of biological thought still call themselves Darwinians, they simply indicate how far they have deviated from the principle of this great biologist in their ideas of *orthogenesis*.

The great difference between Goethe and Darwin is clearly depicted in an experience which Virchow relates in his "Goethe als Naturforscher" (p. 43, 1861). In 1794 Goethe had come to Jena and listened to the lectures of Batsch, the professor of botany. On leaving one of these lectures he accidently found himself at the side of Schiller, who found fault with the fragmentary and disrupted manner in which the lecturer had treated a subject of nature. Goethe replied that there was indeed a manner in which nature could be observed and represented, not separately each element of her various purposes, but in her totality, representing her acting and living, tending from totality into the parts. The conversation became more vivacious and Goethe entered Schiller's house in order to explain to him the metamorphosis of the plants. He sketched plastic schemes in words and it happened that Schiller, the philosopher and poet, who had once won a prize in anatomy, understood him better than the scholars and specialists of his day. But presently, the delightful concord between the two was threatened by an exclamation of Schiller's: "Das ist keine Erfahrung, das ist eine Idee." It was quite natural for Schiller, who was a "Kantianer", to distinguish precisely between experience and ideas, but to Goethe, who had confessed that he had no faculty of comprehension for philosophy in its proper sense, this remark was quite a shock. But the strain passed away and the two great poets separated as friends. The distinction was emphasized by Schiller in this impulsive remark, namely, concepts may come from experience and they may also come from ideas that are not a result of experience. Now the kind of experience from which a man like Darwin forms his concepts is objective experience. Darwin is a man of the most sober and exact scientific methods, who will have nothing to do with ideas that are born of moods and temperaments, who takes the sharply defined conception of natural law for a starting point and attacks nature by single experiment and single observation in uncompromising manner. He belongs to the school of clarification that has been associated in name and manner of thinking with Newton.

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[3] In his Metamorphosis of Plants he does, however, touch upon external causes (air, light, temperature) as modifying growth.

From these, however, Goethe deviates sternly, and frequently gives evidence of passion, and short-sightedness toward his superior opponents. In his doctrine of colors, Goethe undertook to battle with the great mathematical physicist, Newton, and his school. That there is much truth in Goethe's theories concerning color vision has only been recently understood, but the cause of his severe attacks on Newton is an inner, and deeply personal one. He desired to fight against what he terms the devitalizing, exact, and objective study and conception of nature which he feared would rob her of the halo in which he conceived his goddess to be draped.

It is a fight between the nature mystic and the objective scholar of nature. The poet conceives himself as the pious one, defending nature against profanation and therefore he hates all undertakings to force nature to reveal her secrets by apparatus, by refined methods, "by levers and screws."

We can hardly conceive therefore, of two more diagonally opposite types of intellectuality than Goethe and Darwin.

Very frequently a theory has its chief merit in its collapse, for there is no more powerful stimulation to new and more exact research than the collapse of a hitherto cherished theory. We see this in the history of the theories of Stahl, which I have considered in the article on Lavoisier, Chap. xxi, and I believe such a fate is in store for the "side chain" theory of Ehrlich. We have seen that the ideas of Goethe and Darwin concerning the gradual evolvement of higher and more perfect living things are not identical; that Goethe confused *idea* and *experience* in the trend of his thinking. Goethe is not a forerunner of Darwin in the strictest sense, for Darwin is the type of an exact investigator who is entirely free of preconceived ideas. He is not actuated by ethical, aesthetic, poetic, pious, or other ideal motives, but simply the instrument of objective investigation who seeks for biology a purely empirical understanding, and does not tend towards an ideal interpretation. His exact scientific researches, far from being profane, are actually the duty, the sole duty of the thinking intellect. The fine, but far-reaching distinction between idea and experience was emphasized by Schiller, the clear-seeing pupil of Kant, as has been narrated before, when Goethe on one occasion propounded his opinions on natural philosophy (see Page 593).

HIS CONCEPTIONS OF NATURE

The sympathetic student of the life and works of Goethe, who is genuinely in love with his subject, can find many statements of this superlative human being that indicate unmistakably, that Darwin's theory concerning the transformations of the organic forms is based principally upon the same analogies and homologies in the architecture of animals and plants which the German poet first discovered.

Of course we may have to admit that the poet's ideas were only anticipating intuitions. It is Darwin's merit to have traced up the causal connection with great acumen and persistent observation; I mean the causal connection between these animal and plant analogies and homologies which are still evident in types of the most varied organisms. In this way, the poetic presentiment of Goethe was developed to the maturity of a clear and precise conception.

Natural sciences have come nearer and nearer to the ideas and ways which Goethe had formed for himself concerning the direction of the investigations of nature. What Goethe sought was the causality of the phenomena of life; that was the main thing for him and even as a poet he always refused to permit an understanding of the phenomena of life to be confused by metaphysical speculation.

On October 18th, 1827, Hegel was the guest of Goethe and the conversation at tea revolved about the character and merits of dialectics. The philosopher had remarked, in response to some contentions of the poet, that the art of dialectics if solely in the hands of the morbid minded, would be improperly used in attempting to make the false seem true and the true seem false. Hereupon, Goethe replied (Eckermann III, p. 222), "Well, I shall praise the study of nature, which will not permit such a disease to arise, for in it we have to do something that is infinite, which is eternally true and which will repudiate everyone who does not proceed in a pure minded and honest way with the contemplation and treatment of his object and I am certain, also, that every person dialectically afflicted could find a benign cure in the study of nature."

In the same work by Eckermann, "Gespräche mit Goethe," he expresses himself as follows: "If men only would not upset and obscure what is right after they have found it, I should be satisfied. Something of a very positive character is needed for humanity, so that it could be transmitted from generation to generation and certainly it would be a good thing if this positive something were also right. With regard to this, I would rejoice if the natural sciences came into a state of clearness and could stay in that which is right and not again proceed from it, but human beings cannot stay at rest and before one knows it, confusion is uppermost again."

Concerning his method, he has the following to say: "I possessed a developing and unfolding method; not one that brings together and coördinates; with phenomena and facts arranged along side of one another, I could do little, but with their relations to each other, I was more successful" ("Sämmtliche Werke," Bd. 27, p. 495.) And he continus: "I hold firmly to the thought that one should first investigate the determination of every part for itself and its relation to the whole, to acknowledge at the same time the right of every individual and single part and its influence on the remainder and finally, in that manner, the necessary, useful, and utilitarian features in living things would become obvious." ("Sämmtliche Werke," Bd. 36, p. 254.)

The clinician, Heinroth, designated Goethe's method of thinking as one that was objective, and in relation to art, Goethe himself has clearly expressed this, in a letter from Rome, when he said, "I must progress so far in art that everything shall become objective knowledge, not tradition and name."

In Goethe's time, many leading thinkers were dominated by a sensitive rationalism which naturally digressed widely from the objective scholarship of the poet. Let us reflect, for instance, upon the following remarkable passage which is taken from the work of Eckermann (Vol. 2, p. 176): "It is natural to the human creature to look upon itself as the aim of creation and upon other things, only in so far as they serve and are useful to it. The human animal takes control of the vegetable and animal world and whilst he devours other creatures as suitable food, he recognizes his God and praises his benevolence for providing for him so paternally. He takes the milk from the cow, the honey from the bee, the wool from the sheep and in giving these things a useful purpose for himself, he also believes that they were created for that very object. Yes, he can hardly conceive that the smallest weed is not here for his sake; although he may not have recognized its usefulness for the present, still he believes that this may yet in future be discovered and just as the human being thinks in general, so he thinks also in particular, and he does not omit to transfer his habitual aspects of life into science and to inquire of the individual parts of an organic being what their object and usefulness may be—in this method he might run along for awhile in science, but very soon he will hit upon phenomena where such puny views are unsatisfactory and where he will become involved in a maze of contradictions unless there is some higher guiding principle. Such utilitarian doctrines may well say, the ox has horns in order to defend himself, but I inquire, why has the sheep none, and when it has horns, why are they wrapped around his ears so that they are of no use to him? It is an entirely different aspect, however, when I say, the ox defends himself with his horns because he has them.

"The quest after the purpose,—the question 'why,' is by no means scientific. We make more progress with the question 'how,' for when I ask, how did the ox come to have horns, this leads me to a contemplation of his organization, and to compare him with other creatures and then instructs me at the same time, why the lion has no horns and can have none.

"The teachers of the doctrine of utility would believe that they must lose their God if they could not pray to Him who gave horns to the ox, in order that he could defend himself. It might be permitted for me to adore Him, who in the wealth of His creation was so great, that after thousandfold forms of plants, He made yet one in which all others were contained and after thousandfold forms of animals, He made yet a creature that contains them all, the human animal.

"One may reverence Him who gives food to the animal and to man, food and drink, as much as he may partake of, but I pray to Him who has laid such a productive force into the world, that even when only the millionth part enters into life, the world swarms with creatures so that even the great catastrophies,—war, pestilence, deluge, and fire—cannot destroy this world. THIS IS MY GOD."

According to Virchow, Goethe believed that man was but an intervening form to a still higher creature to come on earth.

CONCERNING THE PRIORITY OF THE DISCOVERY OF THE VERTEBRATE THEORY OF THE SKULL

Goethe was an artist at drawing, a most remarkable talent in a poet and philosopher. In the Goethe House in Weimar where one can find an enormous collection of minerals—18,000 in number, some pieces of rare beauty and perfection—are also to be seen, skeletons and skulls on which Goethe made his comparative anatomical studies. Here are preserved numerous forms of apparatus, physical and electrical, with which he experimented. In astonishing abundance, are here presented all of the things he used for his optical experiments and studies, such as colored papers, silks, pieces of flint glass, mirrors, prisms, and polarization apparatus. They are still in the same papers in which Goethe wrapped them. The whole magnificence of his studies could be here reproduced by a Goethe worshiper, with the original apparatus of his demigod. The library is extremely rich in works on natural sciences; but to me the most remarkable things to be seen there are his drawings and graphic representations which furnished the material for illustration of his works on natural science. Tables with anatomical, botanical, and geological representations are there, still unpublished. He furnished most important drawings to Lavater's work on Physiognomy (Sämmtliche Werke, Vol. 25, p. 195. Comp. Vol. 22 p. 372. Eckermann. II p. 70.).

He was Lavater's collaborator and his studies of animal skulls laid the foundation of his theory that the bony capsule which encloses the brain is built up according to the same fundamental type as the bony tube which surrounds the vertebral column, so that the osseous capsule surrounding the brain is a higher evolvement of the vertebral column. This statement is applicable, not for all parts of the skull, as we include them in the ordinary sense of the word, but only for those parts which are the essential enclosures of the brain. To these skull bones, in the more restricted sense, the facial bones are added as a further equipment and therefore they stimulate the assumption that they represent parts that can be compared to the ribs, or to the extremities. Just as the rib is not a part of the vertebræ, but simply an extension, so the facial bones are not integral parts of the cephalic modification of the vertebræ.

Just how far this comparison can be extended, it is difficult to say, as the anatomists are not agreed as to how many of the facial bones might be added to the cephalic-vertebral column as essential parts.

In 1790, Goethe had already recognized that the brain capsule proper consists of three modified vertebræ and in accidentally finding the skull of a ram in the Jewish cemetery of Venice, he conceived the idea that the facial bones might be interpreted as consisting of three further modified vertebræ.

It is astonishing to note the technical masterfulness with which he speaks and describes the bones of the base of the skull and of the face.

He speaks of sphenoid and ethmoid as if they presented to him no difficulties whatever ("Sämmtliche Werke." Bd. 40, p. 447).

When we endeavor to sink ourselves in Goethe's method of thinking concerning the spinal column, and how the vertebræ became metamorphosed into the bones of the skull, we enter upon a historic discussion. This sometimes culminated in fierce polemics and controversies in which some of the most brilliant anatomists and naturalists of his day participated. But the discussions on this subject have even continued to our present time; thus we meet the name of John Peter Frank, who, it has been claimed by some, was the original discoverer of the vertebrate theory of the skull; no less a man than the great German physiologist, Johannes Müller, asserted that the clinician first projected the idea of a similarity of these parts (Joh. Müller: "Vergleichende Anatomie der Myxinoiden. Berlin 1835, p. 121. "Gedächtnissrede auf Rudolphi." Berlin, 1837, p. 15).

When we look at the article by Frank closely (De curandis hominum morbis Epitome. Mannheim, 1792, Lib. II, p. 42.) we find two places that might suggest that he had expressed an idea relating to this anatomic problem. On page 42, Frank states simply that the skull cavity is similar to the spinal canal, but in a second place Frank pronounces the skull to be the last and highest vertebra and also that every vertebra could in itself be regarded as a little skull. This at least might be interpreted as pointing in the right direction but it is not the vertebral theory of the skull as Goethe and Oken developed it, for in this theory, we are not dealing with the circumstance that the skull is a vertebra, or that every vertebra might be looked upon as a small skull, but rather that the skull in its composite structure is traced back to a series of modified vertebræ which are to be determined by accurate comparison.

As a matter of fact, Goethe's priority is not touched by Frank's statement, which was published in 1792, for Goethe's expressed opinions antedate this by two years (J. P. Frank. "De vertebralis columnae in morbis dignitate oratio academica. Delectus opusculorum medicorum antehac in Germaniae diversis acedemiis editorum." Ticini, 1792, Vol. XI, p. 8.).

The great importance that anatomists attribute to this problem is seen in the publication of Pouchet's ("Histoire des sciences natur. au moyen âge ou Albert le Grand et son époque." Paris, 1853, p. 269 sq.) in which he claims that Albert the Great (Albertus Magnus) had been familiar with the vertebrate theory of the skull. There is just as little possibility that this learned Dominican monk had any idea of this theory as that Empedocles had any correct idea of the theory of evolution (see Goethe as a Forerunner of Darwin, p. 589). Virchow in his article on "Goethe als Naturforscher," p. 120, goes into the writings of Albertus Magnus with some detail and concludes that the claim has no foundation whatever. Albertus was a Suabian Count and Chief Provincial of the German Order of Dominicans and teacher of Thomas Aquinas.

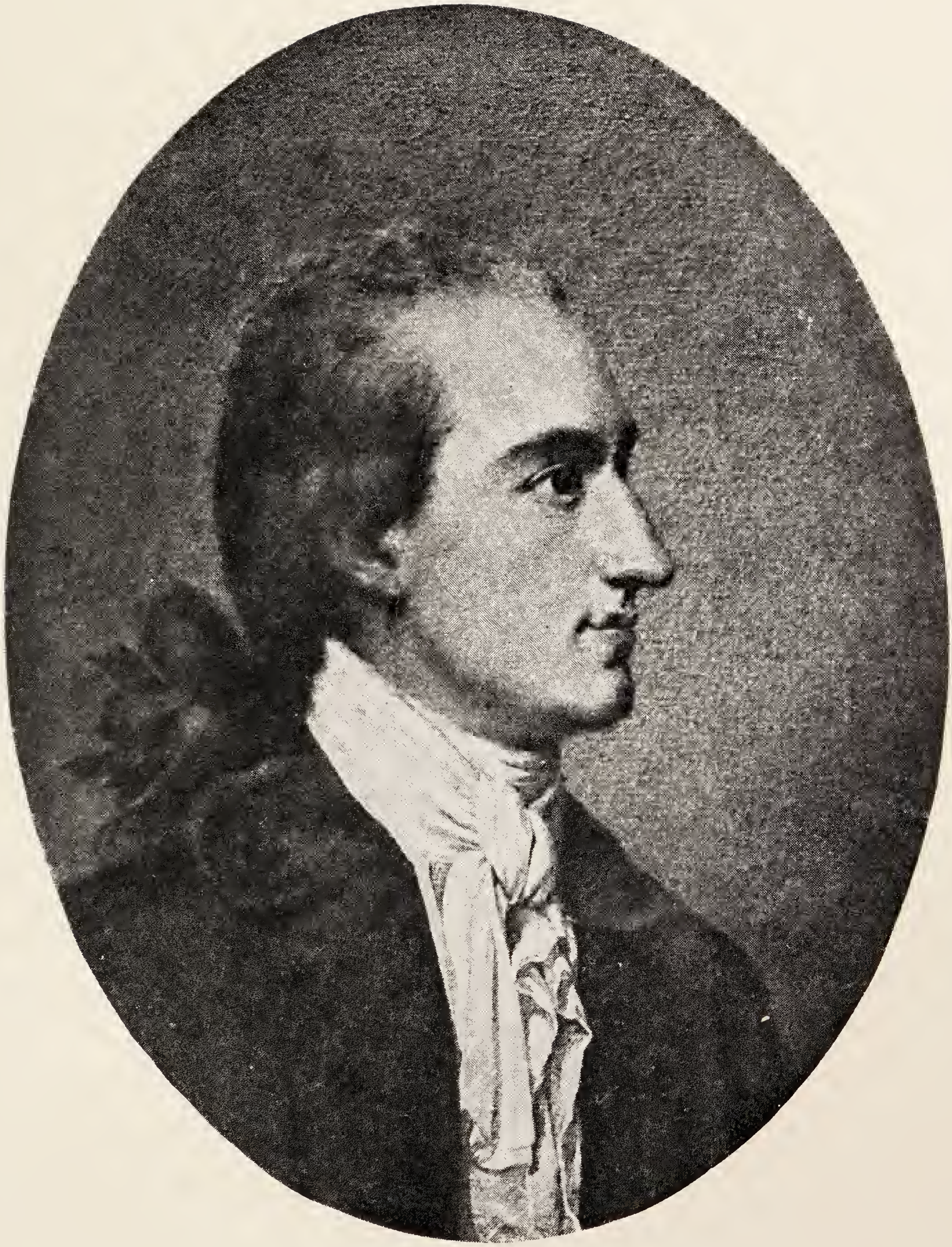
The other celebrated names which we shall encounter in this discussion are Richard Owen: "On the archetype and homologies of the vertebrate skeleton" (1848); Holmes Coote: "The homologies of the human skeleton." (1849); and T. H. Huxley: "On the theory of the vertebrate skull" (1858). Then we shall encounter a most intensely interesting discussion between Cuvier, who together with Schiller had been a student at the Karlsschule in Stuttgart, and later became a professor at the University of Paris, and Geoffry St. Hilaire, Cuvier opposing the theory of Goethe and St. Hilaire defending it. No less an authority than Alexander von Humboldt has described this controversy to us, as it was Humboldt who made the study of great men one of his sports and who encouraged Goethe in many instances to write out his brilliant ideas. But we cannot conclude the list of celebrated names in this connection before we state that among the actual human skulls studied by Goethe were those of Raphael and his friend Schiller. There is some doubt, however, whether he really had in his hand the genuine skull of Raphael, for Karl Gustav Karus asserts in 1853, that the real skull of Raphael was discovered two decades before that date in the sarcophagus of the Pantheon.

METAMORPHOSIS OF PLANTS

The conception of continual development in living things was the bolt of lightning that parted the fogs of that century, and flooded it with light. The metamorphosis of a plant is only one particular application of the idea of development, showing the ceaseless, ever-progressing growth and transformation of the fundamental or original organ into more and more perfect and effective organs, in order to eventually reach the highest point of organic activity.

Finally, Goethe identified the concept of metamorphosis with the conception of development in general and he states that this thought encompasses his understanding of the entire organic world. This we can conclude from his letter from Naples on the 17th of May, 1787, when he writes, "The same law will be applicable to all other living things.

In the discovery of the metamorphosis of plants Goethe gave a magnificent testimonial of the idea of development in general. It will be recognized that he did not establish and advance the idea of development as

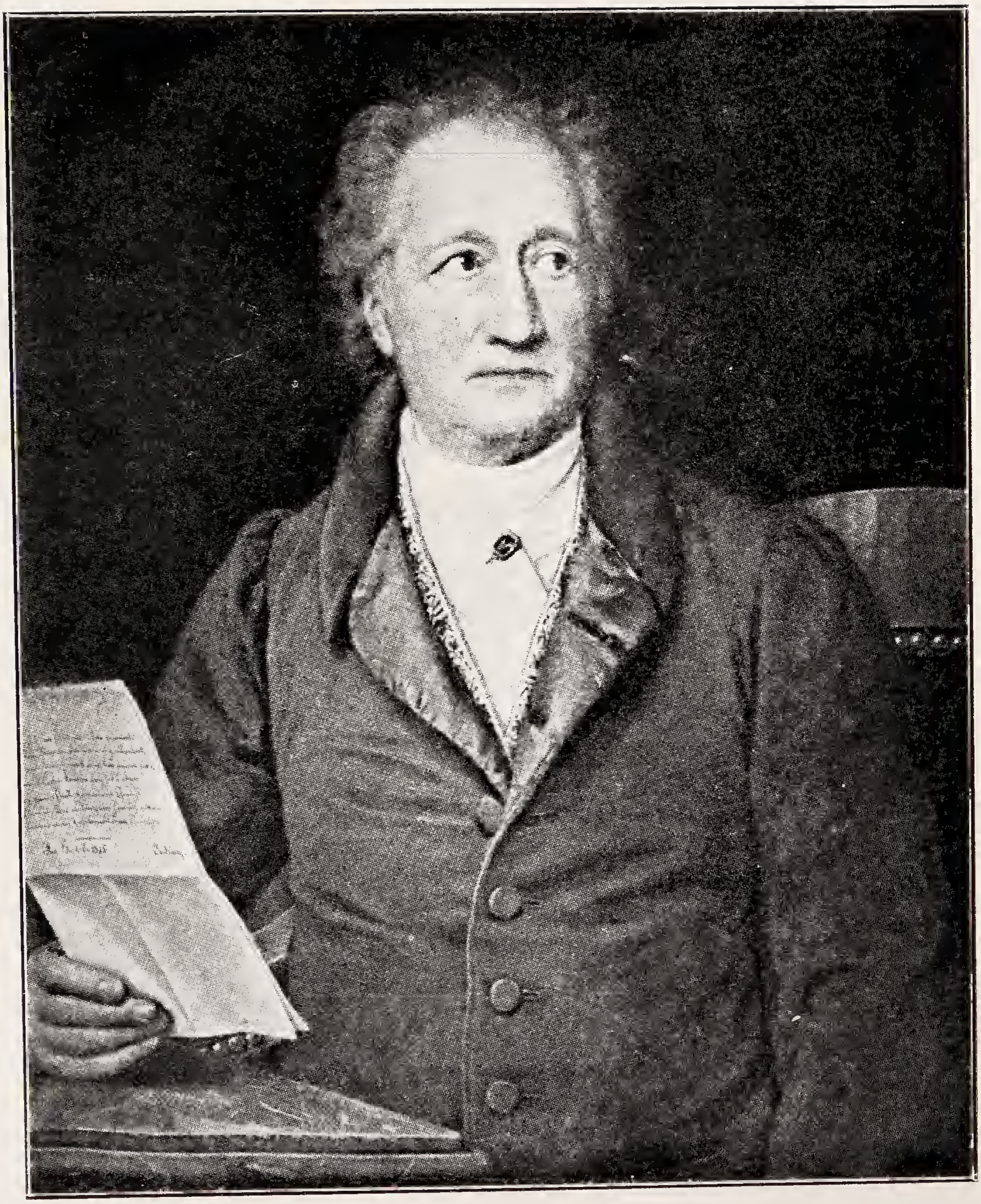


Goethe
(1779)

Goethe
(1786)

Goethe
(1791)

Goethe
(1825)



Goethe
(1828)

GOETHE
(1832)

the highest scientific principle until he had studied and grapsed the true history of the plant, the successive coming into being or existence from insignificant beginnings. In other words this was the manner of progress: first study or observation, then the detection of the changes of form in various parts of the plants from one underlying aboriginal forf or type, and thirdly, the establishment of a principle as a basis for biologic research. This method is nothing more nor less than what modern biologists designate as the *genetic* method, and one of our greatest naturalists (Virchow) states without reserve that it was Goethe who founded the genetic method in all its generality. Goethe's method of thinking is genetic, and when that statement is fully understood we at once bring this great poet, philosopher, and naturalist closer to our understanding and from now on we may be better able to perceive his amazing contributions to those human achievements which we designate as "art." Goethe had traveled to Italy because he hoped in the rich flora of that country to gain a deeper insight into the nature of plants and to elevate his ideas to the dignity of a well-substantiated theory by a closer objective study of plants than was possible in his own country. But Italy is also the land for the study of art, and it soon revealed itself to him that in the gift of the genetic method he had found the key for the deepest insight into human art. In other words by perfecting his aspects of nature he gained a perfect artistic consciousness. He won a true perception of art by the insight gained in the metamorphoses of plants. On November 11, 1786, he writes to Frau von Stein: "You know my old manner of treating natural subjects; thus I am treating Rome and already revelations are rising up towards me. Just as I observe nature, so I observe art." And on the 29th of November, he writes to Herder, "Now my dear old friend, architecture, sculpture, and painting are just as mineralogy, botany, and zoölogy to me."

It was now clear to Goethe that the deepest insight into nature is just good enough for the highest perfection of human art, and that for the mastery of art the same genetic method was required as for the mastery of nature. He says in fact, "We can only compete with nature in our use and applications of art when we have to a degree at least learnt from her the methods which she uses in the formation of her work. Now how does nature proceed to produce the living form, the models for the artistic? The answer is by development. We have then on the highest stage of human achievement, namely, art, not to seek the perfected creature, that which is a complete being as such, as the object of art, but rather the relation of various parts of the object to the totality of the object."

In the doctrine of the metamorphosis of plants, Goethe had a predecessor in Germany, namely, Kaspar Friedrich Wolff, who expressed the identical thought, that all collateral organs of higher plants are modified leaves. Wolff's observations were unknown to Goethe, however, when he first announced his own views concerning this interesting subject. In fact, Wolff's work remained unknown to all of Germany until Goethe himself pointed out its merits and gratefully acknowledged him as a talented "Vorabeiter." Now we know that Wolff's conception was not as correct as Goethe's, for he understood the homologies in plants to be due to degenerations and atrophies—they were according to Wolff, retrogressive or pathologic metamorphoses. But according to Goethe, they are progressive metamorphoses—as we should now say, a part of the living plants' endowment of adaptation to environment. It is an actual indisputable fact that science has received and taken over the doctrine of the Metamorphoses in living things from Goethe.

But it required decades before science took possession of this priceless gift of thought and observation, for the "Versuch die Metamorphose der Pflanzen zu erklären" appeared in 1790. Not until the summer of 1831 was it received by a scientific body of acknowledged standing. In that year, Goethe presented a translation of this work, that had been made under his direction by Soret, to the French Academy of Sciences. The work was presented by the great botanical expert and professor of botany at Paris, Geoffroy de St. Hilaire. In his report to the French Academy we find the following remarks:

"When Goethe came out with this work in 1790, it was little appreciated; indeed, there were opinions that regarded it close to a mental aberration. Likely there was an error in it, but such a one as only a genius could commit. Goethe was wrong, namely, to let his treatise appear too early—half a century too soon—before there were botanists that were capable of studying and understanding it."

What higher approval could the poet expect or could we demand? And yet we should appreciate only the smallest part of this extraordinarily brilliant contribution, if we could see in it only the demonstration of the identity of all those formations which we designate as lateral organs of the plant axis. Far and beyond this, an infinitely greater, higher and more comprehensive thought underlies the whole trend of argument, namely, the *idea of development.* Nowadays we use that term in such a matter of fact way and use it so frequently that we can hardly imagine a time in which it was ever used differently or in which it was not used at all. If we reflect that there was a long period in the history of science in which the belief was deeply rooted that living things always possessed the form, shape and function in which the observer at the time beheld them, how could there be any thought of development, especially of creative force acting *continually,* through inherent energies, within the living thing.

At Linné's time (born 1707) the chief aim of botany was by his superlative talent directed toward the establishing of a botanical system by external signs exclusively, and supplementing it with commentaries. Linné and his school classified and systematized the organization of plants and exhibited no desire to trace the inner connection of the plant organization as a totality. They had not the remotest idea that the task of a science is not concluded with the establishment of systems and classes, but is not even begun until we investigate the processes of *coming into being of these organizations.*

In his essay on Joachim Jungius, we find a reference by Goethe to Bacon, of whom he says: "He regarded the differentiation and accurate description of that which is distinguished as the true doctrine of Nature," and then continues, "The conviction that everything must be complete and perfect before we should devote due attention to it, has put the century into a fog." Indeed all scientific intellects of the day appeared to be controlled by the thought—"Nothing can come into being except in such a way as it has already existed." According to this concept the plant was thought to be completely preformed in the seed, in a most diminutive form.[4]

There was therefore no idea of development but only perhaps of evolvement. This doctrine of preformation dominated all thinking on genesis of living things and held human knowledge in a state of lifeless rigidity, from which somatic death Goethe rescued it by his elucidations on continual development.

GOETHE'S INTEREST IN CHEMISTRY AND PHARMACOLOGY

It is a singular coincidence that Schiller who was an educated physician practically abandoned his scientific researches in his later years, whereas Goethe, who was originally a lawyer, pursued the study of medical matters from the time of his youth. As a boy he showed his liking for plant life in his excursions to the suburbs of Frankfurt, and in his "Wahrheit und Dichtung" he relates how, when a student at Leipzig, he dined with Professor Ludwig, physician to the court, where he met several other medical men and young doctors, with whom he engaged in a very animated discussion over the lectures on Haller, Linné, and Buffon, quite in contrast to the wearisome expoundings of his lawyer friends and philosophers Ernesti,

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[4] See St. Augustine "De genesi ad Litteram. Also St. Augustine and Evolution, by Henry Woods, S. J.

Bohm, etc. On his return to Frankfurt, Goethe took up the use of retorts, distilling apparatus, and blast furnaces, in part suggested to him by one of his older friends, Fräulein Susanne von Klettenberg, by which means, according to Welling's "opus mago cabbalisticum," he prepared a silicic liquor by fusing quartz sand with alkali, giving an extremely deliquescent salt, to which he ascribed peculiar healing properties. He prepared many other crystalline combinations and medicinal salts. During his second semester at Strassburg, Goethe took up the study of chemistry under Spielmann and of anatomy under Obstein. He also perused the writings of Boerhaave and Paracelsus, to which he refers in "Wahrheit und Dichtung."

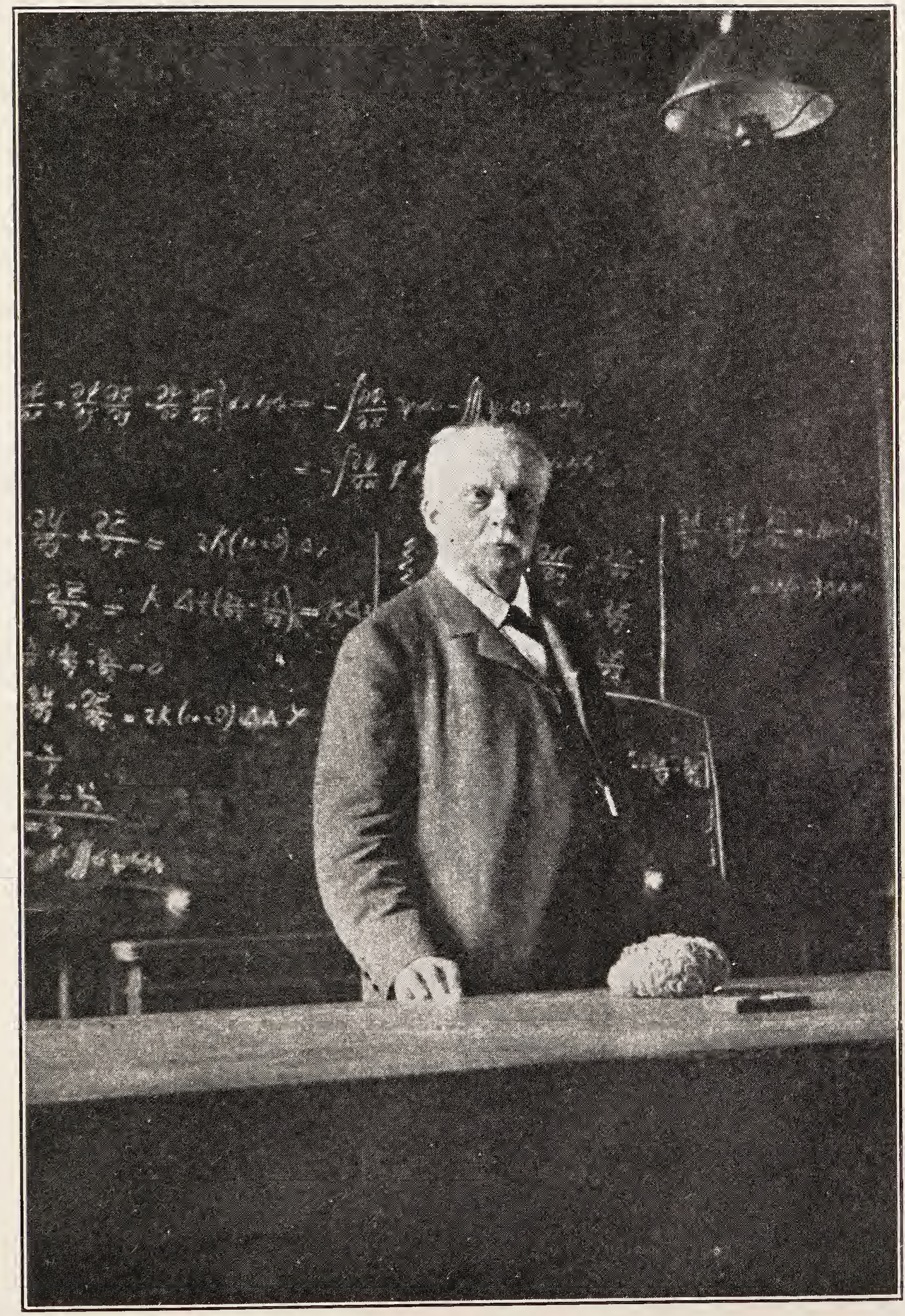
Numerous references are found in "Faust" and various alchemistic sentences and quotations from medical authors and medicine are noted. Thus he cites from *Schubsii Theses ad mat. med.* the formula for an emmenagogue:

℞. Arist. rot. ʒ II
Tart. calyb. ʒ I
Aq. Font. ʒ II
Fiat infus.

(The measures as given here are very likely incorrect.)

At that time the druggist was looked upon as one of the busiest and best informed naturalists and people would go to him for advice in many and divers subjects of nature. They brought to him all sorts of bugs, worms, plants, stones, etc., for explanation or to add to some collection for further study. When, therefore, Goethe left Strassburg and made his trip through Switzerland and had arrived at the Court of Weimar in November, 1775, he became the archducal president of the chamber for mining, forestry, horticulture, and director of the ducal museum; he was constantly in scientific contact with artists, miners, mechanicians, physicians and apothecaries.

In his investigations in the fields of mineralogy, geology, and mining his chief advisers were von Tebra, Lenz, and Voigt. He established a museum of mineralogy in Jena and received some particularly fine specimens of adularïa from apothecary Bergmann, found on a field near Trier. Apothecary and administrator Frass in Hof-Geismar sent him some Hessian aragonites and apothecary Gabora, of Göllnitz, contributed such beautiful aragonites and other precious stones, that Goethe recommended the award of the medal of honor by the Grand Duke. Apothecary Vogel, of Bayreuth, imparted to Goethe his discovery of the decolorizing properties of coal. Goethe came in personal contact frequently with the apothe-



HELMHOLTZ

caries of the archduchy, especially Wilhelmi, of Jena, and Dr. W. H. S. Buchholz, apothecary to the court (1737–1798) in Weimar. This Dr. Buchholz was a most versatile naturalist and chemist. His studies in botany mark him as a predecessor to Darwin's theory and Buchholz, the only apothecary of the town, encouraged Goethe in all his researches in this line, as well as furnished him with able chemical assistants. In his researches into the various forms of air, Buchholz astonished the natives of the city by sending up the first fire balloon and liberating a score or more of pigeons from it. It seems strange to speak of two such divergent subjects as the above, but at that time, chemistry and botany were the main subjects of scientific investigation in Weimar and Buchholz wrote a dispensatory on these subjects. He had in his own garden not only the official plants, but also many rare and exotic specimens.

In Goethe's "Tages und Jahresheften," 1796, speaking of his relations to Buchholz, he says: "A company of highly educated men meet every Friday. I read a song from the Iliad by Voss, for which I received applause, the poet received praise and the translator well deserved credit. Every member contributed to the discussion of any topic brought up. Buchholz in particular gave interesting narratives on his physico-chemical researches."

During the year 1796, in which he attended these Friday meetings at the home of Dr. Buchholz, he wrote his incomparable idyl "Hermann and Dorothea," in which he extols the qualities of his fellow-citizen of Weimar, as a man ever ready to help his neighbor and to give his aid to those desiring knowledge on the wonders of nature, and he gives a tribute, well deserved, to the profession of pharmacy, in choosing his characters of Heiberg's "Apotheker Heinrich" and Hans von Zobelitz "Provisor Dingelda." In the following year Goethe wrote his "Metamorphosis of Plants."

At one of these weekly meetings at Buchholz's, his provisor Dr. Göttling made certain experiments to render panes of glass opaque, but was unsuccessful. Among other experiments, he discovered that phosphorus would burn in nitrogen. He was the author of a "Handbook of Theoretical and Practical Chemistry," in two volumes, published in Jena 1798–99, at which city Goethe had him appointed as professor of chemistry pharmacy, and technology. Upon his death in 1809, Goethe again recommended a pharmacist, Dr. Dobereiner, to fill the vacancy. Dobereiner had already won a reputation as chemist by his many original researches and discoveries, so that Goethe became his constant consultant upon pneumatic, color, and fermentation problems in chemistry and this is attested by the fact that there are in existence on these subjects forty-five letters from Goethe to Dobereiner and six from the latter to Goethe. Dobereiner contributed largely to stochiometry in which Goethe took a lively interest. He also had correspondence with Gmelin and Schweigger, whilst Berzelius and Oersted visited him frequently. Goethe recognized at once the importance of the discovery of the latter, namely, the deviation of the magnetic needle by the electric current.

In botanical matters Goethe was in correspondence and personal touch with von Martius, Skell, and especially Nees von Esenbeck. His relations to the latter, upon whom he looked not only as a botanist of the highest order, but as a man of great personal characteristics, are attested by seventy-five letters still in existence.

Besides the above, Goethe acknowledged his indebtedness to a family named Dietrich, in Ziegenhain, who for generations had supplied pharmacists and herbalists with material collected by them and whom Goethe often consulted about certain specimens. He was always supplied by the grandson of this family, Friedr. Gottlieb Dietrich, with the freshest, blooming plants gathered in the surrounding mountainous regions.

Of Linné, Goethe wrote to Zeller as follows: "I have again read his works and am frightened at the extraordinary accomplishments of this man. I have learnt a great deal from him, but not botany. He has confused me by his systematic arrangement and laws regarding the species."

From his botanical researches, many think that Goethe was the forerunner of the Darwinian theory. It is hard to say in what branch of the natural sciences he most excelled. His great tenacity in the study of optics and colors is well known. Of chemistry, he wrote to Fräulein von Klettenburg, August, 20, 1770, that it secretly had mastered him. He took up anatomy by attending the lectures of Loders every morning, even in the deepest snow. Botany had an irresistible attraction for him, whilst zoölogy contributed to his knowledge of comparative anatomy.

ASPECTS OF EVOLUTION COMPARED WITH THOSE OF ANCIENT NATURAL PHILOSOPHERS

According to Virchow, Goethe was the first one to distinctly announce that his view of nature was creative power acting uninterruptedly, as it were, in continuance. This we find clearly stated in his little book on the Metamorphosis of Plants. One of the infirmities of all hypotheses concerning the origin of species is that they can usually be driven back to some starting point where the very condition of things is assumed for which the theory professes to account. Helmholtz also states very definitely that Goethe's explanation of the origin of species is at the foundation of the Darwinian theory; on this special question I have submitted a personal study (p. 589). The British writers who have been rather conservative in their judgments of the genius of Goethe, declare that if we go back of Darwin for authors of the hypothesis of evolution we will have to mention names that even antecede that of Goethe. This is undoubtedly true if we are to judge simply by historic statements concerning the imaginings to which great thinkers have given expressions. But it is not true if we permit ourselves to be guided solely by such expressions as are supported by objective studies of nature itself, and Goethe was a close and objective observer of nature. If simple theorizing will constitute authorship, we shall have to go back to Empedocles, 504 B. C., who believed that in the realm of living nature, plants originated first, then lower animals, then higher animals and finally man. He described the active factor in this development as being the destruction in their struggle for existence of the animals unfavorably constructed, whilst those capable of surviving propagated the species and transmitted their favorable structure. Empedocles was the first philosopher to announce a theory of such far-reaching importance. As was usual in his days he followed up many sciences, but chemistry and physiology seem to have been those in which his brilliant mind gave birth to the most original conceptions. For instance, he was the father of three fundamental thoughts which have maintained themselves in modern chemistry up to the present day. The first was, that there was a multiplicity of chemical substances but a limited number of fundamental or elementary ones; secondly, that they could enter into many combinations among themselves, and thirdly that many quantitative differences and alternating proportions of these combinations could be recognized. These are profound and enduring contributions to the world of thought. In biology, however, his theory of evolution just stated in the preceding, was really nothing but an anticipation, a presentiment, a doctrine not supported by objective observation. Concerning his ideas on the survival of the fittest, and his attempt to solve the riddle of the adaptability to purpose, they are so mixed with childlike precocity and daring efforts to rob nature of her veil in one keen dash, that they read like efforts of a man who does not seem to have learned the ABC in the school of scientific endeavor and reserve. Theodor Goemperz in his "Griechische Denker" (Band I, page 196), quotes a sentence from Empedocles which suggests him as a precursor of Goethe in the domain of comparative morphology, and it is a thought which might have been helpful to later naturalists in the erection of the edifice of the theory of descent, but let us see whether this sentence justifies such a conclusion; here it is: "Hair and foliage and dense plumage of the birds, are one." It must be evident that this is a very flimsy foundation upon which to compare Empedocles as a man of equal intellectual capacity with Darwin or Goethe. For when we read deeper in Empedocles we find that his leading thought "that the perfect was evolved from the imperfect" was described in the most adventurous terms, something as follows: At first heads without necks or trunks sprung from the ground, arms without shoulders, eyes without a face, and the "tie of friendship", as he calls it, linked some of these fragmentary creations together; others were driven about in a lonesome way without getting a firm footing upon the "strand of life." The unions that did fortuitously take place often resulted in wonderful monstrosities, double headed creatures, human forms with the heads of oxen; but these are examples enough to demosstrate the fanciful and wildly imaginative speculation of the Greek philosopher.

XXX

THE WORK OF CHARLES DARWIN IN THE LIGHT OF RECENT BIOLOGIC RESEARCH

THE four great branches of study that have furnished the evidence of organic evolution are, comparative anatomy, embryology, paleontology, and experimental breeding or genetics, but there is an additional method which has almost been forgotten and this is the method described by De Candolle.[1] The method is described on page 43 of Ostwald's German translation and consists in selecting as large a number of persons as can be persuaded to submit themselves to investigation and examining them for their physical peculiarities, their health, merits, talents, their parents, and grandparents, what they remember of traits that have been transmitted from previous generations, etc. etc. There is an advantage in selecting older persons because they are not so likely to conceal deficiencies but confess them more freely than younger persons because they have become indifferent to the opinions of others. De Candolle's anthropometric biologic and psychologic studies aim at the following four qualities:

1. The external forms and the physical appearance.
2. The internal constitutions as far as they can be revealed without an autopsy.
3. The instinctive dispositions, inclinations, feelings, emotions, instincts, such as are recognizable even in children before education and example have influenced them.
4. The intellectual endowments.

One must pay attention to the extremes in these four qualifications, for it is these that signalize the individual, for the average and the middle qualifications belong to the race. For instance, all human beings have a memory, but a very strong memory or weak memory is the indication of an individual. I will not go into the finer details of this highly interesting and useful program of De Candolle's, but one of the features of the fourth class which he mentions, is correct or mediocre power to form conclusions, sound or paradox habit of arriving at judgments. Into this category falls one of the commonest characters of human beings, and because it is common it has been called "common sense." The French designate it as *sens commun* and the Germans speak of it as *Gesunder Menschenverstand.* The Scottish philosophers have endeavored to fathom the nature of common sense and Beattie, who in 1770 brought out a history of philosophy, gave the notable definition: "Common sense has in modern times been used by philosophers, both French and British, to signify that belief not by progressive argumentation, but by an instantaneous, intuitive, and irresistible impulse derived neither from education nor from habit but from nature." This *communis sensus* was estimated highly by the Romans and used in exactly the same manner as we use it to-day. Horace (sat. I, 3, 66) tolerates the rebuke "Communi sensu plane caret." Lord Shaftsbury has given one of his most successful essays this Latin title and Goethe took the French dictum "Le sens commun est le gènie de l'humanité" into his collection of important proverbs.

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[1] L'Histoire des sciences et des Savants depuis deux siècles, suivie d'autres études sur des sujets scientific, en particulier sur la sélection dans l'especè humaine. (1873).

It is well to bear in mind in spite of all these important approvals of the term "common sense" that it is an instrument which can be used safely only by the highly gifted. The difficulty with common sense is this, that in important situations, when the emotions and bodily feelings take no part in the matter to be decided, this natural instinct expressed by common sense rarely decides between alternatives, and that we do not concede habitually a decision to it, even when it expresses itself very emphatically. All of our purposeful actions are arrived at after long vascillation and careful deliberation, and yet there is a place for common sense if the individual possess it, and since this was a feature in the character of Charles Darwin which one can detect in most of his observations and experimentations, I shall dwell to some extent upon it here.

Common sense comes in as a great gift and instrument if, after due deliberation, discernment, and testing, there is still an inhibiting state or vascillation of the mind. Then common sense compels the balance of judgment to swing one way or the other. Sallust says, "Priusquam incipias consulto, et, ubi consulueris, mature facto opus est" ("Deliberate well before you begin: but, when you have so deliberated, then act with promptitude, decision"). We may wait, it is true, and consign to centuries to come, the toils, the glories, and the hopes of science, or we may rely on an easy effort distributed over a length of years for the accomplishment of much that vigorous exertion might now effect; but we should recollect the admonition of the poet:

Nimm die Zögernde zum Rath,
Nicht zum Werkzeug deiner That.

The feeling of the astronomer, laboring under the weight of his vast cycles, patiently watching the slow evolutions of cosmic events, and breathing forth his aspirations after a perfection which he perceives to be attainable in that tone of protracted hope which borders on resignation, has somewhat too much pervaded other sciences. There are secrets of nature we would fain see revealed while we yet live in the flesh, resources hidden in her fertile bosom for the well-being of man upon earth we would fain see opened up for the use of the generation to which we belong. But if we would be enlightened by the one or benefited by the other, we must lay on power, both moral and physical, without grudging and without stint.

De Candolle does not agree with my definition of common sense. He leaves out of his definition the factor of the instinctive, subsconscious action. He states that common sense is the capacity for weighing off antagonistic facts and opinions for the purpose of extracting that which is most probable. He distinguishes it from the capacity for reaching conclusions which he defines as the gift for logically tracing out a series of connected ideas. In what I have said just previous to this, it must become evident that common sense is the very fulcrum upon which the decision or a conclusion rests.

As it might be thought that this was the most telling characteristic of Darwin's mind, it is now important to enumerate twenty-nine other characteristics which De Candolle has discovered in Darwin, and to state that he boldly compares him with Napoleon Bonaparte, for he says "Darwin was also a conqueror." In a few years he conquered a prodigious domain for science against a powerful opposition. Then follow the moral and intellectual indications which De Candolle has gathered from Darwin's work through his personal relations with him, for De Candolle and Darwin were personally acquainted. He also gathered information from Darwin's relatives and friends.

It must be born in mind that De Candolle enumerates the defects and faults as well as the advantageous character traits of his great men. It is fascinating to observe his comparison of Darwin with Napoleon. Darwin's twenty-nine character traits are the following: Strong will, tenacity, activity and diligence, love of truth, honorableness, kindness, generosity toward his predecessors and rivals, sense of duty, eagerness for knowledge, modesty and humility, independence of opinions, courage to express them, no superstition, regular habits, exact observance of rules and conduct, punctiliousness, no inclination towards society, no gift or endowment for plastic art or music, strong attention, pronounced common sense, correct power for conclusions, fine sense of observation, accuteness in forming deductions, capacity for generalizations, good memory, strong fantasy, low metaphysical capacity for abstraction, little talent for mathematics, but extraordinary capacity for looking over and comprehending a great mass of single details.

I will not reproduce De Candolle's account of the character traits of Napoleon Bonaparte, among the advantages of which are mathematic talent, strong will-power, great confidence in himself, diligence, etc., but among the disadvantages we find dishonesty (shown by broken treaties) despotism, jealousy of rivals, contempt for the weak, pride, vanity, superstition, licentiousness, vehemence and murder (he was responsible for the assassination of the Duke d'Enghien). De Candolle gives thirty-seven character traits of Napoleon with fifteen defects and no moral advantage. Darwin has twenty-nine character traits, not one defect, and many advantages.

Among De Candolle's character traits of Darwin are disinclination to seek society, no endowment for music and art, and strong fantasy. Of these three traits I wish to speak in detail. Darwin was the very opposite of those investigators who cannot live without entertaining themselves with collaborators and pupils, and who never miss an opportunity for attending society meetings. He did not decline to converse and his conversation was always pleasant and congenial, but he preferred a retired life in the country without going to London for the social season, and he carried out this life consistently. During his youth he spent five years aboard a ship. Concerning his lack of talent for music and art, there is a note in De Candolle's work (l. c. p. 62) that Darwin wrote to one of his relatives concerning the emotions which music evoked in him. It moved him to tears occasionally, although he had no good ear for music. I have studied this phenomenon in personal friends and should class it among the collateral psychic effects of music caused by a vivid perception without a special power of musical appreciation (see Chap. IX).

Opponents of Darwin accuse him of having had too much fantasy, but the conservative scholar who reads his works finds that almost everything is grounded on single observations, the results of which are grouped with severely conscientious accuracy. This would indicate rather a lack of fantasy, or at least a fine control of it, for every experimenter requires a certain amount of fantasy to think out the plan of the experiment and to conjecture the relation between the facts. Darwin was the grandchild of a poet and De Candolle reports that Galton told him that in his youth Darwin was passionately fond of poetry.

In my Methodology, Chap. I and II as well as in my investigations of the life and work of Lavoisier, Haller, Goethe, Darwin, and Virchow and others, I have emphasized the characteral traits in all of these men which must be present before any telling scientific work can be accomplished. Predominant above others there must be memory, discernment, will-power, endurance, sense of order, attention, independence of mind and opinion, eagerness for knowledge, love of truth, and love of work. But these qualities must not be too much overshadowed by exuberant development of fantasy, nor by vices or moral defects. Most all of these men, judged from the standard of Christ-like perfection, evidence one weakness or another, but there must be no vices, nor must the good qualifications be inhibited by a luxurious development of the imagination. Some of these qualifications may be present to a small extent, but this defect can be vicariously made up by an overdose of another good qualification. Thus a good judgment may replace a weak memory, a strong will and memory may replace lack of attention. Great facility in comprehension may replace a defective endurance and application. Great master minds very rarely have the good character traits always present in the same amounts, but in Darwin we have an example of a man in which they were most abundantly and homogeneously represented. The only other great personality that approximates him in this was Leonardo da Vinci. Another feature I must emphasize here is the difference in advantageous and disadvantageous character traits in youth and mature age of the master mind. Almost all of the anecdotes and narratives of a light vein that are recorded, to indicate the humaneness of great men, refer to their early years, even if they are historically well-founded. The mature master is an earnest thinker, very severe with himself, and too conscientious to waste his time on trivialities. It is not the right thing to insert humorous anecdotes in a character study of master minds of medicine, a natural science.

In his essay, "Über Vererbung beim Menschen" (Verhandlungen der Gesellschaft Deutscher Naturforscher und Arzte, 17th to the 24th December, 1922, p. 129) F. Lenz goes out of his way to emphasize that all efforts to improve the efficiency of the human race by bodily and physical education are perfectly hopeless. This is a standpoint which even the most violent antagonists to the transmission of acquired characters by heredity wisely leave undecided, for education and bodily improvements are effects of environment, and some of our leaders among the geneticists prefer to leave the question of the effect of environment on the soma and germ plasm open. E. G. Conklin discusses this question in his book on "Heredity and Environment" in which he defines the essence of education as being self-discovery and self-control. When education helps the individual to discover his own powers and limitations, and shows him how to get out of his heredity its largest and best possibilities, it will fulfill its real function, but he also says that notable human improvement can take place upon two conditions: first, the physical and intellectual improvement of the individual through environment, and training, must not interfere with his racial and ethical obligation, that is, individual freedom must be subordinated to racial welfare; secondly, the promotion of human evolution must be undertaken by society as its greatest work. These two things have sprung out of Darwin's epoch-making researches, and the greatest leaders in genetics and eugenics are followers and relatives of Darwin, his son, Leonard Darwin, and his cousin, Sir Francis Galton. Clinicians and practical animal breeders of race horses, cows, and hunting dogs are loath to accept the dictum that training during the lifetime of the individual, cannot affect the offspring. Physicians believe to a very large extent in the transmission of some acquired diseases. That leads them to believe also in the transmission of advantageous acquisitions.

PSYCHOLOGY OF DARWIN

Charles Darwin was born on the 12th of February, 1809, on a Sunday, in the village of Shrewsbury, England. His father, Dr. Waring Darwin, was at this time a respected and busy physician, who was famous for his excellent memory and the sincere interest he evinced in the weal and woe of his co-human beings, which interest, it is attested, was the result of his great goodness of heart. Of his scientific works, the one "On the Ocular Spectra of Light and Colours," published in 1786 and mentioned by Goethe in his treatise on colors, has become most widely known. Dr. Darwin married a daughter of the famous pottery manufacturer Josiah Wedgwood, and became the father of a large family. He died on the 13th of November, 1849.

Dr. Erasmus Darwin, the grandfather of Charles, possessed great insight and ability for research, the very qualities which made the grandson so famous. Dr. Erasmus Darwin was born on the 12th of December, 1731, at Elton and died on his estate near Derby in 1802. He was one of those rare persons whose vision takes in the whole perspective of things, a genuinely philosophic nature which never tired of seeking the connecting link in the manifold phenomena of nature.

It has recently been the custom to refer frequently to the works of the older Darwin and to cite here and there some of his statements. Among a hundred scientists, however, there can be found scarcely one who has actually taken the trouble to scan the pages of his "Zoonomia," with which we are here concerned, so as to form an acquaintance with the contents of this remarkable work, the express purpose of which was "to connect the scattered facts of medical practice and to bring the laws of organic life under one point of view."

The superior comprehension with which Erasmus Darwin treated scientific and biologic problems, is evident in his accurate criticism of the Preformation or Capsule Theory, largely associated with Albrecht von Haller, according to which every individual (since the creation of the species) is derived from its predecessor in a similar manner as a single capsule would be derived from a series of capsules one smaller than the other, if a set of boxes or capsules were so arranged that the series contains an infinite number, one contained within the other. In regard to this absurd and now completely rejected theory, Erasmus Darwin makes the following important remarks in the chapter devoted to reproduction, in the second volume of his "Zoonomia": "Many philosophers have found it very difficult to comprehend reproduction in animals and have assumed that the entire series of numerous descendants have been preformed in miniature in the original individuals of the species, these infinitesimally little beings simply spreading or developing later, as the embryo in the uterus. Aside from the fact that this idea is not supported by any known analogy, it attributes moreover to material organisms a greater subtility of form, than we can assume probable; each of the capsuled embryos is supposed to contain all of the most complicated parts of the body; these parts would have to be much smaller than the little demons which tempted Saint Antony; two thousand of these were reported to have danced a Sarabande on the point of the finest needle without being in each other's way. Others have assumed that all parts of the embryo were formed in the male, before they were deposited in the egg or in the mother and that they were then developed according to the above hypothesis. This assumption, however, is nothing more than an attempt to get rid of one difficulty by substituting another for it. They could not understand how the embryo develops in the uterus or the egg and hence assumed the development before it arrives at its destination. To these two theories, we place in opposition: A) many animals, e. g., crabs, can replace whole organic parts, such as a leg, which has been torn off; others, e. g., snails or worms, can replace a head or a tail if one or the other has been amputated. In the case of these animals, then, new parts are supplied, which it would be impossible to assume to have existed in minature beforehand. B) In many diseases new parts or formations develop, e.g., on the cornea during inflammation of the eye, and in ganglions and ulcers, concerning which it would not be right to assume that their prototypes existed in the embryo. C) Moreover, how could bastard animals be created, inheriting some qualities from the father and some from the mother?"

These objections are so much to the point, are so accurate, that no better arguments could be offered to-day to briefly demonstrate the absurdity of the preformation or capsule theory. Eramus Darwin, however, is so thoroughly aware of the primal error involved in this matter, and of the resulting great obstacle to the whole science of the phenomena of life, that he brings forth from his extensive accumulation of information other weapons so as to hew out a path to the correct view. I shall quote in full the respective argumentation, which is characteristic of the unique logic of the older Darwin. He says: "When we seriously consider the great changes which take place after the birth of some animals, e. g., in the case of the vari-colored butterfly from a creeping caterpillar, of a lung-breathing frog from an aquatic tadpole, of a bearded man from an effeminate boy, of a nursing mother from a childish virgin, which changes, however, can be checked by mutilation of certain reproductive organs—when we, again, picture to ourselves the great changes which are brought about in some animals either by accident or by means of training, e. g., in the case of horses, whose strength or speed have been trained for various purposes, to serve as beasts of burden or as racers; or in the case of dogs whose strength or courage have been trained (witness bulldogs); or for keenness of scent (bloodhounds and pointers); for rapidity (hunting dogs); or for swimming and the pulling of sleds in the snow (the rough-haired dogs of the north); or finally, the playdogs for children and lapdogs—if we, moreover, consider the great changes in form and color, which we can note daily in the case of smaller animals through domestication, e. g., as with bunnies and pigeons and even through the variations of the seasons, e. g., as with sheep which have hair instead of wool in the warmer climates; moreover as with hares of the snowy regions which become white in the winter months—if we add to the above the various extensive changes in the form of man through his mode of life and occupation, which become permanent after a number of generations, e. g., as with those who work in front of an anvil, or in foundries, or at the loom, or as porters, or as ropedancers, can be differentiated according to the form of their limbs—when we enumerate the great changes which take place in the case of species of animals before their birth, that is to say, such changes as make them revert to types of their parents, which had undergone previously changes brought about by accident or artifice, and on account of which these changes were passed on to the descendants, —with all of this, we cannot do otherwise than become convinced that the fœtus or embryo is formed with the addition of new parts, not through the development of a primal nest of embryos, which like the goblets of a sleight of hand man are encapsuled one in the other."

The very beginning of every organic being, Dr. Darwin pictured to himself as a filament and compared it to the fibers out of which the ends of absorbing material proceed. This living filament he regarded capable of becoming active through various excitations and assumed that under certain conditions it could form into a ring and through its further accretion of nourishing particles it could transform itself into a living tube. Although this conception of the first stages of embryonic evolution does not coincide with the results of present-day microscopic research, nevertheless his accompanying remarks, in particular his emphasis of the fact that with the addition of new parts, new kinds of excitation must spring into existence, which then again react on the whole organism and thus create a very complicated correlation between the separate organs and their functions, are in as good standing to-day as a hundred years ago. We have progressed, in the meantime, very much further in this branch of biology and can prove, in many instances, physiologic correlation, but we can explain it in very few cases. The whole region is still very much of a *terra incognita* of the science of organic nature, which awaits further exploring through the biochemistry of the internal secretions and the neuro-chemic relations associated therewith.

Through a better understanding of the process of individual evolution, Dr. Darwin soon arrived at a means of interpretation of the origin of species and it is here that his remarkable acumen shows itself in all its glory. The reader should note that the later assertions of the Frenchman Lamarck have been anticipated by Erasmus Darwin. Consider the following line of argument: "Since air and water have been placed in unlimited quantities at the disposal of animals, there are three great objects of desire, the exploitation of which has brought about changes in the form of some animals: I sexual gratification, II hunger, and III safety. The desire for the exclusive possession of a female was the one great requirement in the animal world which resulted in the development of weapons with which to combat opponents. Thus, for instance, the shield-like hide on the shoulders of the boar is only a weapon of defense against animals of the same species, and was thrust upwards in a slanting direction. The tusks, also, are for no other purpose than for defense, since the boar is not a carnivorous animal. Thus the horns of a buck are sharp at the extreme end so as to wound his antagonist; they are also branched in order to ward off the blows of the antagonist who is equipped with the same weapon, and hence are intended to oppose other bucks who would dispute the exclusive possession of the female, since the female, like the ladies of the time when knighthood was in flower, follow the standard of the victor. Birds, which do not gather food for their young and hence do not marry, are equipped with spurs for the fight for possession of the female, e. g., cocks and quails. It is certain that these weapons were not given them in defense against their enemies, since the females of the same species are devoid of such equipment. The prime object of this contention among the males of a species is apparently for the improvement of the species by having the strongest and most active specimens reproduce."

This principle of natural selection, upon which, later, the grandson placed so much importance, was printed with spaced letters for the purpose of unequivocal emphasis, and thus to the Grandfather belongs the honor for having conceived the idea.

Erasmus Darwin continues in his classical discussion as follows: "Another great requirement of the animal world is a means by which food may be acquired; and as a result of this requirement, the animal species have undergone changes. Thus, the nose of the pig has become hardened to enable him to root up the ground in quest of insects and roots. The trunk of the elephant is a prolongation of the nose, adapted to breaking down treebranches for sustenance and to drawing up water without the necessity of flexing the knees. Animals of prey have stronger snouts and claws. Cattle have a coarse tongue and similar gums so as to better strip off grass. Some birds have hard beaks in order to fracture nuts, as for instance the parrot, while others also have hard beaks so as to crush seeds, as with the sparrow. Other birds have long beaks in order to pierce marshy ground in quest of insects. All these things seem to have developed in the course of many generations owing to the continual endeavor on the part of the animals to procure food; and in this way these characteristics have been passed on from generation to generation in ever increasing perfection."

In his treatment of the provisions which nature has made for the gratification of the third great desire, the desire for safety, Darwin states that some animals are equipped with wings in place of outer limbs in order to escape from their enemies, while others have large and long fins or membranes, as with flying fish and the bat. Others, as for instance the hare, acquired fleetness of foot, while others again developed hard or quilly shells, as is the case with the turtle or sea-urchin.

Finally he closes with the following characteristic résumé: "If one contemplates after all this, the great similarity in structure of the warm-blooded animals, thinks of the great changes which they undergo before and after birth, bears in mind that some of these changes took place in a small period of time, would it be too daring to assume that in this great lapse of time since the beginning of the earth's existence, perhaps millions of æons before the history of man begins, all warmblooded animals have sprung from a single living filament which was supplied by the First Great Cause with animal life, and with the power to obtain new parts, accompanied with new yearnings, led by excitations, feeling, will, and which thus through unceasing activity, came to possess the power to improve itself and to pass on to posterity this improvement?"

The quotations show sufficiently that not only the general trend of thought of the grandson Charles, but even special views and modes of reasoning may be recognized in the works of Erasmus Darwin. Are we here to believe that it was a play of chance, or are we much rather to recognize the influence of the mysterious chain of inheritance which makes itself evident in the third generation; or recognize that the remarkable phenomenon which we call Atavism has asserted itself,—according to which theory, as is known, the qualities of a father remain more or less latent but come out in the grandson with all the more power?

We have an unquestioned right to interpret the life and the evolution of Charles Darwin from points of view which lie within the sphere of the evolutionary theory and we propose to make use of this right. It surely was not a matter of indifference as to the final accomplishment of Charles Darwin, that he had an advantage in descending from a clear-thinking energetic father and from a philosophic, highly gifted grandfather. The astonishing mental energy which was crystallized in the grandson is surely only to be explained by the operation of the principle of heredity.

"If one reviews," says Professor Victor Carus, the distinguished scientist and translator of the numerous works of Darwin, "the whole line of ancestors of the famous scientist, a very fortunate development in the power of observation and speculation, particularly from the third from the last generation on, becomes evident. But not only these mental qualities have been inherited in a conclusively demonstrable manner, but also splendid traits of character and a predisposition to the finest promptings of the heart show an ever increasing accentuation in the succeeding generations."

After this review of the immediate ancestors of Charles Darwin, we now return to Charles Darwin himself. When a boy, he attended the public school of Shrewsburg. At 16 (1825) he was sent to Edinburgh University to take up medicine as his father and grandfather had done before him. According to a later admission of his, the dissection of bodies was so repugnant to him, that he recalled having only once visited the dissecting room, where the students worked. After two years, Darwin left Edinburgh and went to Cambridge where he—it will be astonishing to learn—made arrangements to take up the study of theology and prepare for the ministry. Fortunately the young man attracted on one occasion the attention of Professor S. Henslow, who was delivering lectures in Cambridge on botany and geology. Darwin was in the beginning not drawn to the latter so much by his great learning as by his well-meaning, sincere, and unassuming character. Gradually, however, and particularly through botanical excursions with Professor Henslow, a taste for a serious contemplation of nature was awakened in Darwin, and along with it a joy in making collections, observations, and comparisons. It did not take long before he discovered a new species of insect in the marshes on the outskirts of Cambridge. From this time on he devoted himself to research work; he ceased gradually to be merely a chaser after specimens and an animal lover who takes interest only in dogs, foxes, and partridges. Upon good authority it is asserted that Darwin, before making the acquaintance of Professor Henslow, was an enthusiastic lover of hunting, for which sport he was naturally adapted because of his robust constitution which could cope with all hardships. Through Henslow he also took up geology and it was in this department of science that he later reaped his first scientific laurels. Thus Darwin lived, dreaming, searching and observing quietly until the year 1831, a year which became decisive as regards his future intellectual development.

In the summer of this year the twenty-two year old scientist was invited by the Commander of the Gunboat "Beagle," Captain Fitzroy, to join a surveying expedition on this ship as naturalist, the trip to extend to various parts of the world and to last five years. Darwin, who had already intended going to the Canary Islands on his own responsibility, did not hesitate a moment; he accepted the offer of the Captain and made only this condition, that all specimens collected by him, such as animals, plants, minerals, were to remain in his possession. In return he waived all salary or remuneration. The English Admiralty agreed to Darwin's counter suggestion, and thus nothing stood in the way of his participation in the expedition. On the 27th of December, 1831, the "Beagle" set sail from Devonport. From this time on, the map of the world ceased to be a mere white card with colored lines; it became a painting of the most varied and animated figures. At such times, when he was not suffering from seasickness, he took up the study and the observation of nature. A moonlit night with a clear sky and the dark phosphorescent sea filled his being with exaltation. Even the severest storm with its mountainous billows did not frighten him. The ship took its course to the southwest; passing the Cape Verde Islands it headed for South America, running along the coast to Tierra del Fuego and the Falkland Islands.

The Chiloe and Galapago Archipelagoes were also visited. Then Captain Fitzroy steered to Otahaiti, Australia, New Zealand, and Vandiemens Land. He also anchored at Mauritius, St. Helena, and the Azore Islands. Five years the "Beagle" went on its way. A long trip such as this afforded the young scientist an opportunity to become familiar with all that is worth seeing and knowing in this world.

The first great impression which inspired Darwin with enthusiasm for the tropics came to him on the 29th of February, 1832, in the vicinity of Bahia. "This day,"—thus states an entry in his travel-diary, "was a happy day for me. A naturalist must feel happy when he wanders for the first time in a Brazilian primeval forest. Among the many striking objects, the uniform richness of the vegetation deserves the greatest praise. The grace of the grasses, the novelty of the parasite plants, the beauty of the flowers, the dark green of the foliage—all these things add to the effect; a most remarkable combination of sound and silence dominates the shady portions of the forest. The chirping of the insects is so loud that it can be heard aboard a ship which lies at anchor a considerable distance from the shore, and yet in the solitude of the forest, there is an ever present silence. For anyone who derives pleasure from natural history, such a day affords more enjoyment than he can ever again hope to find."

On the 1st of August, 1834, the "Beagle" again anchored at Bahia and Darwin obtained another enjoyable and instructive view of the tropical forest. Literally speaking, he could not appease his hunger to see. He calls Brazil "a large, wild, luxuriant hot-house, which nature created for its own purposes, but of which man took possession and filled with neat houses and orderly gardens."

The "Beagle" expedition marks in every way a complete change in Darwin's life. He who formerly had not been accustomed to methodical, intellectual work, who scarcely knew how to operate a microscope, had developed into a conscientious observer, analyst and sketch artist. The deficiencies in knowledge and ability which had before been so apparent, were overcome during this time. On the 2nd of October, 1836, he again trod on English soil at Falmouth. The results of this lengthy trip have been recorded in an extensive work which was published in five volumes (1840–1843) by Charles Darwin, in conjunction with a number of savants of the first rank. The title of the work is: The Zoölogy of the voyage of H.M.S. "Beagle," under the command of Captain Fitzroy, during the years 1832 to 1836. Published with the approval of the Lord Commissioners of H. M. Treasury. Edited and superintended by Charles Darwin, Naturalist to the Expedition.

The various volumes contain in natural order the following:

Vol. I. Fossil mammals, by R. Owen, with a zoölogical introduction by Charles Darwin.
Vol. II. Mammals, by G. R. Waterhouse, with a geographic introduction by Charles Darwin.
Vol. III. Birds by J. Gould, with notes regarding the mode of life and distribution by Charles Darwin.
Vol. IV. Fishes, by L. Jenyns, with notes by Charles Darwin.
Vol. V. Reptiles, by Thomas Bell, with notes by Charles Darwin.

The English Government granted a thousand pounds sterling to help defray the cost of publication of this work, and Darwin himself made extensive pecuniary sacrifices to finish the printing of the volumes. Finally, the magnificent work was on sale, and aroused the greatest attention in the scientific world. Their production had required three full years.

In 1839 Darwin went to Maer Hall in Staffordshire, to the home of his Uncle Wedgwood. There he became acquainted with his cousin Emma, a beautiful and intellectual young woman, for whom he developed an irresistible affection. Indeed, he married her in the same year, and after living for some time in London, he retired with his young wife in 1842 to an estate in the County of Kent, where he could devote himself to his studies and to his health. Here in the quiet little hamlet of Down, Darwin was for some years active only as a private tutor; later he assumed the rôle of magistrate, in which position he had, to be sure, sufficient time at his disposal for numerous biologic investigations and breeding experiments. The results, as everyone knows, were incorporated in a series of epoch-making works. Twenty years Darwin worked in seclusion for himself; he wished first to be convinced of the truth of his idea, before he gave out to the public any information concerning it. What was this idea? The answer cannot be framed into two words. From Darwin's own report, we know that he acquired during his "Beagle" trip many facts which could not be explained scientifically according to the prevalent notion that each zoölogic or botanical species springs into existence independently of the others. Along with these facts must be reckoned the geologic sequence of organisms, their propagation in former times and now, and also the many similarities which they present among themselves.

Darwin appreciated the difficulty of setting up a satisfactory hypothesis for the explanation of these facts and with the following words he describes what took place in his mind: "When I visited the Galapago-Archipelago (which is located in the Pacific Ocean about 500 English miles from the South American coast) during the trip of the 'Beagle,' I found myself surrounded by peculiar species of birds, reptiles, and snakes, which existed nowhere else in the world. And yet almost all displayed American characteristics. In the song of the mocking bird, in the sharp call of the carrion vulture, in the large opuntia growing in the shape of a chandelier, I recognized plainly the proximity of America, and yet these islands were separated by many miles from the mainland and differed greatly from it in their geologic constitution and in their climate. Still more striking was the fact that most of the inhabitants of the various islands of this little Archipelago differed specifically from each other, although closely related to each other. I have frequently asked myself the question, how these peculiar animals and plants have sprung into existence. The simplest answer seemed to be that the natives of the various islands descended from one another and in the course of their descent underwent modifications; and that all inhabitants of the Archipelago have sprung from the nearest mainland, viz., America, from which colonization would most naturally have come. But for me there remained for a long time an inexplicable problem: how the necessary degree of modification was attained."

Darwin further on relates the fact that he found in South America (in the vicinity of Montevideo) large pieces of bony plated armor which, except for size, differed little from that of present-day armadillos. This fact also, he asserted, contributed to forming the opinion that the fossil forms are related to the still existing forms by bands of blood relationship. Darwin saw clearly that the innumerable former inhabitants of the world and those still existing are mutually connected by the most peculiar and most complicated relationships, and that they can be, similar to varieties under species (or subvarieties under varieties) classified into groups, but with much higher degree of differentiation and subordinate to other groups. Everything pointed to a common descent from a few primal forms, although with manifold modifications. But for the time being, that was as far as he went. Darwin was not able, up to the time of his return to England, to fathom the "Why," the nature, and the manner of such a modification. Only after the lapse of a few years, when he had made numerous breeding experiments with pigeons, poultry, dogs, and rabbits and had studied the effect of artificial selection on products of domestication, did he arrive at a clear conception of the extent of the variation that descendants of carefully bred animals possess from their parents. With cultivated plants of all varieties he worked in the same manner and, according to his own admission, these experiments were responsible for giving him a clear idea of the plasticity of organisms; in other words, of the possibility of modification of their parts or of the whole structure. In the same manner Darwin came to realize that this proneness to change which we encounter throughout our cultural products, cannot be brought about by any direct means through the agency of man. We are not able to create varieties nor can we prevent their springing into existence. The only thing we can do is to preserve those already present and to increase them. Man unconsciously exposes organisms to new and changing living requirements and the modifications make their appearance. The real cause of their inception, however, remains a mystery to him.

Darwin for many years concentrated on this line of thought. He could not progress one way or another along the path which he was now pursuing. At last, however, came the impulse from without which gave a new direction to his investigations and with one blow revealed the real cause of the processes of formation of varieties.

It was the perusal of the famous work of Thomas Robert Malthus on the law of population which directed Darwin to his principle of natural selection. Concerning this work, which aroused in the first decades of our century the greatest sensation, there were spread not only then, but even now, the most erroneous views, and the English Ministry—which decried as heretical the discussion of the law of population and placed a ban on the work—must be held responsible for the fact that the world is not as familiar as it should be with a book which offers evidence of that which human acumen in conjunction with rich stores of knowledge can do towards the discussion of a difficult problem.

Darwin, to whom the general condemnation was just one more reason for taking Malthus' book in his hand, was astonished to find right on the first pages of the book, the passage which was in complete agreement with his own views: "Throughout the animal and vegetable kingdoms, nature has strewn with prodigal and generous hand the seeds of life; but she was comparatively miserly with room and the required nutriment with which to support them. If the germs of existence could unrestrainedly develop, in the course of a few thousand years they could fill millions of worlds. Want, that dictatorial all-permeating law of nature, holds them within prescribed boundaries. Plant and animal species contract under this great restraining law and man can not escape from it, however he may strive to do so. Plants and unintelligent animals are driven on by a mighty instinct to enlarge their kind, and their instinct is not held back by any regard for the progeny. Where freedom therefore exists, the reproductive instinct is practiced, and the excessive effects are later on checked through want of room and food."

According to the English ministry, which would have preferred to destroy the essay on population by fire, the teachings of Malthus involved a degradation of the Creator. For to assume that God could not feed the creatures He had brought into the world but would have to allow them to perish from scarcity of food was, it was argued, a serious attack on the benevolence and omniscience of the Supreme Being.

Darwin discovered, to the contrary, that Malthus' description of the events of nature's household were exteremely rational and contained an explanation of just how an animal or plant species could develop to a higher degree and to greater perfection in the course of many generations.

Like a flash of lightning the thought came upon him that next to artificial selection there must also exist a natural selection. For is it anything else than an act of unconscious selective propagation when, owing to excessive multiplicity of forms and the resulting rivalry for means of subsistence, the weakest specimens or those that cannot adapt themselves to new conditions pass out of existence?

If, as Darwin has unquestionably determined, organisms variegate spontaneously in all directions, then necessarily some varieties must appear which are very adaptable to changed living conditions. These altered individuals of an animal or plant species will be those whose prospects for surviving in this universal competition are the best and since the progeny is usually like its progenitors, a very great degree of probability will exist that the favored varieties transmit their superiority to their descendants. By means of this process, which Herbert Spencer has termed "the survival of the fittest," if it has been going on for many generations, new organic forms must necessarily come into existence which forms will surpass in some way their progenitors. In consequence of this natural selection brought on through competition or selective breeding, the new and more adaptable forms must constantly increase in number and become diffused over a wider area. The final result must be that the original form will gradually decrease in numbers until it disappears entirely. At large intervals then, the earth is populated with new organisms without a real break in this chain of reproduction. The fossil remains, which we find in the sedimentary strata, are the remains of creatures which perished because they were unable to adapt themselves to new living conditions. If paleontologic information were not so incomplete, so full of voids, as is unfortunately the case, we would be in a position to trace the descent of present-day organic forms into the earliest epochs of the world's history. All these forms must be regarded as lineal descendants of such forms as existed long before the siluric epoch. Never has the line of succession of one generation after the other been broken, never has a universal flood destroyed the world. All now extant animal and plant varieties must be regarded as the result of slowly operating and still existing forces, not through marvelous acts of creation and violent catastrophes.

This is Darwin's theory, which has been so frequently misunderstood and antagonized. It is the result of investigation and thought extending over many years, upon the most difficult problem which mankind has had to face. The path to a solution of this problem led through a mountain of prejudices. It was regarded as a fixed dogma that every genus was indebted to some act of creation for its existence. By means of the almighty "Let there be," of the Godhead, it was believed that at various times in the history of the earth, certain elementary atoms were commanded to form a union for the purpose of creating living forms. Such creatures were considered absolutely invariable—the development of the individual from the egg to maturity was the only thing known; the question of development of species merely dawned in the minds of a few distinguished persons but aroused no general interest. When Erasmus Darwin published his sketch of a theory of descent in the second volume of his "Zoonomy," no one regarded it as anything but a philosophic caprice which was overlooked in deference to the author of the "Botanic Garden," receiving no recognition whatsoever. That was the situation at the close of the past century and at the beginning of the present (19th). In 1809 appeared "Philosophie Zoologique" of the Frenchman Lamarck, a very important work which contained an outline of a true history of development of animals and plants. Fifty years it was passed over in silence and received recognition again when Darwin's stirring book on the "Origin of Species," which appeared in the fall of 1859, demonstrated at once the real existence and the incisive importance of this problem of the species.

The unequalled literary success which met Darwin's book was due to the thoroughness of his investigation, the force of his arguments, and the overpowering quantity of facts which he set forth to establish his reasoning. He who has read and comprehended the work on the Origin of Species will not do otherwise than agree with the deductions which it offers. There is not a doubt which can be harbored nor an objection which can be interposed which has not been raised by Darwin himself and brilliantly met, if one is willing to carefully study his work. The boundless objectivity and honesty with which the English savant presents his discussion must arouse astonishment and in consequence, the most sincere admiration.

Merely to state the facts, however, would not be sufficient to explain the constant and continual increase of impression which is being made by the book which has become the common property of all civilized nations.

The secret of its success is not due to the advantages which Darwin's work shares with other intellectual productions, but to the fact that it consists not merely of paper and the printing thereon, but contains a discovery which had not been made by any previous savant or natural scientist. This discovery succeeds in demonstrating a *vera causa,* a truly acceptable cause for the variation of species. No one before Darwin had recognized that the necessary outcome of the potentially geometric increase which is common to all organisms, was the competition for existence; nor had they discovered in this fact the principle of natural selection, which preserves with greater certainty than artificial selection those individuals best adapted to changing living conditions, and relegates the others to destruction.

Neither Erasmus Darwin nor Lamarck could have offered so plausible an explanation for the improvement of living forms. Both attached too much importance to the use and non-use of organs, and furthermore assumed that in these organisms there was a striving or straining which, operating from generation to generation, finally brought about modifications in the organization of the creatures so that they constantly became more improved and refined.

One feels that with this kind of paraphrasing nothing is accomplished. It will never be possible to understand the method of operation of a law of development innate to an organism; such a law would not be a result of scientific investigation, but rather an assumption based upon speculative considerations. It is not possible for the natural sciences to concern themselves with it, since these sciences are concerned only with facts.

In this proof of a real cause for the development of species may be found the invincible power of Darwin's reasoning. "He who cannot appreciate this, is beyond all help."

Among those scientists who most vehemently opposed Darwinism and fought hand and foot against its acceptance into the realm of science, one of the most distinguished was Louis Agassiz. Shortly before his death, while lecturing on the natural foundations of relationship among animals (Spring of 1873) he expressed his repugnance to the theory of evolution in the following words: "There are no church or birth records in the animal realm, nor are any genealogic trees maintained by which we could determine whether the individuals with whom we are now familiar descended from this or that ancestor. We can only compare the animals anatomically and physiologically, follow the manner and course of their individual development, observe their mode of life, determine their geographic distribution, investigate their gradual sequence in the various geologic periods with a great mass of observations and comparisons—and by collecting all the results of such investigations and observations, put the animals into family groups according to points of similarity. But to go further than that and to assert that because animals resemble each other, they necessarily must be descended one from the other, is to assert something of which we have no knowledge. Resemblance does not prove descent by right of succession."

Thus Agassiz. His statements merely prove, however, that it is possible to possess a wide store of knowledge in the spheres of zoölogy, paleontology and history of evolution, and at the same time be unable to comprehend a new theory which was propounded by another distinguished scientist.

Evidently Agassiz never clearly understood that all classification is genealogic in character and that all scientists—unwittingly—always seek a common descent when they arrange natural objects into larger or smaller groups in accordance with the degree of resemblance. In confirmation of this view, let us cite the fact that every natural historian unhesitatingly included in his first group, the species, both sexes, although the males frequently differ in their intrinsic characters from the females. The insistence on relationship in this case is undoubtedly founded on the incident of descent. Thus we include, without wavering, the various larval stages of the same individual in the species, no matter how much they differ from the mature animals, just because we know that they are related to the latter by means of genetic association. On the other hand the human intellect is always prone to create new species on the basis of apparently important differences.

Thus the orchid forms, known under the names monachanthus, myanthus and cetasetum, were formerly treated as distinct kinds. But when it was observed that they appear at times on the same plant, they were demoted to varieties. This example shows quite plainly how much arbitrariness obtains with the artificial classification of the objects of nature. Finally, it is the authority of an expert which decides whether a species is a species, not a fixed principle which could be adhered to in all cases. To what extent descent has been the guiding idea in all classification appears from the fact that the more stable characteristics, not the most essential, are selected as the basis for grouping. The English breeder Marshall says that the horns of cattle are very suitable for classification because they vary less than the form and color of the body. In the case of sheep, he did not consider them as dependable because a greater variation was observable. Stability is, as may be seen, just another word for uniformity of inheritance, and this shows plainly that descent always plays an important rôle when classification is to be considered. We are inclined to include the most dissimilar creatures in one group, as for instance the grown males and hermaphrodites of certain "cirripods," which hardly have a common characteristic, as soon as we know that they are connected organically through descent. On the other hand, we are ready at once, as intimated above, to create new species and determine genetic differences, when we wish to define organisms, of whose descent and origin we are ignorant. The history of zoölogic and botanic systematics shows that the most trivial distinctions between forms are regarded by many scientists as sufficient to elevate both to the rank of species, if they are not connected by intermediary stages.

Darwin put an end to this arbitrary procedure. If the views which have found their expression in the theory of selection will continue to prove their soundness as heretofore, a great revolution in the natural sciences will come about. Those engaged in systematizing will then cease to quarrel as to whether or not an object before them represents a species. It will simply have to be acknowledged that the only difference between species and varieties consists in that the latter are to this day still connected by intermediary grades whereas the species were formerly so connected.[1]

Much that was incomprehensible formerly we can now understand with the help of the theory which has made Darwin's name immortal. In the light of the teaching of Agassiz concerning the acts of creation, how mysterious is the similarity of structure between a human hand, the foot of a dog, the wing of a bat, the fin of a seal, and how easily explainable, according to the principle of natural selection, as small sequential variations which appeared in the descendants of a single progenitor.

Within the limits of this exposition it is impossible to discuss all the arguments which speak for or against the Darwinian theory of selection. The famous investigator has done this himself in a classical manner; just read the closing chapter of his work on the Origin of Species and ask yourself if the subject could have been discussed in a more careful and more comprehensive manner, than was done here.

In the same chapter Darwin makes this prediction for natural history, which the influence of his theory has elevated to the science of biology: "A large and almost untrodden field will reveal itself for investigations concerning the causes and laws of variation, concerning reciprocal relationship, concerning the consequences of use and non-use of the organs of the body, concerning the influence of external living conditions, etc. The study of cultural products will increase immensely in importance. A variety brought out by human agency will become a more important and a more fascinating object of study than the multiplication of already existing species of our systems with a new one. Our classifications will, so far as this is possible, develop into genealogies and then first reveal the so-called "Plan of Creation." Rudimentary organs will speak with infallible dependability of forms, long obsolete. Embryology will reveal to us the partially hidden development of prototypes of each of the main groups of the system." [2]

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[1] A very unequal opponent to Darwin made his appearance in the person of M. Renooz, a Frenchman, who published a book with the title "Histoire des Animaux," which purports to be a *"Nouvelle theorie de l'Evolution"* and antagonizes Darwin's theory with silly objections. It was brought out at Paris by Baillière and Son, consisting of several volumes.

With these words Darwin has given us his scientific testament. Everywhere the doctrine of the force of development has put the intellects into motion, has clarified ways of thinking and has transformed a belief in the efficacy of irrevocable laws of nature even in the organic world, into the religion of the century.

Many a one who now repudiates the new point of view will some day come forward as did Agassiz and, like him, confess: "I must admit that I was not prepared to see this theory accepted to the extent that is the case and by the best intellects of our time." This remark of the intellectually most important opponent of Darwin was uttered at the time on the estate of Mr. Winthrop at Brooklyn in the presence of the English physicist Tyndall. The latter has attended to its universal circulation.

In lay circles it was formerly assumed that there were serious objections to the theory of selection, and the opposition which Agassiz fostered unremittingly against it was adapted to lend assistance to such an assumption. In the meantime, however, Carl Gegenbaur, one of the most prominent authorities in the sphere of comparative anatomy, had declared that in his department of science no experience exists which could contradict the theory of evolution; quite the contrary, that everything points to it. And the famous geologist Bernhard von Cotta, before his demise, had also broken a lance for Darwin by saying (Geology of the Present, 1872, s. 265): "From the geologic point of view, the theory of selection is incontrovertible; in truth geology offers numerous facts for its corroboration."

But just the same, Darwinism remains mainly a scientific theory and those who strive to elevate it to a world philosophy and who attribute to it a revolutionary influence upon our religious and ethical life, assign to it a rôle which, in the present phase of its existence, it should not be called upon to play. Virchow, on the occasion of the convention of German anthropologists in August, 1881, at Frankfurt, rightly remarked that everything possible which does not really belong there, has been placed under the concept "Darwinism."

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[2] "The Origin of Species."

There are such pseudo-Darwinists in large numbers, and when one listens to the perorations of some ultra-Darwinists, one is almost inclined to believe that the sacred image of Sais * has been completely unveiled, that nature holds no longer a secret which cannot be unraveled by the principles of inheritance and adaptation. These wild enthusiasts might be recommended and urged to frequently recall Darwin's words, "We do not know how ignorant we are." It never occurred to the great teacher to attribute to his scientific principles a potency which, because of the very nature of the case, they could never acquire. Darwin was to the highest degree sincere, cautious, and modest. All extravagance was repugnant to him. In those qualities which distinguish the dreamer and enthusiast, Darwin was wanting.

At various places in his works he expressly emphasized the statement that the theory of evolution does not aspire to explain the origin of life on this earth. He regards all speculation on this matter as futile. Every thoughtful scientist must agree with him on this point. Not alone the very beginning of life, but also the causes which have developed and diffused it and which still operate, are completely hidden from us. We are, to be sure, constantly using the term "Development" and employing it in all departments of natural history, but with respect to the nature of the same, we are still completely in the dark. With the present state of our knowledge, we are not even in a position to advance a hypothesis in explanation of the phenomena of evolution. The entire process of evolution is the expression of a force, which is absolutely beyond our comprehension.

If we say: The first species sprang from a primal cell by means of "generatio acquivoca," that is, parentless germination (from combinations of inorganic matter) nothing really has been said. For what do we know about matter, except that it reacts on our senses and undergoes manifold changes?

Notwithstanding, we must not resort to a belief in miracles and erroneously conclude that because of the confessed admission of our present ignorance and because of the narrowness of our mental vision, it is immaterial whether we continue our researches or not. That would be a fatal mistake from which we must carefully refrain. No! the scientist must have faith in his ability to grasp cosmic and earthly phenomena and see how far he can, on this assumption, proceed with his intellect. He

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* *"Das verschleierte Bild zu Sais,"* Poem by Schiller.

must not believe that he can substitute science for religion. The object of religion, the First Cause, or God, cannot be treated in a scientific manner, nor determined by measure. It remains exactly where it is: an infinite distance from the means which are at our disposal for the comprehension of that which exists; only the assurance that there is a First Cause which is still active has maintained itself through history. The opponents of the theory of evolution who are veritably smitten with a fever when they hear the name of Darwin, exhibit plainly by their passionate conduct for which there is no reason, that they have never read a line of the great investigator. Darwin presents his theory with such modesty, that no one could be affected unpleasantly.

In the foregoing chapter we have spoken only about the investigator Darwin. It remains for us to say a few words about the noble qualities of character, the gentleness and loving heart of the master. In the bright, honest gaze of Darwin one could read that his soul was free from all deceit. As a faithful husband and a devoted father of a large family, he furnished proof that his views on the operation of natural laws was compatible with the development of high moral qualities. To the contrary let those who refer to Darwin's doctrine as materialistic and atheistic ponder over their own conception of the Godhead and their conduct—whether it is as clean and devoid of blame as was that of Darwin. The famous investigator led in Down a quiet, exceedingly well-ordered life and only thus is it possible to explain that his body, weakened by lengthy ocean trips, was able to last until his seventy-third year. Darwin was large of stature, broad-shouldered and quiet, serious, and dignified in manner. He had the appearance of an English country gentleman, not of a savant. All such who were personally near to him have affirmed that he was possessed of a benevolence ready for big sacrifices, of genuine devoutness, and of great leniency to the failings of his fellowmen.

MODERN CRITIQUE OF THE THEORY OF EVOLUTION

In 1866 Ernst Haeckel announced the doctrine that the development of the single individual from the ovum, autogenesis, was an abbreviated recapitulation of the evolution of the stem or family, phylogenesis. That means, it pointed out the way on which the species originated in the course of uncountable generations. This he termed the *biogentic law.* It is not generally pointed out by geneticists that this conception was not original with Haeckel, but was announced in 1793 by Karl Friedrich Kielmeyer, to whom I have referred as having been the instructor of Goethe and Schiller, and also of George Leopold Cuvier, the comparative anatomist acknowledged that Kielmeyer gave him the data from which his comparative anat. researches started.[3] Johannes Müller was also a pupil of Kielmeyer. I have referred to this in the article on "Goethe as a Naturalist," Chap. XIX. Fritz Müller in 1863 expressed similar views concerning the Nauplius as the phylogenetic common form of all crustaceans. Historically, therefore, the so-called biogenetic law of Haeckel, has quite a history before Haeckel's time.

The view that the higher animals repeat in their development the adult stages of lower animals and that this is the embryonic evidence of the evolution of higher living forms, from lower forms, furnished the inspiration to the school of descriptive embryology for the last generation, and it is still contained in nearly all contemporaneous text-books of biology. Dr. Edward C. Jeffrey in an article entitled, "The present Status of the Biogenetic Law," Science, December 12, 1924, page 531, speaks of it also as the doctrine of recapitulation, and the chief aim of his article is to defend it as one of the principle cornerstones of the theory of organic evolution. To accomplish this, he resorts to paleobotany, and emphasizes that the doctrine of recapitulation is admirably illustrated by the still living genus, Pinus. There is nothing that was not more or less generally known in the article of Professor Jeffrey, for that the younger stages of certain plants should be more like ancient plants that have lived in ages long past, is no more striking than that the tadpole, the precursory stage to the adult frog, to-day has gills, just as its adult ancestors had in the days gone by. The difficulty in taking such an extremely confirmative view of the biogenetic law is the fact of mutations. New species can undoubtedly originate all at once by what has been termed "saltation," or sudden change. New plants have been produced experimentally in botanic laboratories (Hugo de Vries). The past history of such species nevertheless continues of great importance phylogenetically wherever it can be traced with this restriction, that the possibility of such a past being recorded in the developmental stages of animals becomes problematical and of doubtful interpretation.

Thomas Hunt Morgan in his book "A Critique of the Theory of Evolution," looks at this phenomenon from a different standpoint. He calls attention to the fact that there are a great many embryonic stages which cannot possibly represent ancestral animals. Among these are the hundreds of adaptations and falsifications of ancestral records, and he reviews the four great historical hypotheses concerning evolution. The one is by Geoffroy St. Hilaire, who was a disciple of Buffon and whose conceptions of evolution are at the base of our thinking to-day as far as the interaction between organism and environment is concerned. According to Bergson, the principle feature of living protoplasm is that it responds to the situation in which it finds itself, in as favorable a manner as is possible, and evolution has followed no preordained plan. It has brought about its own existence by ceaseless adaptation to each new situation in the environment as it arose.

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[3] See A. Moll, Die Medicinisdle Facultäl d. Carlsakademie in Stuttgart, 1859.

The second theory is that of Lamarck. It is the biologic, carrying into effect the old proverb that practice makes perfect. Any organ, tissue, or even cell becomes by use a better instrument for performing its task. But through disease it degenerates. The fact that many outstanding types of behavior had been acquired through continued association, formed by contact with the outside, does not contradict this doctrine.

The third principle is that of Bateson and Nägeli. Nägeli thought that in all living protoplasm there is an inherent potency for growth and expansion, but the further development of his ideas is too hypothetical for discussion in this book. He does not for instance define whether this potency for growth is due to chemical or physical constitution of the living thing that is growing, or whether it is a purely mechanical principle. Bateson's views which are classified with Nägeli's have in my opinion no resemblance to them whatever. According to Bateson, evolution has been due to the continuous loss of inhibiting factors. Living matter was arrested by many checking forces at the very beginning of the living world. As these various restricting and checking factors were lost, new things began to be evolved.

The fourth theory we have to narrate in this connection is that of Darwin and Wallace, which is also dimly announced by Goethe and known as the doctrine of natural selection. It is well known that Darwin demonstrated that the material on which natural selection works was furnished by so-called *chance variation.* By chance he did not mean that these variations were without cause. He preferred to teach that the particular combination of cause which resulted in these chance variations was for the time being not known. Darwin was himself searching for the physical causes of variation, and held out the promise that they would eventually be found in natural processes.

In the second chapter of Morgan's book (l. c.) he presents a lucid explanation of Mendel's work in furnishing the experimental proofs that heredity could be explained by a simple mechanism. Mendel was experimenting between 1857 and 1865 with plant hybrids. He published his results in the Transaction of the Nature Society of the city of Brünn in 1865, establishing three laws, first the dominance rule, that is, of the characteristics of the dominants and recessives; secondly, the rule of division or splitting up of the germ cells of the parents' *Anlagen* into their components when bastard plants are developed; third, the law of the independence of the characters that have been divided. Mendel's work was really not known until 1900 when Tschermack, Correns and H. de Vries placed them in the right light. This long concealment of Mendel's epoch-making discoveries prevented Darwin from making use of them. The laws of Mendel have been designated by different terms since his first publication. The second law is spoken of as *segregation,* and the third as that of independent *assortment.* For these special definitions I refer those interested to special works on genetics and evolution.

To return once more to the controversy concerning the relative importance of the biogenetic law or recapitulation and the doctrine of mutation, —after Weismann had urged that variations are germinal, that is, they first appear in the ovum and the sperm, as changes that later produce modifications in the individual, it was made clear how the offspring of a pair of animals were not affected by the structure or function of their parents, but that the germ-plasm was the identical stream from which both the parent and the young have arisen. Next it was discovered that changes in the germ-plasm may produce a change in the development of the next generation of individuals, not only from the beginning of the life of that next generation, but at any stage of its development. New characters, therefore, need not necessarily be conceived as the continuum attached to the end of an old series. Bateson and de Vries have advanced the idea that variations are discontinuous. Experience with this so-called discontinuous variation explains the sudden changes in the characters of new generations and attributes them to changes in the germ-plasm that have remained latent or potential and can crop out at any time in the life of an individual of the next generation. The new characters do not add themselves to the end of an already existing series of characters, but they can affect the structure and function of adult individuals, without, as it were, passing beyond and through them. The biogenetic law has received a severe shock through the views of Bateson and de Vries. It is for this reason probably that Morgan interprets the embryologic evidence on which Kielmeyer and Haeckel developed this law, in a different manner. The gill arches in the embryo of any mammal according to Morgan, do not indicate, as has been supposed, an aquatic habit on part of the ancestral forms from which on the basis of paleontological and evolutional evidence the warm-blooded animals are presumed to have come, but he suggests that the presence of gill arches in the mammalian embryo is a response to the amniotic fluid in which the embryo must exist. In other words, the so-called vestiges of a former state are not true vestiges but represent merely a contemporaneous larval adaptation of the embryo to a temporary aquatic existence.

These newer expansions of Darwin's evolutional hypothesis prove that such a momentous addition to human knowledge cannot be brought out in a complete and perfect form, at once the idea which Ostwald attributes to the work of such great men as he designates as *Klassiker*. It is only in rare instances that the work of a great man receives no additions.

It has happened that a new conception of a great Master Mind breaks in on an unprepared generation that contains no minds capable of comprehending the new great thought. This happened to the "phase law" of Willard Gibbs of New Haven, which did not break even into the attention of scientific men until ten years after he announced it, and only then very slowly. The theory of the equilibrium of all chemical conditions, which is ascribed to Helmholtz, had already been announced before him by J. Willard Gibbs, but although Helmholtz did not know of Gibbs' work, he confirmed it in all particulars. Gibbs is our foremost scientific American *Klassiker,* and his work was handed down so complete that even Helmholtz could not add to it. There can be no doubt that judged from the thoroughness of his work, Willard Gibbs is the highest scientific genius that the United States has produced.

We can reach the conclusions concerning the expansion and extensions of Darwin's work at the present day, that the characters of living things are inherited according to Mendel's laws, that mutations may give rise to new characters in a way which is still not understood, and that evolution takes place by embodying into the race those mutations that are beneficial to the life and reproduction of the organism. The other great factor is what Darwin called the process of natural selection, or Spencer called the survival of the fittest. By this is meant that those individuals that are best equipped to cope with their environment are most likely to survive and leave offspring, and in the course of time their descendants will replace the less adaptable individuals of the species by competition.

Now the increase in the number of best fitted individuals in a species means that further advantageous results will ensue to the race which are not included in such a simple definition, but have been referred to in previous chapters under different names.

SUPPLEMENT

In his intensely interesting work on the "Origin and Evolution of Life" (pages 138–139), Henry Fairfield Osborn attributes the first observations of how new specific forms arise to Wilhelm Heinrich Waagen (1841–1900) an Austrian paleontologist and stratigraphic geologist. We must call attention to the fact also that the term *mutation* was originally used in its correct bearing on evolution by Waagen in 1869 (not by de Vries). In 1889 Neumayr—another Austrian paleontologist—described a tendency of form of living things, to evolve in certain definite directions under the designation *Mutations-richtung.*

The publication of Wilhelm H. Waagen's "Die Formenreihe des Ammonites radiatus" (1869), marked a classical biologic event, for in it he demonstrated for the first time how new specific forms slowly arose in the past. Among the fossil Ammonites of the Jurassic period which are to-day represented by the pearly nautilus he observed and described the actual mode of transformation of one animal form into another. Through this so-called mutation of Waagen, he established for the first time, the law of minute inconspicuous changes of form, which accumulate so gradually and slowly that they are observable only after the passage of long periods of time, sometimes thousands of years. The term *Mutations-richtung* means that these minute changes which would be imperceptible if judged by short time spaces, take a definite direction. According to Henry Fairfield Osborn ("The Origin and Evolution of Life," page 138) this essential law discovered by Waagen is one of the most important in the whole history of Biology, and Osborn has subsequently demonstrated that by it new characters arise definitely and continuously and above all adaptively.

In the preceding I have referred to mutation in relation to the so-called biogenetic law of Haeckel and in that case the word *mutation* was used in a different sense—namely the sense in which the great botanist Hugo de Vries used it in 1901. As Waagen had introduced this term 32 years before de Vries and in a different sense, it becomes necessary to distinguish the two different meanings which these two brilliant pioneers attach to this term.

Waagen (1869) meant by mutation an extremely slow but definitely directed continuous adaptive change, so minute and inconspicuous that geologic time periods are required for their recognition. We now know that they represent a true evolution of the hereditary chromatin and this very gradual evolution of adaptive form is in Osborn's opinion contrary to Darwin's principle of the selection of chance variations. ("Origin and Evolution of Life," p. 140.)

The paleontologic studies of Waagen, which Osborn states are contrary to the Darwinian principle of chance variation, receive a different and profounder interpretation by H. S. Jennings ("Observed Changes in

Hereditary Characters in Relation to Evolution." *Journ. of the Washington Academy of Sciences,* Vol. vii, No. 10, May 19, 1917 with important bibliography on p. 300 and 301).

Osborn presents an exposition of the origin of new characters from minutest beginnings, when given stocks are traced from earliest to later stages and describes how these characters pass by continuous gradations to the highly developed forms or types. Thus far this is apparently in agreement with experimental results of other workers referred to in the article of Jennings.

But when Osborn further endeavors to affirm that these developing characters do not show random variations in all directions but follow a definite course which might seem to have been in some way predetermined, he meets with opinions and experimental results by other eminent biologists that are not confirmatory of his views.

It would appear that there must be some directing tendency, some inner necessity which compels a developing organism to follow a definite course. The term orthogenesis has been assigned to this phenomenon. According to H. S. Jennings variations do not follow in one determinate direction only and he brings out the evidence that by selection we can move in more than one direction, and emphasizes that we do find that diverse courses are followed by given characters, in diverse branches of a given group, and that this is particularly true of the characters of shape and proportion and at times also of structural and numerical characters.

Very instructive is the argument of Jennings when he points out that there are two kinds of character variations in given geologic strata and that Osborn in agreement with Waagen, describes them as first, persistent changes, and secondly, ephemeral variations living only a short while. The first are in the line of determinate descent—the second are variations from this line constituting only transient varieties, like the diverse races of *Difflugia corona* which Jennings has described.

Now Waagen and Osborn claim there is a great difference between these two character changes, namely those which are in a determinate line persist into the next geologic stratum, whilst the transient varieties disappear. The persistent changes Waagen called mutations (1869).

Whilst Osborn considers the second type of varieties non-heritable modifications, Jennings emphasizes that in our present geologic period we find just such divergent forms in great number and find also that their peculiarities are heritable. There is therefore no reason for supposing that these variations were not heritable in earlier geologic periods. There must have been many races heritably diverse, just as there are now, and these are what Waagen calls varieties.

This being so, the only difference between Waagen's mutations and his varieties is that the former persisted and the latter did not. But according to Jennings, this tells us nothing as to why the latter did not persist and he declares it quite possible, that a process of natural selection by external conditions must have been operating when many diverse stocks existed, some of which persisted, while others were destroyed.

This conception is well founded and there does not appear to be sufficient ground for stating that the development of given characters followed a definite course as if predetermined. It cannot be claimed as proven that Waagen's persistent mutations are contrary to Darwin's principle of natural selection of chance variations.

Hugo de Vries (1901) meant by "mutation" that certain characters arise in plants by sudden changes and *dis*continuously; they are sometimes referred to as *Saltations* (from saltare—to spring, leap, or jump). These changes show no definite direction or adaptive trend. In contrast to Waagen's mutations, those of de Vries are interrupted leaps in directions that are entirely fortuitous, that is, they may prove either adaptive or inadaptive—the special direction to be later determined by selection.

Wilhelm Ostwald in his "Grosse Männer" gives a classification of great men into romanticists and classicists. The romanticist influences his time strongly and causes active currents of opinion in his contemporaneous stream of science. In the nature of things, this more personal form of scientific activity must disappear relatively quickly. As another feature of the romanticist's work, he cites his tendency to conclude his research before he has made the best use of his time and efforts. The romanticist has an excess or superfluity of ideas, plans and research possibilities, but he also has a great rapidity of mental reaction which equips him for particular, rapid work. Usually he accomplishes a great deal of work in a short time, but there is always the danger that he may become satisfied with a final disposition of his work, before he really has reached conclusion of it. The classicist, on the other hand, is one who restricts his work to one or at least very few problems. It is his greatest care to pursue his problem to a point that neither he nor anybody else can add to it or improve on it. For that purpose the classicist never permits a piece of work to leave his own hands and to be executed by somebody else, but the romanticist is glad to get rid of it. The great mathematician Gauss complained in his later life that his younger colleagues had taken results from him which he had gained long ago, but did not wish to publish them because he had not yet completely followed them out in all their details. His aphorism usually was "Pauca sed matura," which is the characteristic proverb for all pronounced classicists. Whilst the romanticist does not hesitate to indicate his error, the classicist prefers to represent his work as the product of his own labors exclusively and accordingly he clears up all evidences of the processes and methods by which he reached his eventual formulation of results and his conclusions.

The contemporaries of Newton reproached him by saying that although he showed them the height of his discoveries, he removed the ladder which led him up there. This side of the classicist's temperament, as interpreted by Ostwald, is discouraged by him because it renders scientific achievements more difficult and does not facilitate investigation. He considers it an esthetic prejudice which sacrifices the educational effect that the demonstration of the paths and methods which led to a certain eminent discovery would have, to the gratification of being considered the exclusive possessor of a particular scientific achievement. For the understanding of such paths and methods by posterity would make it easier for them to reach definite scientific goals. Moreover a knowledge of the methods of which great minds make use would be of importance for a theory of human genius, because these working methods might possibly reveal more of the constitution and peculiarity of creative working than the completed product of a scientific research could do (Ostwald, l. c. page 385). In this very interesting discussion upon the different types of scientific minds, Ostwald does not in my opinion sufficiently consider the influence of the volume of already established scientific knowledge that preceded, or existed at the same time or succeeded the date of any scientific investigation. It is not simply the contemporaneous knowledge that gives direction to the thought-work of the investigator. He may be personally confident that his experimentations and observations are complete in every detail because he can conceive of nothing further to add. A succeeding period, however, may view the same problem in a different perspective and this for two reasons: first, because mental attitudes change with a lapse of time largely because our capacity for understanding requires repeated and prolonged mental digestion and meditation, no matter what the special problem of natural science investigation may be; and secondly, because new experimental results in collateral branches and new interpretations may throw a different light upon the problem than has hitherto been possible. There is a mutation-process in the manner of forming conclusions by the scientific mind just as there is in the evolution of living things. For these reasons we cannot consider Ostwald's distinction between romanticists and classicists as universally applicable. As far as such distinctions could be applied to the life and work of Darwin, they fail entirely. At the time that Darwin was working upon the evaluation of his scientific observations

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and the formulation of his conclusions, there was no one who could have added anything very material to either the work or its methods and yet his work has been expanded and added to continually and received new interpretations from his day to the present time without very materially changing the bedrock of facts upon which the structure of Darwinian evolution rests.

XXXI

THE BIOLOGY AND PSYCHOGRAPHY OF THE INVESTIGATOR

IT is a favorite expression of biographers to speak of the personalities they are depicting in the following manner: "Some day, some one will present us with a biologic or psychologic method wherewith we will be enabled to understand the mental operations of great thinkers, investigators, discoverers, research workers, etc." With the word *"some day"* they indicate that that time has not yet come, but that they are looking forward to it. In endeavouring to present an insight into this most difficult of all biologic investigations, I will not use the phrase "some day" because that day is here now. We have been in the period of analysis of the human mind ever since the thought world of Socrates, Plato, and Aristotle, has revealed to us, some of the inner psychic processes of man.

The great work of John Theodore Merz, "A History of European Thought in the Nineteenth Century," is replete with expositions concerning the method of operation of the minds of great men, from the earliest thinkers to the present day. In the last twenty-five years the mental laboratory of great men has become the subject of investigation from the strictly biologic, biometric, and statistical standpoints. Abderhalden's "Handbuch der Biochemischen Arbeitsmethoden" presents recent treatises on the psychologic analysis of human thinking, even of human dreams, making it possible to approach these problems by strictly biologic methods, and in Bleuler's "Das Autistisch undisziplinierte Denken in der Medizin und Seine Überwindung," there is a highly instructive chapter on the various kinds of thinking, another on the probabilities of psychologic conception, and on the discipline of thinking in scientific publications. The student interested in the psychography of great medical thinkers is referred to these profound studies for guidance and inspiration.

It will be a surprise to the medical reader to learn after he has entered far enough into this class of literature that one of the chief weaknesses of the psychologic study of great thinkers and one of the chief obstacles to progress in this line of research, is the lack of fitting definitions. We are suddenly confronted with the truth that man has become convinced that there are processes of consciousness which he is incapable of expressing in words. In Schopenhauer's opinion the real objects represent most always only the most defective examples of the idea contained in them. Here genius requires phantasy in order to see in the thing not only that which nature has really formed, but also that which she planned and endeavored to form, but failed to accomplish because of the struggle and competition of her many forms among themselves. Every conception that corresponds to reality is deduced from many, sometimes from innumerable, experiences which cannot be expressed in a few words and therefore accurate definitions are scarce. All long definitions are unavoidably inaccurate because the mind requires brevity for a clear understanding, and one of the great defects of all works on simply narrative or pragmatic history is the willful setting up of definitions according to the private needs of the writer who then goes on to operate with definitions of his own invention, as if they had been universally accepted. One of the first requisites, therefore, of studying mental operations is clear definitions of the phenomena and processes of thinking that we are capable of understanding, and also definitions of the procedures and means by which we endeavor to penetrate into the secrets of the mind and by means of which we hope to synthesize the results of our psychologic studies.

AUTISTIC THINKING

This is the kind of thinking which disregards the limits of experience. It abandons the control of its results by reality and relinquishes the test of logical criticism. Autistic thinking does not seek the truth, but rather the fulfillment of wishes. Accidental association of ideas, vague analogies and affective desires replace the solid experiences of mental associations that are gained by a strict logic and realistic thinking. Autistic thinking is the curse of medical historiography, but also of psychologic investigations into the mental laboratories of great investigators. When the human mind undertakes the audacious problem to investigate itself, it is confronted with so many difficulties that for making a beginning we must first establish a category of fundamental concise definitions. In the following, therefore, I will endeavor nothing more than to give a few definitions of the manners in which great thinkers have conceived the mental operations of other great thinkers, believing with the German poet quoted in the article on Leonardo da Vinvi, *Medical Life,* February, 1924, that

Man kann nur einen Diamant
Mit einem anderen schleifen,

meaning that the revealing of the operations of one great mind can only be accomplished by the efforts of another mind equally or approximately as great.

EXACT AND SCIENTIFIC THINKING AND ITS LIMITATIONS AND MATHEMATICAL EXPRESSION

In the recent physiologic researches of Jacques Loeb and his school, and in the publications by Karl Pearson as well as those of Raymond Pearl, a method of thinking has been developing that is expressible in measures and numbers. As desirable as this is in the biological sciences, that deal directly with the metabolic functions of living things, it is practically impossible of application in the investigations of the mind. Professor Michaelis has written a book, "Einführung in die Mathematik," which is to facilitate a mathematical treatment of biological problems. I have endeavored in several pyschologic investigations to work along mathematical principles, but soon have come to the end of the application of these methods. That kind of thinking which can be made expressible in numbers constitutes only the smaller part of the thinking that is scientifically necessary, for when we begin to study the mind, qualitative differences cannot be adequately expressed in numbers, and quantitative differences, whilst more expressible in numbers than the qualitative, are only within certain limitations to be encompassed by the mathematical method.

A mathematic expression of a living process has value only if the facts underlying the figures correspond to their accuracy, that is, if the measurements expressed by the figures are just as accurate as the numbers themselves. Bleuler calls attention to the impossibility of obtaining a mathematical statistic of the time of optical perception during experimental reading with the apparatus known as the tachistoscope. In ten experiments a mean value calculated to the tenth decimal may be reached in such experiments, but are they mathematically exact results? Do they represent reality absolutely correctly? What they give us is in reality only a number which is subject to the theory of probability, because in the *first* place, the experimenter may have made an insufficient number of tests, *secondly,* the measurements may not have been sufficiently accurate, for in the use of a watch that gives fifths of seconds only the first decimal number can be correct, but in using a chronoscope, perhaps the third decimal number may be correct. Other factors enter into this experiment that vitiate its result. These are the personal equation of the individual, the degree of accuracy of the vision, the degree of education, and culture, the practice in reading, the individual disposition for the day and for the moment; moreover these factors enter both into consideration of the subject that is experimented upon as well as of the person that performs the experiment. Moreover, the experiment conducted only upon one person, no matter how many times, is not applicable in its results to other persons. After all the figures have been obtained in this type of experiments, there is a possibility that they may be erroneously combined or erroneously interpreted, and the most exact mathematical methods cannot prevent this, for mathematical methods in themselves do not produce real exactness of deductions. The best that can be said of them is that their correct application can produce under favorable conditions the greatest exactness that is obtainable. The words *correct application* imply a process of thinking. Therefore the mathematical methods by which some hope to obtain exact reality or real exactness concerning processes of thinking are in themselves capable of application only by a process of thinking. In short, the mathematical method is nothing but an instrument, and like all human instruments, its success or failure depends upon the degree of dexterity with which the mind is capable of using it.

SOURCES OF ORIGINALITY

Every original pathfinder is a genius. Every leader and reformer in any domain of human activity, who makes even the smallest part of crude nature subservient to man, is a genius, and every artistic or scientific accomplishment in medicine by which the means for a relief of suffering and the cure of disease of men is advanced, is the work of a genius. It is not at all necessary that genius should be always original. As a matter of fact it is impossible that all contributors to the advance of medicine should be original. Goethe in one of his letters to Eckermann writes, "It is said in praise of the artist that he has everything from out of himself. If I only did not have to hear that again." Judged accurately, the productions of such original genius are mostly reminiscences. Whoever has sufficient experience will be able to demonstrate that in individual cases. On another occasion Goethe said, "They are always talking about originality, but what do they mean to say? As soon as we are born, the world begins to work in upon us, and that continues so until our end. Moreover what can we really call our own, but the energy, the force, and the will. If I could tell all that I owe to great predecessors and contemporaries, not much would remain." Chateaubriand gave expression to this statement: "An original author is not he who does not imitate, but he whom nobody else is capable of imitating."

If we could devote the time and energy for an exact investigation concerning originality, as well as imitation, that is detectable in the work of most medical geniuses, the amount which the greatest of them owe to others and sometimes to unimportant and lesser talents, would be astonishing. It has happened that smaller talents have frequently hit upon an important theme or idea, but did not know what to do with it. Then later some medical genius elaborated this idea into a great constructive doctrine. In addition to whatever familiarity with the history of medicine I may possess, I am somewhat of a lay musician and composer and as such have been able to note repeatedly the manner in which the greatest geniuses in music have borrowed from comparatively unknown composers. For instance, Mozart has utilized an apparently insignificant motif from a sonata of Clementi in his Overture to the *Magic Flute,* and what is more striking, he has evidently done this in a conscious manner in order to demonstrate what this motif really contained and what a master composer could make out of it. Ralph Waldo Emerson has gone into these obesrvations in great detail, and given expression to a study of genius which is widely quoted in German literature pertaining to this subject. Emerson says "almost everything which the genius thinks, says, and does has been thought, said and done in a similar form before. All that genius does is to give it a more tangible form. It takes the kernel of a subject from out of the concealing shell. It steers straight for its goal, instead of going by detours. Everything that genius produces is never its own work exclusively, but the work of many thousands, just as a commanding general does not win a battle alone, though he may contribute the most important brain work towards it. Great men distinguish themselves more by their comprehensive intellect and by the height of their viewpoint than by originality. If we demand of originality that it should produce everything from out of itself like a spider draws its web from out of its own entrails, and which finds the glue and the stones formed in itself to build it own house, then great men are by no means original.

"The characteristics of truly valuable originality consist not in its dissimilarity with others; the hero stands where the knights are grouped most densely; amidst the fullness of events he perceives what men are wishing for. He shares their longings, his vision is sharper, his arm reaches further, and thus he arrives at the goal toward which all are striving. The greater the genius, the deeper he is in debt to others . . . etc.

"The genius of our life does not tolerate the individuality. It wishes nothing of individuality to be great, except through generality. There is also no choice between them. It isn't so that a great man opens his eyes on one beautiful morning and says, 'I feel full life in me. I will sail the seas and discover the Antarctic Continent. I will find the square of the circle. I will investigate all the plants of the earth and reveal a new food for mankind. I have a new architecture in my head. I see a new source of mechanical energy'—but the great man swims along in the current of events, feels himself driven forward by the ideas and needs of his contemporaries. He stands where all eyes are following one path, where all hands are pointing to one place, towards that point he shall go, etc. . . .

"Every master found his materials already prepared for him, his force lying in the fact that he felt himself at one with his people and loved the materials which it had prepared for him. The world has carried him so far on his path; the human race that preceded him, has leveled the ground, reduced the hills, filled up the valleys and bridged the rivers. Men, nations, poets, artisans and women have actively labored for him, and he steps in where they have been working. Some think he chose any other particular thing aside from the general trend, just as if he had everything to do for himself. But if this were so, his strength would exhaust itself in the first preparations. One is tempted to assert that the great force of genius by no means consists of originality, but in the receptivity from others to let the world do everything and keep the path open for the spirit of the hour when it passes through our soul."

The objective historian dealing with medical historiography, has to take exception to many of these assertions of Emerson. It is not true of medical genius except in very few instances that his magnitude is owing to the work of others. Sometimes the work of the preceding epochs has actually inhibited and checked the development of true medical genius. This will be demonstrated in the biographic historic researches on Leonardo da Vinci, Michael Servetus, in the History of the Discovery of the Circulation of the Blood and on Lavoisier.

Ostwald in his "Forderung des Tages" (p. 302), states that the personal psychic make-up of the investigator and the peculiarity and manner of his workings will determine how soon or late he will reach the zenith of his activity, and how long it will last. But a state of intellect of any time does not require a great measure of knowledge of the sciences that are already preëxistent. So rarely is this the case that such a broad knowledge would sooner act as a disturbing factor by restricting the perspective to very limited directions hitherto preferred by science. On the contrary, a state of mind will be more productive when it is unencumbered and free of the knowledge that antedated the research of any particular worker. This would enable him to test all traditional viewpoints in an unrestricted manner. Elsewhere Ostwald says that the least burdening of a young intellect with knowledge that has been handed over to him is a favorable condition for successful research. Apparently Emerson and Ostwald represent diagonally opposed views, and as these views are extreme opposites, one could immediately deduct therefrom that the truth lies somewhere midway between the two views. Ostwald, having been a world-renowned chemist, and one of the pioneers of physical chemistry, ought to have been more familiar with the history of medicine previous to his time. In fact, among his *Grosse Männer,* of whom he gives biographies, there are Humphry Davy, Julius Robert Mayer, Michael Faraday, Justus Liebig, Charles Gerhardt, Hermann Helmholtz. Mayer and Helmholtz were medical men, at least in the beginning of their careers, and Liebig and Gerhardt were chemists. He has also written interesting biographies of Jacob Henricus van't Hoff and Paul Ehrlich. But if the previous medical training of these men is investigated, it will be found that they had undergone a very classical education and also a thorough training in the natural sciences.

With a few of the men whom I depict in this work, their elementary education was not traceable with sufficient thoroughness to be utilized as historic factors, but with the majority of them there was every evidence of their familiarity with the state of science previous to their birth. With art and literature such incompleteness of early education among the geniuses may be more frequent than in medical history. So much is clear, that genuine originality which is the exponent of genius, is something that has become organic in the individual and presupposes an inherited *Anlage* which cannot be acquired by any energy or industry no matter how intense. Another characteristic of originality is that it bears the earmarks of the unintentional. It comes uncalled, of its own initiative, or it comes not at all. Séailles' "Das Künstlerische Genie," instructs us that "All stereotyped thoughts, all artificial feelings, all modern notion, all that is rigidly school-like, all that is strange to his true nature, falls away when an artist becomes in the highest degree himself. He is then most original when the intellect again becomes nature, when he lets himself go into his subconsciousness, when he draws a work out of his true inner *ego* (just as a plant comes out of the earth), when he evolves it from his own elements, gives it form and life from out of himself. Then he manifests himself in this work and his own individual ego radiates from himself. One need only strenuously to grasp after originality in order not to find it. To find one's own self, one must lose one's self. That is, one must renounce all that which one has conceived as the ego. Without hesitation, the research worker must give himself entirely to the idea for its own sake, because one loves the idea free from all personal interest."

Otto Jahn in his scholarly work on Mozart quotes this great composer as saying, "Just in the manner as my works and products assume the form and expression that they become Mozartian and not according to the manner of any other composer, this is comparable to the process by which my own nose has become just so large and bent that it is Mozartian and not like the nose of other people, for I lay no stress upon peculiarity and could not even describe my own originality. It appears to me perfectly natural, but people who have an exterior presence are different from each other, outwardly and inwardly. At least I know that I have not given to myself one or the other (meaning Mozartian form of composition and Mozartian nose)." In medical genius we often observe the same features of intellectual production as well as external appearance. No one can say in every instance, just how the great work has become that which it is. For everywhere, where we have the rare opportunity to observe the work in the making, we become cooled and disappointed. But the complete work rejects all thought of how it became that which it is. It tyrannizes opinions by its very present perfection. That is the reason why we know so little concerning the manner in which great medical works, discoveries, doctrines, and inventions were evolved.

In one of his letters to Eckermann, Goethe identifies genius with productive force. His words are the following: "It is not only in making poems and writing dramas that one can be productive. There is also a productivity of deeds which in many cases stands significantly higher. Even the physician must be productive if he wishes to be truly a healer. If he is not, then he will succeed only now and then as if by accident. Generally he will not rise above bungling. Productivity and genius are very closely related things, for what else is genius than the productive force through which deeds originate that can persist and endure before God and Nature."

THE SUBCONSCIOUSNESS IN THE MENTAL EQUIPMENT OF GREAT THINKERS IN MEDICINE

The discernment of the human intellect and human feeling has made it expedient to conceive of psychic activities as occurring in the over-consciousness and in the under- or sub-consciousness. The over-consciousness is the storehouse for the acquisitions of the single individual. We call it the reservoir of personal experiences. It is the consciousness of reason that is peculiar to man. This upper or over consciousness is far better understood and unless specially designated, when only the word consciousness is used, it is the upper or reason consciousness of personal experiences which we mean. But there is also a more impulsively, instinctively working consciousness of the feelings which we designate as the subconsciousness and which is the preponderant consciousness in the entire animal realm. It is this which is the storehouse of the most ancient instincts that are of advantage to the species, the depot of feeling gained in the ceaseless hard struggle for existence, and which in critical moments assures an automatic action of races, nations, tribes, and families. The upper consciousness, the reason, serves mostly for the egotistical purposes of man, whilst the subconsciousness serves the social impulses.

According to E. H. Weber and Fechner, these two forms of consciousness are organically connected with each other, and they speak of a *threshold* of consciousness by which they mean the border territory which both separates as well as unites these two kinds of consciousness. The value or the height of this threshold is variable, that means, the realm of thought territory, that has to be traversed to get from one into the other, varies. It is great during sleep, less in the waking condition. It is diminished by concentration and attention. It is increased by diversion. Many processes which at first have to be done in the over-consciousness later transpire in the under-consciousness. In fact, the most admirable and complex actions in order to be executed perfectly, must be practiced so often that they can be performed by the sub consciousness.

A great many of the most eminent thinkers of all nations agree that the greatest achievements of human genius arise out of the subconsciousness. Among the French this is emphasized by Montaigne, by La Bruyère; among the English it is expressed by Swift, Fielding, Carlyle, Sterne, and Spencer; among the Germans, Schopenhauer, Möbius, Goethe, Schiller, Lessing, Mozart, Wagner, Helmholtz, and almost by all the Italian anatomists and physiologists mentioned in my article on the circulation of the blood, and especially, in the studies on Leonardo da Vinci.

My personal acquaintance with musical composers of note with whom I have been fortunate enough to discuss this problem, in this country and in Germany, confirms the opinions of Goethe and Schiller namely, that everything which a genius does, originates in the subconsciousness. A man of genius may of course also act according to his reason after careful consideration and conviction, but all of that is only accessory. No work of genius can be improved by reflection, nor can the exercise of reason free a work of genius of any possible errors. Only genius itself can rise gradually by its own reflection and own effort so that it eventually produces exemplary works. The feeling of the source of great work in the subconsciousness is very aptly described by Schopenhauer when he says "I am blessed with fruit. My spirit accepts food from the world by my reason and the senses. These, however, only give body and nourishment to the work, but I could not say how nor why this work has arisen in me and not in somebody else whose person might furnish it the same nourishment."

This is one of the best descriptions of the intellectual compulsion of a great work. It arises in the subconsciousness. Great thinkers occasionally express themselves in these words: "SOMETHING IS THINKING IN ME." Goethe as well as Mozart expressed the opinion that the ideas of which they became aware during the waking state, at daytime, had frequently been formed at nighttime in regular dreams in order to appear as a marvelous entirety or something that was already present when they opened their eyes. Brilliant medical ideas have been described to me by Dubois-Reymond which, I was personally aware, had forced themselves upon him almost against his will. Musical compositions are frequently born out of the subconsciousness in the same manner, so that I know from my own experience that they are at times tyrannical in their insistence upon the attention and will not release the subject in whom they arise until they are actually noted down. Goethe has somewhere described that in a very beautiful verse,

> Was vom Menschen nicht gewusst
> Oder nicht bedacht
> Durch das Labyrinth der Brust
> Wandelt in der Nacht.

Medical thoughts are not comparable to musical thoughts except with great stretch of the imagination, but the processes by which both arise out of the unconscious are very similar in both instances. In a discussion between Sylvester, the mathematician, and Kronecker, the physiologist, as regards the nature of mathematics, they finally reached a common ground in the agreement that mathematics is poetry. Personally I can conceive of profound medical thought as an all-pervading harmony and in speaking of the history of medical thought, I may aptly borrow an expression of Karl Lamprecht in his essay in the *Zeitschrift für Kulturgeschichte* (1898, Bd. 6, S. 40), where he speaks of the all-pervading, concordant harmony which penetrates all history and which he calls the DIAPASON (from the Greek words δια, meaning "through," and πασων, genetive plural of πᾶς, meaning "all"). That there is a diapason in medical history must become evident to the thorough student of the writings of Sudhoff, Pagel, Neuburger, Singer, and among our American medical historiographers, the works of Garrison, Streeter, Packard, Welch, and Walsh. That this all penetrating harmony is not always perceptible is due to the fact that its perception requires three conditions of mind which can only come from long continued culture. These conditions are broad experience, conservative critical judgment, and the warm, sane heart. Mozart expresses ideas concerning his compositions which are strikingly like those expressed by Helmholtz in an address on the occasion of his seventieth birthday. Perhaps it is not altogether accidental that this great physicist and physiologist was not only the author of the monumental "Sensations of Tone," but was himself an expert in musical appreciation.

Now, as regards the origination of thought, Mozart says (Otto Jahn): "When I am entirely by myself and in good spirits, perhaps traveling or sometimes walking after a good meal, or at other times during the night when I cannot sleep, then the thoughts come to me in currents from where and how and why, that I know not. I cannot help it. Now the musical thoughts which please me I try to retain in my head. Sometimes I hum them to myself. If I can retain such a thought, then soon others associate themselves. Eventually I find where the fragments are to be used to make a 'pastry' out of them, according to counterpoint and the tone of the different instruments. This warms my soul and gradually, if I am not disturbed, the tone-mass becomes continually greater and I expand it further and clearer until this tone-complex in my head verily becomes almost colorful, and although it may be long, I can still overlook it afterwards with one glance like a beautiful picture or a pretty human being. I can overlook it as one totality and not as one part following another part in the manner it came to me, but in my imagination I seem to hear it altogether. Now, that is a great treat, to find all that and to make all that, which occurs within me as if in a deep dream, but the rehearsing of it, all of it together, that is the best. Anything that has come into existence like that, I do not easily forget and that is the best gift which the Lord has given me."

Helmholtz in the address alluded to before, after stating that the field of work in which he did not have to depend upon "lucky intuition," was almost the most pleasant for him, continues, "As I frequently happened to come into the uncomfortable situation of waiting upon *lucky thoughts,* I gained some experiences concerning how and where they came to me. These may perhaps be useful to others. These thoughts sneaked into my consciousness without being recognized as to their significance. In that case some accidental occurrence may have later helped to make clear when and under which conditions they came. Otherwise they seem to be present without one knowing whither they came. In other instances they are presently before my mind like an inspiration, without any effort, but as far as my experience goes, they never came to an exhausted mind and never at the writing table. I was always compelled to turn my problem to all sides to and fro, so that I could overlook all of its bearings and complications in my mind and could run through their entire relations without writing. To do this without much preceding work is not possible. This done, it was necessary to have an hour of complete rest at the end of which bodily freshness and quiet well-being supervened before the fortunate thoughts came. Frequently they were present in the morning, according to the verse cited from Goethe (see above) which Gausz has already noted. They came especially when I was occupied in the gentle ascent of wooded hills during sunny weather, but the smallest quantities of alcoholic beverages seemed to disperse them. Such moments of fruitful thoughtfulness were indeed very enjoyable, but the reverse side, that is, when the redeeming intuitions did not come, was not beautiful. For weeks and months I could grapple with a problem until I felt like the animal that Goethe describes which is led by an evil spirit in an endless circle upon a desolate, desert meadow, whilst all about there may be green, beautiful pasture. Eventually it was often an atrocious attack of headache that redeemed me from the spell, and liberated me for other interests. The writing out of the scientific investigations is mostly a laborious work. It was to me, at least. I have written and rewritten many parts of my treatises four to six times, and rearranged the order before I became satisfied. But in this careful composition of the work, there is a great gain to the author. It forces him to the keenest testing of every single expression and conclusion. I have never regarded an investigation as complete until it stood before me written out completely without logical defects."

Medical discoveries, new doctrines, great new thought structures like the side-chain theory of Ehrlich, and the theory of ionic dissociation are known to have undergone a process of maturation in the subconsciousness of the author before they were expressed in written or spoken language. Frequently these great ideas surprise the upper consciousness by their far-reaching results, of which the originator had no presentment. The will seems to retire during such a psychic gestation, whilst the incentive that has been given by the will continues. Psychic life seems to reveal itself with less inhibition out of the absolute stillness of the subconsciousness. It sometimes happens that concrete conceptions that have been sought with great mental effort for a long time and could not be formulated, suddenly flood consciousness spontaneously and without any effort of the will, and the great thinker expresses a feeling of surprise on the first aspect of his own work. It appears as if it had formulated itself in his own *sanctum sanctorum* of consciousness, as if he had really not given the great thought structure or doctrine or truth or discovery himself, but rather as if he had received it from somewhere (Seailles). When I had occasion to discuss the origin of the theory of ionic dissociation with my friend, H. J. Hamburger, of Groningen, Holland, and also with van't Hoff, I arrived at the extraordinary process of mental elaboration of great ideas, indirectly, not from what these men told me about it themselves, but by listening carefully to the simple, naïve descriptions which they gave of their remembrances concerning the first thoughts they had on this highly fruitful doctrine. (See Article on Pasteur on "Mental elaboration, etc.)

The vision of truth, however it may be conditioned by facts, discoverable by analysis, is in the *last resort to be attributed to the operation upon man of some power that is above him, and to this power we can ascribe a sort of control or guidance in the development of civilization.* This is an old doctrine and is especially connected with the name of Plato. The doctrine finds support in the experience of all discoverers of new truths, of all new insights into human physiology and pathology, in the experience of all great inventors, namely, *that the creations of their minds come to these men as a revelation. The great thinker has the vision but what he sees seems given from without, at least so far as it appears as a vision of the truth—the truth is something that accords with the nature of things and in it this nature is self-revealed, and the so-called great individual is simply its minister and interpreter.*

This is my personal conviction concerning a great many of the constructive ideas and discoveries in the history of medicine. Some may argue that such a view has no place in science. To this I may answer that science does not deal with ultimates but is concerned, it seems to me, with the discovery and orderly assembly of relevant observable facts; in other words, with the analysis and synthesis of observable conditions. When, therefore, for example, science treats of the development of ideas, it proceeds just as when dealing with the development of any physical thing. It collects all the relevant observable facts bearing on this problem, takes account of the instances of the same, or similar ideas, already announced, of the cultural conditions, education, endowment, social relations, etc., of the individual who makes these contributions or his epoch-making discovery, and having done so much is content to let it go at that. But there is always a residuum, a mystery. The question arises, is there anything in addition to heredity and environment which conditions the quality of the human being? The psychic *Anlage* of the individual is not completely exhausted by details of heredity which modify them, and certainly the mind of the concrete individual is more than the sum total of the circumstances which make up his environment. James Ward has employed the term *psychoplasm* on the analogy of the term *bioplasm* of the scientist, and it is an hypothesis of some philosophic thinkers that psychoplasm is a something found or presupposed in the individual which is imbedded in or part of a universal mind, or consciousness. In my opinion there is not yet sufficient evidence even from the purely psychologic standpoint, for the formulation of such a hypothesis, but I do believe, from the study of the minds of great thinkers, especially of the mental operations of the master minds in medicine as far as the sources and documents and personal acquaintance permits them to be studied, that truth is not as the pragmatists teach, merely a useful instrumental device of *our* minds for effective action, but that it is really something embedded in the nature of things to which our minds should submit, and that discovery or evolution of great constructive ideas is really a revelation, a manifestation of truth to our minds.

I am convinced that there is room for this kind of belief in our minds, namely the beliefs over and above what scientific construction calls for, but I am aware that they are nothing but beliefs as yet and are in need of constant critical examination and revision. Arthur James Balfour, in an address to the British Association for the Advancement of Science, (Cambridge, 1904), in speaking of the nature of reality, said "The common notion that he who would search out the secrets of nature must humbly wait upon experience, obedient to its slightest hint, is but partially true. This may be his ordinary attitude; but now and again it happens that observation and experiment are not to be treated as guides to be meekly followed, but as witnesses to be broken down in cross-examination. Their plain message is disbelieved, and the investigating judge does not pause until a confession in harmony with his preconceived ideas has, if possible, been wrung from their reluctant evidence. This proceeding needs neither defence or explanation in those cases where there is an apparent contradiction between the utterances of experience in different connections. Such contradictions must of course be reconciled and science cannot rest until reconciliation is effected. The difficulty really rises when experience apparently says one thing and science persists in saying another. What is the origin of scientific instinct? What is its value? Whether it is a mere prejudice to be brushed aside or a clue which no wise man would disdain to follow, I cannot now discuss."

In the continuation of this profound address, Balfour dwells upon the limitations of our senses, and suggests a certain inevitable incoherence in any general scheme of thought which is built out of the materials provided by natural science alone, and here is where the thoughts have to be entertained, which I have spoken of a little while ago, as regards the nature of truth, and from the studies of the master minds that I have mentioned, it is evident that truth comes as a manifestation from without, and as a revelation. As one of the evidences of the quiet work of the subconsciousness, it may be well to introduce an observation which everyone can make or perhaps has made upon himself. When a certain thing or event or connection of things or name has escaped our memory, it is sometimes impossible, in spite of all mental exertion, to recall these things or name. It is then best to dispel this entire matter from the mind and try to rest or think of something else. Often an astonishing experience takes place. The desired name or the things which we wish to recollect suddenly rise up out of the subconsciousness. They force themselves over the threshold into the upper consciousness and surprise us by their voluntary appearance without our ever having done anything to bring this on, and when all of our efforts prior to this were in vain.

There is historic evidence that all great ideas, doctrines, and discoveries, etc., in medicine had a period of incubation. Frequently the minds of these great masters are so pregnant with constructive ideas that they actually become incapable of themselves to release the uncrystallized thought structure held within their consciousness. Here it requires some external factor to bring the entire thought mass into crystallization, and it is something like the process which Goethe has described as existing in water just prior to the moment of freezing. The very slightest touch of a vessel of water in this condition of turning from liquid into solid is sufficient to convert it into ice. These external releasing factors are sometimes spoken of as the accident of an invention or discovery. Sourians speaks of the accident as the peculiar principle of invention and discovery. As a matter of fact accident may occur in countless human beings without having the effect that it does when it happens to a master mind. Sourians describes this more accurately in defining accident as an occasional coincidence of physiologic and psychologic mechanisms with subjective aims that we must presuppose. This makes it clear that the subjective aims must already be present in order that the external factor of accident can release and coördinate the thought mass that has been endeavoring to form itself in the subconsciousness. Goethe describes this external factor which released his vertebral theory of the skull from the state of a subconscious formulation and coördinated it to the definite form of the doctrine that the bones of the head are in reality modified vertebrae. The releasing factor he describes was a burst skull of a sheep which he found on the dunes of the Lido which separates the Venetian lagoon from the Adriatic Sea. On seeing this sheep's skull, subconscious ideas immediately crystallized into the conception that the skull bones were modified vertebrae. Accordingly the bones of the head like the vertebral column, are composed of metameres, and Goethe thought that he could distinctly recognize six metamorphosed vertebrae in the structure of the skull. Leonardo da Vinci in his work on painting says, "When I carried a number of thoughts a long time within me, I believe that I saw the picture as a sudden discovery before my eyes. If I was looking at a fissured wall, I saw the outlines of what I had conceived long ago." Spots in a wall and fissures would have meant nothing to an ordinary man, but Leonardo da Vinci had carried the plan in his brain weeks before.

In the history of the discovery of the four eminent metabolic functions of the living organism, namely, digestion (see article on William Beaumont), respiration (see article on Lavoisier), pulmonary circulation, and the greater circulation (see article on Servetus, and on the history of the discovery of the circulation of the blood), this so-called releasing factor which gives the final impetus for the crystallization of a large mass of thought material accumulated in the subconsciousness, is not so manifest in the earliest workers in any particular field. This is partly due to the fact that our records concerning the earliest workers are not so complete and accurate as those concerning the recent work. There is more accurate literature about William Harvey than there is about Hippocrates or Galen, and it is partly due to the fact that the more ancient thinkers and workers, because of the lack of precise methods of experimentation, indulged more in speculation. This brought it about that the accumulation of solid facts in any particular branch of science gradually became a part of the subconsciousness of later workers. But it was not until Harvey that the releasing factor for a clear cut demonstration of the circulation received its final impetus.

We have so far become familiar with the consensus of opinion of many great men that their great constructive ideas are not original with them, that they frequently confess surprise at the far-reaching bearings of their own great thoughts. What is popularly known as originality is only so in the understanding of the populace or of the laity. As far as they can judge, it is original. The great man is well aware of his debt to those who have preceded him. Another great insight we have gained is that in the production of great thoughts, doctrines, discoveries, etc., there are two processes; one is the accumulation of facts and truths and insights in the subconsciousness, the other is the release by some external factor of this thought mass into a definite new concept, idea, doctrine, theory, discovery, etc. As the process of accumulation may be handed down in some manner from predecessors, I have termed this the *period of incubation of a great idea*. I don't mean to say thereby that great ideas are transmitted by inheritance which would be a confession to the theory of the transmission of acquired characters, but I do believe in a special psychic ANLAGE of the mind of the genius.

In order to facilitate the manner in which great thinkers have been classed into types, I will introduce the following synopsis of the views of Plato, Bacon, Kant, Goethe, Gompertz, and Ostwald.

PLATO—For the views of Plato concerning types of thinkers, see the article on Methodology.
BACON—Classifies his subjective and objective workers as 1. Ants and 2. Spiders, respectively.
KANT—1. Law of specification.
2. Law of Homogeneity.
Both laws probably coexist in most investigators and are both objective.
GOETHE—1. *Nutzensuchenden,* Utilitarian.
2. *Wissbegierigen,* Eager or curious for knowledge.
3. *Anschauenden,* or Intuitionalists.
4. *Umfassenden,* or Comprehending Scientists.
GOMPERTZ—1. Insatiable thirst for knowledge.
2. Striving for mental freedom from contradiction.
Both mind qualities, but not differentiating.
(In his "Griechische Denker.")
OSTWALD—1. Classicists—*Klassiker.*
2. Romanticists—*Romantiker.*

The different manner in which they attack their problem, the form they give to the point or aspect of inquiry (Fragestellung), the contrast in mental initiative, their concept of what constitutes a well-established successful termination of an investigation, the time or rapidity of intellectual reaction, the different ways in which either the purely scientific or the more applied aspects of problems appeal to them, are the salient features that distinguish two classes of great thinkers to which students of the biology of investigators have given various designations.

Bacon describes two groups, the *Ants,* who are collectors and the *Spiders,* who are organizers.

Two fundamental tendencies of the scientific, as well as of the philosophic mind, which are emphasized by Kant in his *Kritik der reinen Vernunft* (Anhang zur transcendentalen Dialektik), are the endeavor (1) to order genus into species, which he calls the *Law of Specification;* and (2) to group similar things to higher concepts, which he calls the *Law of Homogeneity.* This is an attempt to understand rather the purpose of research work than the manner and method of thinking that controls it.

The various kinds of nature observation and study were once classified by Goethe in a manner easy to survey. He divided them into four classes—it is difficult (in English) to define each by one designation as he attempted—but we may provisionally name them as first (1) The *utilitarian* aspects of nature; second (2) The kind prompted by ordinary *eagerness* for knowledge (I should supplement by calling them the curious about nature); third (3) The *intuitionalists* ("Anschauenden") and fourth (4) The comprehending scholars ("Die Umfassenden").

For once, Goethe used designations here which permit of too wide a latitude of interpretation to satisfy the orthologic and generally scientific requirement for exactness.

The first two classes do not define the methods or manner of inquiry at all (Fragestellung); they rather refer to purposes. The term *Nutzensuchenden die das, was die Natur bietet, für ihre praktischen Zwecke verwenden,* expresses a purpose and may apply to all of the other types, in fact, to any classification. All medical science may be considered from this utilitarian aspect for instance.

So also the designation to the second group *die Wissbegierigen, die nur das wissenschaftlich verarbeiten was sie vorfinden.* He means they do not penetrate any further than the matter actually under observation—do not attempt generalization. This obviously has reference to "method" but may also imply purpose. It is not perfectly clear to what Goethe refers when he says *they only work up that which they find for the time being.* For to restrict the field of inquiry is not a fault, but laudable in an investigator. But the avoidance of generalization (with the meaning I prefer to accept) is also an advantage to an investigator.

One has to be somewhat of a *Goethe Kenner* to understand the true inwardness of his third and fourth groups, however, for here he employs expressions which do not define orthologically the thought he really means to convey. The reflection will have to turn on what he really has in mind when he says *Die Anschauenden* and *Die Umfassenden.* As the most cardinal and fundamental characteristic of the psychology of the investigator is revealed here, in my opinion, a detailed consideration of these two groups of this great poet-philosopher, himself a great investigator, may prove instructive; especially as they permit us to view the classification of Ostwald into "Klassiker" and "Romantiker" and also that of Gompertz (given in his "Griechische Denker") in a new light.

What then does Goethe mean by *die Anschauenden* and *die Umfassenden?* The German word "Anschauen" means to look on, or at, more especially here to look into, and the noun "Anschauung" in this sense really means "intuition" (from the Latin *intueri*—to look on or "into"). This undoubtedly means knowledge gained by direct inspection of the mind.

"Anschaulichkeit" or intuition is direct apprehension or cognition—an act of *immediate* knowledge—distinguished from *mediate* knowledge, which is an act of reasoning. Intuition is knowledge independent of experience.

We get a closer insight into what he means when Goethe adds *Sie suchen die Imagination nach Möglichkeit zu vermeiden und führen alles auf Anschaulichkeit zurück,* i. e., they seek to avoid imagination as far as possible and trace everything back to intuition.

Among this third group, whether rightly or wrongly, Goethe classed himself. I have expressed a doubt about the correctness of his inclusion because no one less than Helmholtz asserted that Goethe in his scientific work was prone to lean on or be guided by his imagination. There is however, some good reason for qualifying even Helmholtz's opinion. Be this as it may, it has only an indirect bearing on Goethe's definition. He means by the intuitive investigator, a worker who is objective, who does not allow himself to be consciously guided by his own subjective thinking operations; he does not disregard them, perhaps, but he approaches his subject in a perfectly unpreconceived mental attitude, willing to exhaust every possible objective method that promises a solution of his problem. The *intuitivist* is the objective worker. Kant also gives this interpretation to the word "Anschauung" (see Webster's Dictionary under *intuition.* P. 1134 last Edition).

What does he mean by *die Umfassenden?* "Umfassen" means "to embrace," "to enclose," "to comprise"; also "to comprehend," more especially "to encompass by comprehension." Goethe adds that "Die Umfassenden" in their thinking and investigation operate in the opposite manner from the "Anschauenden" or intuitivists—he states specifically that they start from *ideas* and seek their realization in nature. According to Kant, reason in this case goes from the contemplation of totality as a whole to particular details, i. e., from totality into the constituent parts.

The essential parts of this thought are these, *die Umfassenden* are led by their preconceived ideas; they approach their problems, not with an evenly balanced mind, ready to be turned in any direction by every new truth as it arises, but with an idea already possessing them, and they seek the realization of this idea in nature.

One could not call them idealists, because this term has already an accepted meaning, different from the thought Goethe just expressed. "Ideationalist" would come nearer the intention he held. The cardinal feature of the conception is that the "Umfassenden" work from within, outward; they are the subjective workers. It impresses me that the word "Umfassenden" means that they encompass the problem they take up by the limits of their own ideas.

Whilst the word "intuitivists" is not satisfactory, it approaches the meaning of *die Anschauenden,* but for *die Umfassenden,* I can think of no single word that adequately expresses Goethe's meaning. However, we have now progressed far enough to understand that he means to distinguish between (1) subjective, and (2) objective research workers.

That such a distinction is applicable only to investigators in natural science must be evident to everyone who reads Ostwald's classification of the sciences (in "Forderung des Tages" pages 102 and 103). For the arts it is not a possible classification for how could a sculptor begin a plastic work, or a composer a form of music, without some preconceived idea? For the classic, especially philologic departments of knowledge, it is also not well adapted.

Ostwald's classification (in his "Grosse Männer") consists of two groups, classicists and romanticists. The classicists are the slow investigators, but they exhaust their subject; and when they are through with it there is little or nothing that can be added. They never take up more than one problem at a time. The romanticists are the rapid workers, remarkable more for their brilliancy than for their thoroughness. On account of the multiplicity of their ideas it is impossible for them to exhaust their problem. A scientific problem is in reality never entirely exhausted, for a later generation may find things to supplement and transform the results of former investigations, (e. g., Gravitation from Newton to Einstein).

Two types of thinkers are described by Gompertz (Griechische Denker-Sechstes Buch, as 27. "Aristoteles u. Seine Nachfolger"). They are (1) those with insatiable thirst for knowledge, the absorbers and collectors of facts and data; (2) those who strive toward inner freedom from contradiction and unconditional consequentialness of thinking, the classifiers and organizers. These are really qualities of mind not applicable to the differentiation of types of research workers. Insatiable thirst for knowledge is a characteristic of all the fifteen men described in David Starr Jordan's "Leading American Men of Science," some of whom distinctly belong to Ostwald's type of classicists and others, beyond a doubt, to his romanticists. Gompertz, however, means by "insatiable thirst for knowledge" the acquisition of such a fanatical mental state that thinkers of the type of Aristotle, Liebnitz, Haller, Humboldt and Virchow are produced, and explains that their encyclopedic wisdom will meditate upon artifices and tricks for classifying this immense material of thought. Such men will serve the requirements for the arrangement and systematization of an enormous mass of knowledge. Their thought structure is so immense that it impulsively tends to a clearing up. This is an interesting mechanism of the thought machinery, but yet it is not the essential part of the research apparatus; it is the part that comes into play when the work of research is generally over. It may be applied as well to philosophic reflection as to classic and philologic research.

Interesting and valuable as they are as the expression of the thought of great men, none of these terms of classification include in themselves any psychologic or physiologic process, principle or doctrine. They are definitions that again call for further defining. What would be desirable would be two terms which in their very orthology embraces the essence of the psychic or physiologic mental distinction between these two types of thinkers, and the terms *subjective* and *objective* investigators are the nearest approach to this desideratum that I could conceive of.

I am aware that it is impossible to arrange all leading investigators under this classification. As we shall presently learn, this will be a defect to a greater or lesser extent of all such endeavors to classify men of science, but it is a classification of tentative value, applicable to the great majority of them.

From the time that philosophy as an abstract endeavor first began to occupy the minds of men, to the present day, innumerable difficulties of concept have been created by the gap that separates *man as an object* from *man as a subject.* All the difficulties from the first efforts of the Greek philosophers to bridge over this gap, to the brilliant, but futile generalizations of Du Bois-Reymond (Die Grenzen des Naturerkennens, 1884) and those of Tyndall, Huxley, Herbert Spencer, and Voltaire, appear to have their being, not in any unsurmountable obstacle which anatomy and physiology of the brain oppose to progress, but rather as Wilhelm Ostwald suggests, to an ancient error of conception, namely, that of holding apart matter and force as two different entities. Nowhere is it more clearly represented than in Ostwald's "Vorlesungen über Naturphilosophie" namely that it is more rational to consider matter and energy as identical. Matter as a primary conception does not exist; it is a secondary phenomenon arising through the simultaneous effect (coactivity-*Züsammenwirkung*) of certain kinds of energy. This conception goes even further than the similar "Reflections Suggested by the New Theory of Matter" by the Right Hon. A. J. Balfour (Address of the President of the British Association for the Advancement of Science, Cambridge, 1904), quoted in the preceding.

Every entity in our environment is energy. (Matter does not exist, per se). The next concept that is much facilitated by Ostwald especially, is that consciousness can more logically be regarded as a special form of nerve energy (Naturphilosophie, l. c., s. 393). This may be well founded on physiologic demonstration that brain work occurs only when a certain amount of energy is consumed (Exhaustion may be caused by mental exertion—blood pressure in the limbs and splanchnic region falls when the brain works, because the blood is diverted to the cerebrum—temporary brain exhaustion is repaired by food energy, rest, etc.).

It is a critical moment, when the human mind—which created all natural science—itself becomes the object of investigation by the methods of that science. Little wonder that Du Bois-Reymond said *Durch keine zu ersinnende Anordnung oder Bewegung materieller Theilchen lässt sich eine Brücke in das Reich des Bewusstseins schlagen.* (Grenzen des Naturerkennens, Leipzig, 1884.)

But the most brilliant of modern physiologists is not discouraged to undertake the objective study of brain physiology. Pawlow (Naturwissenschaft u. Gehirn, Wiesbaden, 1910) advises that all efforts on brain function must start by the same methods that have hitherto given reliable information with organs of a lower order. First of all, the changes in the environment (outer world) must be correlated and brought together with the changes in the animal organism that correspond to these outer changes, and secondly, the laws of these relations must be sought and established. Pawlow makes clear what he means by an analogy from experimental physiology of the digestive organs;—the correlation of the organism to the environment by means of chemical substances, the foods.

As most fruitful concept he defines the conditional and unconditional reflexes between the nervous and the digestive apparatus for an appreciation of which, references must be had to the original. Next he describes an entirely objective method of elucidating physiologic problems of the organs of special sense.

Pawlow speaks of the organs of special sense with an orthology that is forcibly indicative of the objectiveness of his method of research. He illustrates what is meant by an unconditional (unbedingter) reflex and by a conditional (bedingter) reflex, by an example on the stimulation of the salivary secretion. The unconditional reflex is the simpler one, that occurs whenever food touches the end organs of taste, causing afferent impulses to travel to the salivary center in the medulla and thence back again, efferently to the gland cells. This unconditional reflex is always ready to be set in operation; it might be called permament; it is the ordinary and more simple and usual mechanism. There are few physiologic conditions attached to it, hence the name unconditional. It is the old salivary reflex, a standing nerve connection, between taste, synapse, medullary center and salivary gland; it operates perfectly in animals that are wanting in the higher (cerebral) parts of the brain.

But the salivary glands stand in another, most complicated relation to the outer world, for we know that the sight, or simply the thought of food causes secretion of saliva. According to the older terminology, this was the *psychic secretion.* For this complicated nerve activity, the highest divisions of the brain are necessary. It is now shown that this most complicated nerve action, involving the most intricate relations of the metabolism of the living thing to the outer world, is a conditional and temporary nerve reaction, "conditional," because it depends upon so many outer conditions (chemical and physical properties of food) and so many complex inner conditions (cerebral, intra and intercentral conducting paths and centers). Temporary it is called, because it is not the usual or permanent mechanism.

These conditional (cerebral or temporary) reflexes have been studied with regard to the time of their formation, their different types and their general properties. As the conditional reflexes have their seat in the highest brain centers, where there is a continual inpouring and assembling of uncountable influences from the outer world, it must be plain that a continual war is waged there between the manifold and different conditional reflexes—a struggle for the choice of any given conditional reflex, out of many that compete for supremacy. This conflict brings with it the continual inhibition of these conditional reflexes.

At present, three types of inhibition have been established: (1) the simple inhibition, (2) the extinguishing, and (3) the conditional inhibition, i. e., a reflex may be, (a) simply "held up" or temporarily held in check, (b) completely destroyed, or (c) allowed to continue under new condition. What an encouraging new outlook for a new psychology!

These together form the group of external inhibitory phenomena for they depend upon the addition of something external to the intra central nerve process, i. e., to the conditional stimulation. Among the other important discoveries announced by Pawlow are the

(a) Continual fluctuations of the conditional reflexes that are already formed, caused by their inner relations, which may even cause their temporary, but complete, arrest.

(b) When an established old conditional reflex is repeated several times without being accompanied by the unconditional reflex, with the aid of which it was gradually developed, then the unconditional reflex begins to lose its effect and may fall to zero. This means, that whenever a conditional reflex, acting as a signal for the unconditional one, begins to signal wrong, it gradually loses its effect as a stimulus.

(c) This loss of effectiveness of the conditional reflex is not brought about by disturbance of this conditional mechanism, but by temporary, inner inhibition of the intracerebral processes, for the conditional reflex extinguished in this way, can after a time appear again as if by its own initiative.

(d) We have hitherto spoken only of excitation and inhibition of these reflexes, but a third process was discovered, which cannot be better defined than as an inhibited inhibition—a process of checking that which is set aside or released by another checking.

(e) The entire nerve activity as evidenced in the brain, as shown by these conditional reflexes, reveals itself as a continual balancing or adjustment between these three fundamental processes, namely, 1st, excitation, 2nd, inhibition, and 3rd, inhibited inhibition.

The newer objective physiology of the organs of special sense, hitherto physiologic doctrines of these important apparatuses, consisted mainly of subjective material, i. e., observation and experiments on the sensations and apperceptions of human beings and in that sense they were devoid of the extraordinary means and advantages which objective research and animal experiment offer.

Those results of sense organ experimentation which seemingly approach completeness, are undertaken entirely from the physical aspect of the problem. In the purely physiologic part of the investigation of the conditions and kinds of irritability in the nerve terminations in a given sense organ, a mass of unsolved questions has already accumulated. But in the purely psychologic part of the domain of the doctrines of the sensations and mental apperceptions, thus far, only the most elementary facts have been ascertained.

A new and more graphic terminology is gradually being introduced. Thus Pawlow speaks of the sense organ, not as we do; a sense organ to him is an analyzer of external energy. He speaks as follows (l. c., p. 14). "The ANALYSATOR is a complicated nerve mechanism that begins with an outer receptor apparatus and ends in the brain, sometimes in its lower, sometimes in its higher segments, and if in the latter way then its terminations are infinitely complicated. A fundamental factor of the physiology of the analyzer is this: each peripheral apparatus represents a special transformer of a given specific external energy into a given nerve process, and here is developed a long row of questions that are not yet solved, not yet even proposed. By which process does this transformation proceed in its final instance? In what does the analysis consist? Which part of the work of the analyzer is attributable to the constructive arrangement and the processes in the peripheral apparatus and which part is attributable to the brain end of the apparatus? By which laws does this analysis of energy occur? All of these and other questions can be subjected to objective investigation on animals, by means of the conditioned reflex. By establishing a temporary communication between this or that phenomenon of nature with the organism, it is readily determined to which degree of analysis of energy of the outer world a given "Analysator" may reach. For example, it is possible to demonstrate without unusual effort, that the ear (sound) analyzing apparatus of the dog distinguishes between the finest tone colors and the smallest intervals. The dog is capable not only of holding apart these smallest tone differences, but he can also retain them permanently, which in the human being, though much less developed, is called "absolute hearing."

In his ability to be stimulated by high tones, the dog goes much further than the ear of man, for the dog can perceive a tone with 70–90 thousand vibrations a second, whereas the limit of human ears lies between 40–50 thousand vibrations a second. It was furthermore discovered that differentiation by way of inhibitive processes is effected by subduing or checking the remaining parts of the analyzer with exception of one definite part which then is the only one acting; also, that when the brain end of a given analyzer is injured or destroyed, the more extensive the destruction, the coarser will the work of the analyzing apparatus become. For example—if with an intact cerebrum even small intervals can result in conditional reflexes, after destruction of parts of the central auditory area, it requires an entire octave to cause a conditional stimulation."

The principal characteristics of this form of work are, (1) That the investigator feels solid and fertile ground under his feet continually, (2) Arrangement of experimental questions in a logical and natural order, (3) A colossal increase of the horizon of investigation becomes not only recognizable, but also accessible by a purely objective method, based on exact scientific procedure.

As a part of nature, every animal organism represents a complex system, complete within itself, the inner energies of which are continually in equilibrium with the energies of the outer environment as long as this system operates as a whole. In this light, all life, from the simplest to the most complex, is a long series of mechanisms for the maintenance of energy equilibrium with the outer world. (Adaptation to environment.)

A special laboratory in which all external influences, such as accidental sounds, light, physical commotion, or concussion, etc., temperature and air changes, can be absolutely controlled, was completed at the Institute for Experimental Medicine in Petrograd before the World War began, to enable Pawlow to continue the line of research.

During the World War and after the Russian Revolution, Pawlow could not execute effective investigations. In 1923 he visited the United States and I had the pleasure of meeting him again at Woods Hole, Mass. He there presented the results of his experimentation on many generations of *mice,* to ascertain whether acquired function (character) was transmitted by inheritance.

THE HUMAN WILL

In studying the personality of great medical thinkers and in endeavoring to understand the manner of operation of their minds the doctrines concerning the human will command study and consideration. This is one of the most difficult subjects in the entire field of psychology, if we wish to keep within the limits and bounds of physiologic psychology. Numerous writers on this subject appear to drift into statements which are not based on physiologic psychology. There are two essential moments in a will process, and hereafter I shall designate the will process by the term *volition.* The first moment is that of *anticipation* and the second is that of *actual willing.* To us there can be no act of willing, that is we cannot become aware of it without some outward action. The action is the outward realization of the anticipated objective content of the will act.

First, the *anticipation* (to feel, possess beforehand, to look for in advance). This means that a coming psychic experience is lived through in the present, and secondly, that a part of this experience that is to come is determined in the present time by the *ego.* The first content of the will act is called the *objective moment* and it includes the conception of aim and purpose, namely, that which was willed and the direction of the will—conception.

Secondly, the *actual moment.* This is the de facto "living-through" of the will act. It is the *realization* of *"I will."* These two factors, the *anticipation* and the *actual will moment,* require for their execution a third factor, the *will fulfillment* which is made up of complex reciprocal actions between the cerebral ratiocination and the many thought and muscular complexes requisite to carrying out a will. There are many methods of studying the will experimentally; *first,* the methods for the investigation of inner will activities. These are the *experiments on reaction,* in which a known or unknown stimulus is applied to a human subject who has to answer to the stimulus in a manner agreed upon. As the sign of the answer may be a movement of speech, it is evident that these reaction experiments are adaptive to the most manifold intellectual processes. It is about the best and most promising method to study the Will. A *second* method is called the *combined procedure* which studies the strength of the will, its power to overcome resistance. If we oppose artificial inner resistances to the realization of an intention or a purpose we can estimate the dynamic side of the will. The resistances have to be created in advance by artificially arranged associations, for which sometimes manufactured words are used (nonsense syllables). In my own experiments I have used inhibitive factors and resistances which I had to discover by a long study of the personality of the subject experimented upon. For instance in one case, I found in a lady possessed of an inherited aversion to cats, that the word "cat" immediately arrested the power of association and of will. The simple imitation of the cat's "meow" or the odor of a cat, produced the same effect. *The third* is the *continued series* method. These consist of an uninterrupted series of reaction experiments. When these experiments follow each other with great rapidity, the tension of the will and the concentration of attention is so forced that the will can only accomplish its purpose under great exertion. Chemical substances like alcohol, strychnine, bromides, tea, and coffee have been experimented with in their effect on the will, and it has been ascertained that there are types of human wills which are not favored in their reaction time by stimulants like strychnine, tea, or coffee, but are retarded by these substances and sometimes favored by bromides and substances that induce quiet and sleep, the so-called sedatives. The reverse is also the case. In the study of human master minds we must not forget that the will is capable of education, for it can be trained and thereby made a means for concentrated mental work. In scientific work, like experimental medicine, the will can be greatly influenced by the emotions, for instance, feelings of pleasure or displeasure. As a great many motives to our will are formed for the purpose of pleasure, or the removal of displeasure, it is a guiding line in historiography for the untangling of complex historic problems to know whether an individual undertook a certain will act for the purpose of experiencing pleasure or removing displeasure. Human beings that fall into this line of will action are known as *habit creatures*. The *motivation* of the will-determination which I have just spoken of, besides those concerning pleasure and displeasure, includes such motives as spring from intellectual deliberation. This kind of motivation is rare among our race. That which comes from pleasure or displeasure is the rule, and only subordinate to that form of motivation which for lack of any other explanation has been called "impulsive." There is also a motivation of choice between several alternatives. The eventual choice being dependent upon the ego, and such a choice is significant of the active participation of consciousness in the will activity. The so-called *free choice* in will activity is bound to deliberate reason.

One can call it free, only in so far as the ego can decide between the conditions that are simultaneously given. Those conditions that are given not simultaneously but in succession cannot be designated as subject to the free choice of the ego.

THEORIES OF THE WILL

These are four theories concerning the will, first the association theory, which reduces all processes of the will to association and reproduction and denies any specificity to will-experiences. According to this theory the will is nothing but a complication of sensations, conceptions, and feelings. Ebbinghaus, one of the adherents to this theory, defines the will as an impulse that has become previsioned.

Secondly, the intellectualistic theory, which sees the essence and peculiarity of the will in an act of assent or agreement.

Thirdly, the emotional theory, which designates the essence of the will as a process of emotion or feeling. All will-actions according to Wundt are affections, which bring about their own solution by their manner of progress.

Fourthly, the voluntaristic theory, according to which the will is something peculiar, a specific psychic experience, which cannot be explained by reference to sensation, conceptions, feelings, or association of these elements.

None of these theories are in agreement with the experimental investigations nor do any of them suffice to explain the specificity of volitional thinking nor of the doctrine of the personal temperament. So they are manifestly insufficient to explain mental qualities in the human genus. To my mind, it is futile to speculate concerning the various functions of the pyramidal cells and the other cell layers of the cortex until we have first established a definite co-relation of the function of these cells with specific acts of the will. At the present time this appears to be connected with unsurmountable difficulties, the chief one of which is that the experiments concerning the will have to be executed upon human beings in order to avoid the controversy as to whether or not animals have a will.

If future investigations on the histology of the cortex succeed in correlating definite functions of the will with specific cells or even only restricted areas of the human cortex, then a will must be denied to the animals because the histology of the cerebral cortex in animals is lacking the highly characteristic layers of cells which modern specialists in brain anatomy, like W. McDougall and Lewellys F. Barker, describe in the brain of the human being. It is in vain to speak of a psychic property like the will in such terms as this: "That underived and unshared power which, if it exists, constitutes the dynamic apriorism of the ego" (W. McDougall quoted by Charles Whitby in his "Makers of Man"—Introduction, p. 11) until psychology, physiology and anatomy have thrown more light on the connection of will and its anatomic substratum, the cells of the brain cortex.

McDougall is of the opinion that the activities of the neurones of the third or highest level have no immediate psychical correlates and serve merely to combine and coördinate the activities of those arcs of the sensory level that do contribute psychical elements. In other words, that part of the general brain-psychosis which goes on in these arcs of the third level is normally above consciousness, just as the somatic psychosis of the body in general is normally below it. The former belongs in fact to the trans-liminal or supra-liminal, just as the latter belongs to the subliminal self. But under exceptional conditions, in those moments of stress or exaltation, when, doubtless, new tracts are opened up in the arcs of the higher level, and new combinations effected between the subsystem of the sensory level, it may well be otherwise. Such moments are commonly accompanied by a feeling of expansion, a sense of enlarged power, which may plausibly be interpreted as the psychical correlates of the opening up of a fresh neuronal tract in the higher level, or the establishment of a fresh neuronal anastomosis. The feeling of expansion is no delusion, since, by the functional change that accompanies it, the mental or practical powers are in some degree permanently enlarged.

The action currents of nerves are accepted as indicative of function of the nerve. It has been found in the vagus nerve in the neck with every contraction of the heart.

PLAN OF PHYSIOLOGIC EXPERIMENT WITH A VIEW TO OBTAINING ACTION CURRENTS DURING WILL PROCESSES

Such an experiment can only be carried out on a human being who has undergone trephining of the skull for some accident and who has recovered from the operation. The accidental location of the place where the trephine has been made will determine which part of the cortex is exposed for observation. Now, my recommendation would be that the cortex which is exposed should be connected with an Einthoven string galvonometer. Let us assume that the wound permits access to the center of hearing, the superior temporal lobe (angular gyrus). The experiment should first be made with actual tone sensations, the striking of a tuning fork, for instance, and thereafter on another day the human subject should be instructed to imagine by his will that he heard a certain sound, or if he is capable of doing so, to recall his music-memory of an entire melody by the power of his will. Then the experiment should be carried out in the same manner on other subjects where the visual area is exposed (occipital lobes). The effort should be made to ascertain whether actual vision is associated with the development of action currents, and thereafter whether the will to imagine something as seen could produce the same action currents, the object of the experiments being not localization of function in the cortex, but whether cortical function produced by the will is associated with the same electrical changes as those which are produced by the normal, specific physiological stimulus. It is known that a nerve may become negative under two conditions, first, during activity and secondly, when dying as a result of injury. But as a so-called demarcation current is really an excitatory current, it is probable that the so-called currents of rest and currents of injury are really currents of action caused by the excitation around the injured spot. Therefore the caution must be taken that the wound is entirely healed and that there is no appreciable current before any experiment is undertaken. There are no experiments on record to my knowledge aiming at the determination of action currents in the intracentral cerebral paths. They would probably meet with too many technical difficulties, the chief of which will be found in the fact that there are too many reactions occurring in the association paths and in the various centers at the same time, and in such short periods of time as to make it technically impossible to record them. But if the experiment could be carried out as above outlined, it would demonstrate that the specific metabolic changes produced in definite tracts and centers of the brain by the normal stimuli coming in from the environment could be set up by an effort of the will. That such phenomena do occur was first confirmed by Johannes Müller in his investigation on "Phantasmen oder Halluzinationen." He gives a masterful definition in stating that they are sense perceptions from inner causes without external exciting objects. Such sense hallucinations are frequently observed in patients suffering from inflammatory conditions of the membranes of the brain or such as are in a state of delirium, as a result of chemical toxins. But they are known to have occurred in normal persons when they are in a state of transition from the state of wakefulness to the state of sleep, or vice versa. That the retina is not necessary for such phenomena is proven by the fact that they have been observed in people who had lost both eyes, and therefore the brain centers are sufficient to produce cerebral visual images. The entire subject of sense perceptions is one of the most important for the study and observation of great thinkers. These observations should, however, be made also in the primary schools on all children and careful records kept of them from year to year, so that the child records of all persons who attain to greatness would be at hand when the psychography of the eminent person becomes important enough to be studied.

PHANTASMS PRODUCED BY THE WILL IN NORMAL PERSONS

Among the rarest and most interesting visual hallucinations are those occurring in individuals that are intellectually and physically perfectly normal and possess the ability to see phantasms when their eyes are closed or when they are in complete darkness. These phantasms exactly correspond to the real objects. I know musicians who can reproduce an entire movement of a new symphony which they had heard but once. There are pianists who can reproduce compositions in all of their details. Johannes Müller described an interesting conversation which he had with Goethe in 1828 and which concerned the voluntary production of phantasms. He writes "I had an opportunity to discuss this interesting subject with Goethe as he knew that images occurred in my own eyes when I was lying down with eyes closed, but before falling asleep. I knew that I was not asleep because I frequently studied these images. Goethe was very eager to find out how these images were formed in me. I explained that I had no influence of the will on their production and transformation. . . . But Goethe could indicate the subject voluntarily and then the transformation of the conceived subject followed in a seemingly involuntary manner, but perfectly lawfully and symmetrically." This was the difference between two human natures, one of which possessed the greatest abundance of poetic formative power whilst the other was directed to the objective investigation of reality, and that which was going on in nature.

In the beginning of this chapter, the subconsciousness as an element of the mental equipment of great medical thinkers was considered. In connection therewith, I desire to call attention to an expression of one of the greatest physicists that ever lived concerning the nature of life, Hermann von Helmholtz in his "Vorträge und Reden," (Vol. II, page 238): "I fail to see how one could disprove a system of even the most extreme subjective idealism which conceives life as a dream. One could declare it very improbable and highly unsatisfactory and I would probably consent to the severest expression of rejection of such a view. But it would be capable of logical execution and it seems to me very important to bear this in mind."

SEEMING WILL-ACTIONS DUE TO SUBCONSCIOUSNESS, REFLEXES, TROPISMS AND INSTINCTS

In his work entitled "Introduction to the Comparative Brain Physiology and Comparative Psychology" Jacques Loeb has a chapter entitled "Criteria for establishing consciousness in lower animals," page 139. He makes plain here that consciousness should only be expected where associative memory can be demonstrated. By associative memory we understand that peculiarity of living things by which they respond to a causative stimulus, not only by effects that correspond to the specific structure of the irritable apparatus, but also by such other effects as are due to secondary causes which evoke results that have before, in some preceding period of the life of the living creature, been caused by the same or a similar stimulus when it had been applied to the organism. For example, the odor of a flower may evoke the vision of a landscape, in association with which that same odor had been previously brought together. Our memory must have the peculiarity of retaining traces of processes which occurred simultaneously or nearly simultaneously at some previous period, not only retaining them, but also of fusing them into unity. We even have to conclude on an associative memory, if a dog or cat responds to the calling of its name. This associative memory has been even attributed to plants but Loeb points out that if a plant cultivated in the Tropics does not endure low temperatures of the North, it is not exactly associative memory but might be due to chemical processes in the plant, because a tropical plant is different chemically from a plant which grows in the North. In this plant a definite causative stimulus does not bring about in addition to its own effects other entirely heterogeneous effects, simply because in the previous history of this individual plant, the secondary or heterogeneous effect had already occurred with that same result, for the plant had not previous to the causative stimulus ever experienced the application of the same causative stimulus. It is therefore not correct to speak of plant memory as we do of animal memory, the criterion of the difference being that the living creature reacts to a specific stimulus, not only by the limited effects of that stimulus but also by other effects which are the consequence of a residuum of a previous experience with the same stimulus.

Only the demonstration of an associative memory can be considered as a criteria of consciousness and intelligence. This is also one of the criteria in the distinction between impulsive action and action due to reason in human beings, and it is here that masses are as a general rule distinguishable in their actions from the actions of the individual. Other things being equal, the action of the mass is very liable to be impulsive, not governed by associative memory, but the action of the individual is more likely to be reasonable and intelligent. This is an important distinction to make in facing causative factors of history and also in not attributing too much importance to the effect of masses and groups in the promotion of scientific purposes. Conscious willing is a function of associative memory.

For history these observations teach us that there are will-actions which are only apparent and not real will-actions. They occur without coöperation of conscious thinking. Such actions among animals are designated as instinctive. The usual definition of instincts is that they are actions that are inherited by the practices and experiences of the previous generations of any animal. Jacques Loeb has demonstrated that actions that are outwardly indicative of well-defined purpose can be reduced to unconscious processes and devoid of all will function.

W. McDougall in his "Physiological Psychology" expresses a belief concerning the various layers of the gray cortex of the brain which is very interesting and has been made use of by numerous students of human character, in order to explain what they conceive as consciousness, superconsciousness and subconsciousness. McDougall's classifications of histological elements permits of the inference that the histological layers of the gray cortex correspond to functional differences, for example, it might be inferred that the neurones of the third or highest level have no direct or immediate psychical correlates and serve merely to combine and coördinate the activities of those parts of the cortical apperceptive levels that actually do contribute psychical functions. There are three histological strata that correspond respectively to consciousness, super-consciousness, and subconsciousness and the arcs of the third level are normally above consciousness, just as the autonomic and somatic nerve processes of the body are generally below it. The architecture of the human cortex has been very thoroughly investigated but the association fibers that connect them and also connect the end organs of the senses with different parts of the cortex have not been so exhaustively studied with regard to their genetic origin. The cause of this is to be found in the fact that the relations of the various cortical centers to each other can only be studied on man himself either by the Wallerian degeneration method or the Flechsig Myelination method, that is, if we intend to get beyond the physiologic results thus far obtained on animals. In this connection I desire to call attention to new tracks of association fibers which must be developing in man at the present time between the external receptor apparatus for body equilibrium and orientation, and certain central cortical areas. Personally, I should expect that the brains of professional aviators would show these new tracks where the brains of men who have never been in an aëroplane would not show them, and in case of marriage between male and female aviators it could be ascertained whether such tracks are transmitted by inheritance to their descendants.

XXXII

HEREDITY, EUGENICS, AND GENIUS

IN 1873, Alphonse de Candolle published a work with the title *Histoire des sciences et des savants depuis deux siècles, suivie d'autres études sur des sujets scientifiques, en particulier sur la selection dans l'espèce humaine.* Since then numerous valuable treatises on this subject have been published. Most of this literature can be found in the appendix to Professor Edwin G. Conklin's book on "Heredity and Environment in the Development of Men," Princeton, 1919. Historically, Sir Francis Galton, quoted on page 328 of Conklin's references, has given the greatest impulse to the study of eugenics. Recent works that appeared in 1924 are "L'heredité," by E. Guyenot, Paris, 1924; "Heredity and Eugenics," by R. R. Gates, New York; "Outline of Genetics, with special reference to plant material," by M. C. Coulter, Chicago.

Concerning the inheritance of acquired characters, see the recent work of Paul Kammerer, New York, 1924. Here this author brings a literature on the transmission of acquired characters from pages 369–404. In spite of the great amount of experimental and general scientific work that is done on this subject, it is still impossible to write a critical history of human heredity because of the incompleteness of confirmed and reliable data.

In a study of the master minds in medicine, I feel it a duty to call attention to the fact that the first to announce the doctrine of pangenesis was Hippocrates. The conception of the word is usually attributed to Charles Darwin who knowing that the personal properties and make-up of the parents were transmitted to the child, and who believed in the transmission of acquired characters, erected the doctrine of pangenesis. The word literally means "everything generates." He actually says, "I venture to propose the pangenesis theory which means that every single part of the entire body structure propagates itself, so that ova, the pollen of blossoms, the seed, bud-cells of different organs, are composed of a large number of gemmules derived from every particular part or unit of the body." By units Darwin understood the cells as they were defined in Virchow's "Cellular Pathology." Darwin states definitely that all personal properties are normally inheritable. Such a conception naturally had to be the premise of his theory of selection. Without such a premise, selection loses its significance. Darwin was not an original investigator of heredity. His cross-fertilization experiments are of interest, it is true, but he based his work on the results of other investigators, particularly on Naudine and Haeckel. It is regrettable that Darwin overlooked the fact that Aristotle criticized and rejected the Hippocratic pangenesis theory.

The only new conception that Darwin gave to this ancient hypothesis was that the presumed gemmules derived from all the cells of the body were transported through the blood channels of the animals. This is the most distinct symptom of his conception of heredity as a process of transportation, and it is through Darwin's influence that this conception still commands the methods of thinking of certain biologists. His own cousin, the celebrated statistician, eugenist and anthropologist, Sir Francis Galton, attacked the theory of pangenesis experimentally. If the Darwinian gemmules were transported by means of the blood, then the transfusion of blood of a certain animal species into the blood channels of the individuals of another species should influence the structure and constitution of the descendants of this individual. The experiments with rabbits of different color all gave a negative result. From now on the history of heredity investigations is one of controversy between the theory of Nägeli, the idioplasma theory, and Darwin's theories that have just been referred to, on the one hand, and Weismann's theories concerning the germplasm and the inheritance substance contained in the germ cells, in which he conceives all the *Anlagen* of the future organism to be contained.

Each one of the inheritance research workers has felt at liberty to coin his own words, and as yet the uninitiated non-biologic person who is eager to learn something about the subject has to familiarize himself with the vocabulary of each one of these geneticists. Nägeli conceived that the structure of every cell consists of two entirely different substances which he calls idioplasm and nutrition plasma. They are arranged in a definite manner to each other within the cell. The idioplasm permeates the nutritional plasma in form of very fine strands. The idioplasm is the more highly organized constitutent and is the bearer of the hereditary qualities. The idioplasm therefore has a very complicated structure. Its ultimate and smallest particles are designated as *micelli*. The entire body is permeated by an extremely fine network or scaffolding of the idioplasm, and any stimulus or irritation which strikes the body in any part must necessarily strike the idioplasm. One sees how close Nägeli's theory of idioplasm and Darwin's theory of pangenesis are related. According to Weismann's theory, the *Anlagen* of the germ cells must be very numerous and they again are conceived as being represented by many different minimal particles. In every germplasm there must of necessity be as many of these ultimate tiny particles as there are independent and variable regions in the complete organism. Between these ultimate particles of the germplasm and the individual parts of the body of the complete organism definite relations exist, in such a manner that no part of the organism can come into existence without the coöperation of these ultimate particles of the germplasm. In truth, the parts of the complete organism, their very existence, is determined by these ultimate particles in the *Anlage* of the germplasm, and therefore these particles are designated as *determinants.*

We have already observed that the germplasm is conceived by all thinkers on heredity as composed of an infinite number of minute particles which, unfortunately for our understanding and memory, are designated by numerous names. Darwin calls them *gemmules,* Dalton speaks of *stirps,* Nägeli terms them *micelli,* Weismann calls them *determinants,* Hertwig calls them *idioplasts,* and just as the physicists and chemists have to revert to molecules, atoms, and ultimately to electrons, so the biologists, who occupy themselves with problems of heredity, must occupy themselves with these small particles of the germplasm. They are the biologic units that bear the qualities of inheritance.

The word "gene" demands definition here also. It comes from the Greek word *genesthai,* which means to be generated. Genes are the heredity *Anlagen* contained in the inheritance material of the sexual cells. These sexual cells are called gametes. Gametes may be male or female and in order to form a new individual, the conjugation of two opposite gametes is necessary. These different gametes are usually spoken of as sperm and ovum and their union is spoken of as the zygote. The word gene signifies that the qualities of an organism are conditioned by special independent factors or units in the constitution of the gametes and zygotes.

If we review and compare all the theories of inheritance they could be arranged in two groups. In the one group the theories of Darwin and Nägeli could be classed and these express that the relation between the body mass which is spoken of as the *soma* and the inheritance substance are reciprocal. The inheritance substance by its activity produced the body or soma and this in turn gave up to the inheritance substance what it received in impressions from the environment. Weismann's theory is entirely different. The activity of the inheritance substance, the germ plasm, it is true, did build up the soma or body, but with this act all relations between the two were exhausted. Whatever had originated from the germplasm was something that existed for and by itself. There were no more regressive relations between the soma and the germ plasm. Of all the influences of the environment which the soma experienced during its individual life and through occurrences in its own inner life, nothing ever exerted an influence on the germplasm. The relations are not mutual as in Darwin's and Nägeli's theory, but only one-sided. They go from the germplasm to the soma, never in the reverse direction. According to the ideas of Darwin and Nägeli, all the qualities that are acquired by the soma must occur in the descendants, because the inheritance substance, (genes, determinants,) etc., is, as we might say, infected by these new acquired characters. According to Weismann's theory such qualities never appear in the offspring.

Having reached this point, we have come to the problem which has occupied the science of heredity in the most intensive manner for the last twenty-five years. The problem of the transmission of acquired characters opens up before us. The question is, can characters or qualities which can be demonstrated to have been acquired and originated in a generation of parents, recur in the descendants without these having been exposed to the same stimuli and the same modifying influences of an environment? There can be no problem of greater importance concerning the human race. None of the advances in civilization or culture and none of the discoveries of science can equal this problem in its pragmatic importance for the human race. The truths which have been exposed by Gregor Mendel, Francis Galton, Thomas Morgan, Bateson and the American School of Geneticists have a far greater practical import for humanity than Newton's discovery of the laws of gravitation or the discoveries of Kopernicus concerning the revolution of the earth around the sun. The problem of the inheritance of acquired characters is of inestimable importance for the production and recognition of master minds in all sciences, and the same problem has developed entirely independently of experiments in crossing. It is a much older problem than any other problems in heredity. If we formulate modern doctrines of heredity with regard to this problem, we must ask the question whether the genotypic basic *"Anlagen"* contained in the germplasm can be modified or not in their inheritance potentials by stimuli (chemical or physical) that emanate from the parental body. The standpoint of the modern theorists on heredity—and a great many of them are personal friends of the author—is that of rejection of this idea. Everything that happens in the processes of heredity is by them attributed to the genotypic constitution. This constitution works unchangeably in generations when they are conditioned in a homozygote manner. But with continued crossing this constitution can be changed when it is conditioned in a heterozygote manner, for in the latter case division and new combination of the genes contained in the genotype must result, and this leads to new forms, races or species. The ultimate unity of the whole process continues unchangeable, that is, the genes. The consequential theorist of heredity concedes no transformation, no new formation, no new creation of genes; therefore no new form, for instance a new species, can arise through the intervention of a new positive gene. The stumbling block with this dogmatic attitude is the well-known fact of the mutations. The positive theorists of heredity which we have just described dare not concede a change of the genotype nor any definite process of new formation. In spite of all arguments to the contrary, there are sudden new characters that appear in single living individuals, not acquired by a slow continuous process of variation, but appearing spontaneously and suddenly, and the important points about these new characters are that they are transmitted to their descendants by inheritance from the time of their first appearance.

These mutations have given the theorists of heredity much difficulty to explain, some of them going so far as to suggest that these sudden mutations are due to a negative process, caused by the loss of genes. The difficulty with this explanation is that it requires other explanations, namely, how can positive acquirements be due to a loss of some preceding cause. Common sense compels us to believe in evolution and the difficulty is that we cannot make clear to our mind in a phylogenetic sense that evolution could be caused by the continuous disintegration of the original inheritance substance. In other words, the ultimate consequence of this last theory would be that adherence to it would compel us to deny all evolution.

NEOTENY OR SALTATION OF AXOLOTL INTO AMBLYSTOMA AND REVERSELY BY CHANGE OF ENVIRONMENT OR THYROID SUBSTANCE

The heredity substance can be influenced by external stimuli that affect the individual and then hereditarily the progeny of this individual, and this change occurs by mediation of the encrocrine glands. It is known that many animals do not develop directly from the ovum into the adult form, but between these two stages they pass through intermediate or larva transformation, for instance the development of the butterfly from the ovum to chrysalis, to caterpillar, to butterfly. Another example is the development of the frog from ovum through the tadpole to the adult. It has been observed that the intermediate or immature stages are retained overly long and sometimes even the complete metamorphosis into the final stage is omitted. Such manifestations are designated as neoteny, which means retaining the youthful or immature stage.

A very curious neoteny has been observed in that the animals do not only fail to pass into the final stage, but that the retarded growth does not hinder maturing of propagation. Complete neoteny checks differentiation, but not the gain in size. The transformation of the axolotl which is found in the lakes of Mexico into amblystoma has been observed first by Marie von Chauvin in 1885. This salamander which has a flat tail usually matures in water and retains its gills. In taking recourse to all kinds of artificial means, Miss Marie von Chauvin succeeded in forcing the transformation of axolotl which normally very rarely occurs—into a full fledged newt. The bottom of the aquarium slanted upward to make it easier for the animals to leave the water entirely. The water had been boiled beforehand so that the animals did not get any air through their gills and were forced to develop lungs for respiration. But the animals which successfully metamorphosed into the land form begot a second generation which underwent these changes without being forced to do so, provided that at the moment when the animals were ready to metamorphose an opportunity was given for them to leave the water.

Through the experiments of C. Jensen, Kaufman, and Laufberger, it has been discovered that even the most stubborn axolotl larvae may be induced to metamorphose by feeding them with fragments of thyroid gland. The thyroid of any animal will suffice. In the common triton salamander or frog larvae, when fed with small doses of thyroid gland, it is impossible to forestall the reduction of their gills and fins, or hemming on the tail, or the development of their lungs, or anything else which belongs to the complete metamorphosis of the aquatic form. Such animals drown if not placed on land in time. Similar effects have been observed by feeding thymus glands.*

I cite these examples of retarded metamorphosis as historic biologic evidence suggesting that similar transformation may be effected in the course of other biologic experiment, that even in the human being the so-called dormant or latent capacity which may be hidden even for one generation, and spring out into the next, may be a modified state of a similar process.

The theory of the transmission by inheritance of somatogenic qualities requires a series of highly complicated processes which are at the present time exceedingly difficult to conceive. I mean the physiologic processes underlying them are not clearly understood. In the case of axolotl and amblystoma, the opponents of the theory assert that the primitive axolotl, the ancestors of all those that live to-day, descended from normal amblystomas that did not become capable of reproducing after metamorphosis until they reached the land. The dropping out of so-called neoteny is nothing but a reversion to type, and Miss Chauvin may have practiced an unconscious crossing and selection experiment of the best fitted pairs for her inquiry.

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* Edward Uhlenhuth's investigations present this subject in a clearer light. See his research on *Relation between Metamorphosis and other developmental Phenomena in Amphibians.* Journ. of Gen'l. Physiol. May 20, 1919 (vol. i, p. 525). Also the *Relation between Thyroid Gland, Metamorphosis and Growth.* Journ. General Physiol, March 20, 1919. Vol. I, No. 4, p. 473.

A very deep insight has been won by the many studies and researches on the mechanisms that control the qualities of the organisms which are inherited. But to the questions where these qualities come from, how they have originated, whether and in what manner they could arise anew, neither breeding experiments nor crossing experiments can give us any information. We have to bear in mind in all experiments on genetics that they are influenced by two theories, the theory of preformation and the theory of *epigenesis*. The pre-formation theory assumes that the parts and constitutents of the adult organism were contained in the germplasm in the same arrangement of organs and parts of the soma, as they exist in the completely developed state, so that development was nothing more than the unfolding and growth of a preformed organism which was a priori given with its entire constitution in the germplasm. The epigenesis means that the germ when it was once started as a zygote, consists of absolutely undifferentiated and unorganized substance, so that in the germplasm there is nothing of the later organism, and everything that distinguishes the new complete ogranism is the result of forces that become active after fertilization. Both theories proved untenable when they were consequentially carried to their ultimate possible conclusions. Still the experiments of some geneticists go beyond their scope when they suggest that all conceptions of evolution are without meaning. Doctrines of inheritance, it is true, can only be a link in the fitting together of the great theories of evolution. They cannot exist usefully by standing outside of the theories of evolution or above them. At the present day one can see that the most valuable and exact advancements made by the methods of crossings and breedings have been completely crushed by the prodigious and continually increasing burden of hypothetical speculations that have been heaped over the experimental results, and as many of the precipitate and hypothetic speculations are not well founded, they threaten to disorganize the entire structure of genetics. It will be necessary to cease from speculation and return to experimental biologic research concerning evolution, research based upon the most exact and controllable methods that the human mind is capable of designing.

America has two institutions for the study of genetics, but we need more particularly an institution for the study of the biology of races or anthropology, investigated from the standpoint of the inheritable differences of mankind.

'EUGENICS

The most stable form of matter that has so far been discovered is that upon which heredity depends. Consequently the selection and multiplication of the best heredity should be the chief aim of every civilized nation. Henry Fairfield Osborn in his address before the Second International Congress of Eugenics, *Science,* October 17, 1921, states very truly that the 500,000 years of human evolution under widely different environmental conditions have impressed certain distinctive virtues as well as faults on each race. In the matter of racial virtues, his opinion is that from biological principles there is little promise in the melting pot theory. Put three races together and you are as likely to unite their vices as their virtues. For the world's work, he continues, give us a pure blooded Negro, a pure blooded Mongolian, a pure blooded Slav, and a pure blooded Nordic, and ascertain through observations and experiment what each race is best fitted to accomplish in the world's history. If the Negro fails in government, he may become a fine agriculturist. The Chinese and Japanese have demonstrated in there respective countries a range of ability in art, literature and industry quite equal to our own, and greatly superior to our own in ceramics, and Ranke in the eighth volume of his *Weltgeschichte,* page 176, states that at one time the Asiatic culture (Chinese) was the oldest and, as far as history can ascertain, the most original epoch of culture. But that culture has steadily gone backward. The oldest Asiatic culture was the highest; then came a second and third epoch in which the Greek and Roman element dominated in Asia, and recent historic research indicates that these civilizations derived more good from the Chinese than the Chinese from them. The third epoch is one of degeneration and was signalized by the invasion of the barbarians, namely, the Mongolians. The idea that there is a progressive evolution of culture, homogeneous and contemporaneous through all the centuries and comprehending all human knowledge and ability, is an error.* Even in modern times the fifteenth and first half of the sixteenth centuries stood higher in general culture than the last half of the sixteenth and the end of the seventeenth centuries. It was not until the nineteenth century that there was a more general rise of culture.

Eugenics, by which we mean the improving of the human stock, indicates a new method of adding to human welfare, especially if it is combined with an equal effort for improvements in human environment. This science encourages the hopeful prospect of rising culture more truly than anything which has yet been tried in the entire history of the world. It seems to me that the controversy between Lamarckians, like Kammerer and Redfield, and the professional geneticists of the evolutional type, may eventually reach clearness in such conclusions, in which the neutral observer may recognize that the differences between the two have been more apparent than real. Kammerer insists that race hygiene of the future will be more dependent on the environment than it will be on the conditions of inheritance, and Casper L. Redfield has written very ably concerning the evidence of the transmission of acquired characters. It impressed the author very deeply to hear the great Russian physiologist, I. P. Pawlow, range himself on the side of those who believe in the transmission of acquired characters by heredity, and that at a recent convention of the American Association for the Advancement of Science, December 27, 1924, in Washington, M. F. Guyer brought forth further evidence concerning the transmission of acquired characteristics. Pragmatically it seems to me that the human race at the present time is more capable of influencing environment than it is of influencing the germplasm, and therefore the opinions of Kammerer and Redfield are more capable of being tested by prolonged experiments. Genetics is the pure science which deals with heredity, the very foundation upon which eugenics is based. Leonard Darwin, in an address before the International Congress of Eugenics (*Science,* October 7, 1921), is of the opinion that it is impossible to conduct the necessary breeding experiments on human beings, and therefore genetic research must largely be concerned with the lower animals and plants, whilst eugenics is concerned with human beings alone.

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* James J. Walsh has argued successfully that the thirteenth was the greatest of all centuries (New York, 1924, Catholic Summer School Press).

The pure science of genetics and the applied science of eugenics cover different fields, but the boundary between them is movable and not absolute. The main aim of eugenical societies should be to formulate sound eugenic policy based on existing genetic knowledge and then to promote the transference of every advance in eugenic theory into general practice.

We do not possess sufficient comprehension of the mechanism of heredity at the present time for exact experiments on human genetics, but the exigencies of human life demand practical results of eugenics at the present time, and as the conditions of environment are more readily influenced by man than the mechanism of heredity, it seems more promising to work along lines that can be subjected to exact experimentation at the present time, always ready to experiment with the mechanism of heredity whenever any hopeful results can be expected therefrom.

I will now review the principle objections to the hypothesis of the transmission of acquired characters.

HEREDITY AND GENIUS

There are numerous reports of the transmission of acquired peculiarities extant in the scientific literature. Nevertheless, the correct interpretation of these cases, surrounded as they are by many obscure conditions, is seriously questioned by a large number of scientists. At any rate, experiments conducted to establish the principle of heredity seem to point to a repudiation of the theory. For instance, the effect of the transference of high altitude plants to a low altitude, and the subsequent return of the same to high altitudes again, e. g., the furry covering of the lion's foot (Edelweiss), lost in the low altitude, returns when the plant is transplanted again to a high altitude.

Some observations tending to prove the theory are as follows: reports to the effect that a form of epilepsy produced by an injury to the nervous system of the guinea-pig again appeared in the young; similarly, in the case of cats which had lost their tails, of human beings who had lost a few fingers, of salamanders whose reproductive power had been lessened through a reduced water supply. The most remarkable case is that of the bear-caterpillar, which, when subjected to abnormal temperatures, develops into a butterfly of distinctively different coloring from the type he originally came from.

In refutation of such evidence, attention is called to the many national, cosmetic and ritual mutilations of human beings carried on through many thousand years, the flattening of the frontal bones of the flat-head Indians, piercing of the ears of girls, piercing of the nose-septum, foot mutilations of the Chinese women, circumcision of many Oriental peoples, filing off of the teeth of African tribes, the long practiced cropping of the tails of domestic animals, none of which are transmitted by inheritance.

Opponents of the hereditary theory hold that all the data so far offered to prove same are incomplete and unsatisfactory. They insist that in the case of the bear-caterpillar, the temperature must have affected the germ cells, whereas in the case of injuries or mutilations, it could hardly be assumed that the germ cells had been affected at all, which fact would, however, be the prime condition necessary for the transmission of any peculiarity.

Nothwithstanding all such arguments in contravention of the theory, one cannot help regarding it with some favor. One meets with so many phenomena in the human and animal body which are intimately associated with the use and non-use of organs, external stimulation, etc., that one can only explain their origin on the basis of functional adaptation and their retention by heredity, in a much more satisfactory way than by the slow-working principle of natural selection, dependent as the latter is on all sorts of contingencies. One could thus comprehend, in view of the complexity and mysteriousness of the bodily activities and their manifold obscure interrelations, how a persistent stimulation, extending over many generations, could, while inciting some organ to a purposeful reaction, finally reach the germ cells themselves, making the adaptation hereditable. The acceptance of such an explanation would not be difficult, in view of the daily occurring cases of functional adaptations of *individual* members of a species. This would explain the negative result in cases of experimentally produced bodily changes and the mutilative customs of races, and on the other hand, would account for the isolated cases presented in evidence. It would go to show that the probability of transmission of injuries and mutilations in the case of the human species are practically zero.

To show how the force of adaptability operates under changing conditions, a few cases are appended: the complete or partial stunting of the eyes of animals which live permanently in the dark. Natural selection could not explain the phenomenon of the formation of the "spongiosa" in the bone structure of various animals, which formation assures the greatest amount of strength with the least possible weight. In the case of flying animals (birds, bats, antediluvial flying reptiles) the spongiosa is even absent, the space being occupied by air. In quadrupeds that walk on all fours, the spongiosa is formed differently from that of such animals as walk on their rear extremities, to meet the different conditions of weight and strain of the body on its carriers. It has been shown that when persons have sustained a break of the upper end of the thigh bone and the bone has wrongly knit, the latter would eventually, when put to use, rearrange its structure to suit the new conditions. This has been conclusively shown by means of post mortem examinations.

All the above reasons have led some biologists to accept the theory of hereditary transmission of acquired characteristics (functional adaptation) together with the proven heredity of spontaneous variations.

Just a few words about evolutionary versus epigenetic heredity. The former presupposes that every part of the fully formed body had originally its infinitesimal counterpart or element in the germ itself. These elements have been called *determinants.* The epigenetic theory accepts as a principle the development from the simple forms to the more complex without there being originally a necessary counterpart in the germ to every part in the fully developed organism.

TRANSMISSION OF DISEASES

As to the transmission of diseases, let us first understand that only characteristics, and not all at that, are transmittable, but characteristics are made hereditable by bringing about a change in the germ cell itself. Now a disease cannot be considered a corporal characteristic or peculiarity, inasmuch as the latter has to do with organic structure, but the disease has obtained its hold through a foreign element, the disease-agent. It has been shown that many diseases are caused by small organisms (bacteria, protozoa, etc.) which form poisons in the body. The effects of these organisms together with the reaction set up by the body cells bring about the phenomenon called disease. Where there are no such organisms, or where their virulence has been nullified, there is no disease. This will explain just why a disease is not hereditary, because the condition would be entrance of the disease agents into the germ cell itself and remain potent in the same. In such an event, there would be no true case of heredity, but rather an infection of the germ cell.

The two diseases which are generally thought by the popular mind to be hereditary are syphilis and tuberculosis. It must now be understood why they cannot be truly hereditary. With respect to the former, it should be said that when a child has shown symptoms of disease, it could only have acquired the disease through placental infection. It will be reassuring to understand the fact that a mother who has been a victim of the disease, but cleansed of the germ causing it, cannot transmit it to her progeny. Likewise, tuberculosis is not really hereditary but where in certain cases young children have been infected, the infection must have been brought about either in above manner, or perhaps after birth, from one of its parents or both, through association.

As to the heredity of predisposition to any disease, practically nothing is known: but valuable suggestions are made by three authors in *Archiv. für Rassen u Gesellschafts-Biologie,* Band 13, Heft 1, 1918. Fritz Lenz —"Über dominant-geschlechtsbegrenzte Vererbung und die Erblichkeit der Basedowdiathese" (p. 1). Dr. Jens Paulsen in Kiel-Ellerbek "Über die Erblichkeit von Thorax-anomalien mit besonderer Berücksichtigung der Tuberkulose." Dr. med. K. Classen in Grube (Holstein) "Vererbung von-Krankheiten und Krankheitsanlagen durch mehrere Generationen." *Julius Bauer*-Die Konstitutionelle Disposition zu inneren Krankheiten 3d Edit. Berlin 1924 S. 12–20. *Paul Kammerer* Inheritance of acquired characters New York 1924 P. 193–356.

According to Lenz l. c., a series of hereditary abnormalities and diseases are known, the progression of which follows Horner's Law, i. e., the disease passes from the grandfather through the daughter, who remains intact, to the grandchild. Women are only exceptionally afflicted with these abnormalities. The diseases can pass in a latent manner through what is known as predisposition, only through the female sex. This predisposition is spoken of as existing under two forms: (1) latent (2) manifest. It cannot pass either latent or manifest from father to son. Among these defects are the following: (a) red-green blindness; (b) inherited atrophy of the optic nerve; (c) myopia existing with night-blindness; (d) albinismus of the eye, existing conjointly with nystagmus; (e) progressive muscular atrophy.

Hereditary progression is explainable if we conceive the abnormalities mentioned as dependent on defects of the sex-determining hereditary units. It is well known that in the human being, sex is determined by a hereditary unit which in the female sex is "homozygot"; in man "heterozygot." Man receives this sex-determining hereditary unit, in each and every case, from the mother; the female receives one of her two units from her mother, and the other from the father. If we designate the sex-determining unit as capital W and its antagonist in the male sex as small w, then the formula for the sex determination is the following:

Man × Woman
Ww × WW
Gametes W, w × W, W
Descendants WW = Woman
Ww = Man

A defect in the W unit does not express itself immediately in the female sex, because the second W unit covers it over in a compensatory way; but in the male sex, a defect in the W unit leads to a manifest infirmity, because here the second W unit which could intervene in a compensatory manner, does not exist.

The relation of disease to heredity is one of the greatest importance in the culture of eminent individuals because a disease that is dominant in the parents should compel consideration with a view to possibly eliminating it, irrespective of whether the offspring will have the qualities for making an eminent thinker or not. Professor Leo Loeb defines three possible classes of diseases as far as heredity is concerned (Scientific Monthly, June, 1923, pp. 573–576). In the first case the germplasm of an individual may be normal and therefore have a satisfactory hereditary endowment, but certain injurious influences attacking from without and causing directly or indirectly a disturbance in the structure, composition, and functioning of the organism, such detrimental influences might be caused by the entrance of infectious micro-organisms, by the absorption of certain poisons, and by the extremes of temperature in the environment. I should add that such poisons may arise within the body itself. They do arise in the condition known as Basedow's disease where the thyroid gland becomes the source of toxic substances that can affect the entire autonomic nervous system. It is also important to call attention to the fact that this class of diseases may be the consequence of severe mental strain, persistent emotional disturbances, in short, all prolonged tension upon the mind and brain of the individual. German statistical records show the increase in hyperthyroidism during and immediately after the World War.

The second class of diseases result from an altered constitution of the germplasm which is transmitted from generation to generation. As germplasm always interacts with certain factors of the environment, the latter also enters into this class of diseases as a component cause. Leo Loeb distinguishes between environmental factors that are constant and fixed and not susceptible of alteration, and therefore not really causes of disease, in the same sense that heredity can be the cause, and secondly environmental factors which are variable, namely, they can be diminished, increased or entirely eliminated, and the disease will vary accordingly.

The third class comprises hereditary disease due to injured germplasm in association with environmental factors that are variable, as just described. Individuals of the third class may appear perfectly normal under a healthy environment, but if a surplus or deficiency of environmental factors occurs, disease results. Healthy persons would show no injurous reaction to such condition.

Leo Loeb classes the causes for deterioration of the germplasm into four groups, 1) the sudden appearance of mutations which he conceives as due, in some cases at least, to loss of some particle of the germplasm 2) the action of chemic substances upon the germ plasm. Stockhard has shown that if guinea pigs are subjected to the action of alcohol for a long pediod of their life, the offspring of successive generations will show definite inferiorities as to fertility and bodily vigor. It is true that members of later generations that have not been exposed to the influence of alcohol show these same hereditary defects that are transmitted as well by an alcoholized father as by an alcoholized mother. But it has also been found, that alcohol by weeding out the weakest germ cells, may eventually lead to the survival of a relatively strong individual. 3) In this group are classed the more specific deteriorations affecting only certain organs. Parts of the germplasm are so affected, that in the offspring similar defects in that particular organ, and only in that organ, appear which was diseased in the parents. This refers to the experiments of M. F. Guyer who experimentally produced defects in the eye of rabbits from generation to generation. 4) This group comprises those hereditary deteriorations of the germplasm which are due to continued in-breeding.

Loeb prefers that the problem be attacked more by spreading knowledge of hereditary transmission through education rather than by a weeding-out process, such as was constantly practiced by the Spartans.

The main reason for considering eugenics and inheritance in connection with master minds in medicine is to ascertain whether by a study of these sciences the number of master minds could be increased. Genius can be acquired only by a few individuals in any generation. The figure that Galton gives is one in a million, but at the present time, in the entire population of the earth this proportion is not maintained. The expression that in each generation there is but one genius is more correct. The efforts to favor the development of genius have met with the objection in a popular magazine: "What kind of a world would this be if everybody was a genius?" Doubtless it would be a very uncongenial kind of a world, for the various kinds of geniuses would find no field for the pragmatic application of their power. If everybody were a genius, there would not be sufficient contrast between the individuals to make the term "genius" justifiable. The rarity and non-inheritance of genius is explained by a variety of facts and circumstances, first, that genius is not prolific in its descendants. The tremendous brain exertions necessary appear to prevent large families. Secondly, genius is no elementary or fundamental characteristic but rather a combination of qualities which only fortuitously occurs in one individual. According to Mendel's law of segregation, the rare complex of characters in the great individuals is again separated in its original elements in the descendants; that is, provided the genius begot progeny. Another reason is that the marriages of eminent master minds are frequently not with women of equal mental endowment. A further reason is that the human race itself has opposed such obstacles to the development of genius as to crush their growth and development. Genius must be given a fair chance. It is not a blessing to possess rare mental endowments, gifts, or talents, when the contemporaneous human beings are jealous of them or for social, political, or religious reasons place obstacles in the way of the evolution of these mental gifts. It might as well be remarked here that even the management of universities is not exempt from prejudice towards the eminent thinker. Ostwald in his *Grosse Männer* gives several examples of this. I know of one instance in which a distinguished clinician called to a new university in another country placed eight of his own countrymen in favorable positions and evidenced antipathy to the local talent in the city where he was placed.

It is frequently asserted that genius will assert itself under any and all circumstances, even where the eminent individual's health is taxed by deprivations and inhuman treatment. But history of these great men shows that while a limited degree of adversity and deprivation is a stimulus to extraordinary brain function, yet genius must be given a fair chance. Of what benefit is the lofty flight of ideas to a poet who is compelled to sweep the streets or tinker in a workshop? The centuries have brought forward not another cobbler-poet since Hans Sachs. There should be capable geneticists and eugenicists employed in every public school system to train the teachers in the methods of observation and recording the characteristics of children as to their fitness for certain directions of training and also as to their incapacity for other directions of training. These eugenic studies should not be limited to the classroom nor restricted to recitation, etc., but the same children should be watched at their games in the gymnasium, in all their favorite pastimes, and the parents should be invited to record and report the conduct of the children at home. As this, however, would depend entirely on the consent of parents and children, it would have to be done with much circumspection and tact, for nothing can be accomplished by force, and the secret of success in this line is coöperation and good-will.

XXXIII

CONCLUSIONS

THE philosopher Boerne has said that the only constant thing in life is change. With progressive civilization, this thought must have forced itself into consciousness of uncountable human beings, without, however, enabling them to attain clearness regarding its meaning. Certain it is that the human mind cannot meditate calmly upon the outer and inner mechanisms of human life during the everlasting process of change. We must create for ourselves more periods of rest and repose, from which peaceful oases in the deserts of constant change we may observe our own standpoints, how we have gotten there, what influences will move us onward. With regard to the great Master Minds in Medicine, their origin and derivation, their work and influence and their fate, it would be very desirable if we could attain to such an island of restfulness and composure to view this most prodigious of all human panoramas. After having read the preceding chapters and combined the impressions there gained with lessons of general history, I believe that I can conclude, first, that there are only two clews to the maze of history. One is the principle of development, and the second is creative personality. A great many other keys to this labyrinth have been stated by different historians, but they can all be classed under these two that I have mentioned.

A further conclusion that I believe is permissible, is that history consists of changes and complex phenomena that do not repeat themselves regularly. It is a grave error to speak of history as repeating itself. The very essence of historic observation is the study of changes that are not repeated.

The third conclusion is that there are no invariable laws that govern historic evolution and this is based upon the next or fourth conclusion. All attempts to demonstrate a single unescapable aim, object or goal, of the human race, as the purpose of its historic evolution, have completely failed. To accomplish this we should first of all be able to define the great realm of human aims, objects, and goals. As a matter of fact, there is no systematic historic formula ascertainable in human aims, and this is quite explainable by the fact that to attain such uniformity of aim, and objects, we should have to assume that all human beings were psychically alike and equal. We have not been able in the past, nor are we to-day able to point out a definite aim and goal toward the realization of which all human endeavors are directed.

There have been eminent thinkers who attributed to eternal peace the direction or goal of human endeavor. Others speak of a Utopia to be found in the social democratic tendencies. It is to be emphasized that a systematic restriction of the realm of aims and objects would force upon the human race a narrowing down of the possibilities of its intellectual development, which in turn would be contrary to a belief in the independence and freedom of human nature. Assuming that there is in reality such a thing as freedom of the will, the most complete history of the past shows no definite complexes or human purposes that correspond for instance to our present world situation, 1925–1927, nor does it seem possible to me, as C. S. Leavenworth teaches in his "Lessons of History," that we of the present generation could dictate to succeeding generations how they are to arrange their life on earth. Mr. Leavenworth even attempts the establishment of a formula of history, which is as follows: "The substance (?) of a historic problem was to its result in a past age as the same underlying substance will be a similar solution in the present." In the second part of this arithmetic proportion the possibility of many variations and also allowance for the unexpected, is included. This side remark at first sight appears most innocent. As a matter of fact it destroys the entire formula because it is upon the unaccountable and incalculable differences in the psychology of human beings that the essence of history is based, and, as we have mentioned repeatedly, this great individual variation of human psychology conditions also that history does not repeat itself. The examples that Leavenworth gives on page 11 and 12 of his book, of the struggles between Scotland and England, that occurred in 1715 and 1745, are not repetitions but simply phases of one and the same process. Human evolutions may take much longer than even in this case. The entire Punic War of nearly 180 years is but one phase of contrasting interests between the Romans and the Carthagenians. One of the wars of France against England lasted 100 years and had various phases such as Leavenworth suggests. In other words, phases of the same historic evolution are here confused with repetitions of history.

A fifth conclusion is that the conscious continuities of different interests and purposeful activities of human beings form and make up the content of historic life, at least in general history. In scientific history this is not so noticeable, for instance, in the science of medicine, conscious continuities, while present, require great historic art for tracing them out. The maintenance and the extension of existing interests and the creation or abolition of new interests—these are the pivotal factors around which the activities of human beings revolve.

It is not always self-interest, but very frequently the interests of the race that form such a center of action. Such interests can become summated and then the different human purposes sometimes fall into an agreement. These in turn lead to long transactions for the realization of such agreements in the interests. The human beings that are participants in such realizations may separate into groups, some of them uniting for the purpose, others separating for the purpose. There are frictions, struggles, for the accomplishment of the purpose. There are contrasts and treaties, even self-limitations for the avoidance of these realizations, because the natural aids over which the human race disposes are limited and because the human race endeavors to continue its own intellectual interests. Before a historic transaction can be recognized, it is necessary that manifold and heightened spheres of interest are created. These must be definitely distinguished from each other.

This condition in history can be observed at every epoch when human beings are not only conscious of the interests that they have in common with each other, but also of the interests that separate them, and especially is it necessary that the race must become conscious that these uniting as well as the separating interests are permanent constituents of their existence. Hegel has said, *where there are no contrasts, there are no interests.* This sentence could be effectively reversed, where there is interest, there soon will be a contrast, but as soon as purposeful actions arise as a result of this relation of contrasts to interests, then the elements of historical evolution begin in what is popularly known as action and reaction. But it is not possible to analyze the psychic constitutents that enter into actions and reactions. How the great men interlock with this factor of history we can only explain by comparison with the effect and work of the great men of general history. In the article on "Darwin," I have called attention to the very effective analogy of the life of this great thinker to that of Napoleon Bonaparte. Hegel calls Napoleon *Der Weltgeist zu Pferde:* meaning that the deeds of this man represented a historic development which corresponded best to the totality of purpose of that epoch. When we reflect, however, that much of this work has been undone, we realize that if only a part of great transformations endure, that part must pragmatically be considered as the resultant of the conflicting interests of a historic phase. Darwin's life was spent in announcing great ideas concerning life. They have been modified and expanded, not restricted and minimized like those of the great conqueror. Therein is demonstrated in a most conclusive manner, that the life of thought is more enduring in its production than the results of applied force.

HISTORIC PROCESSES AND HISTORIC CONDITIONS: STATES OF EVOLUTION

AND OF STABILITY: The word "condition" in general history, as far as it is used to imply stability, is an incorrect designation because nothing is in a state of condition (or stability) in history, never has been in the past, nor will be in the future. Everything is in a state of flux, transition or of becoming; but for heuristic purposes the separation of events and conditions is unavoidable In all clearly recognizable historic records these two ideas reciprocally react upon one another, one giving rise to the opposite. It is also problematical to lay more importance upon one than upon the other, the events, processes, and transitions are of equal importance with the states and conditions. A "condition" is really not a standing still or an arrest, but it is in reality a process of such very slow development that it escapes our ordinary methods of observation. This is true in medical history as well as in general history. Lacombe called the processes "Evénements" (Fait Unique), and the stable condition he calls "Institutions" (Fait un dans sa Similarité avec d'autres). According to these historians, as well as according to Bernheim, human activities may be distinguished as occurring in three manners, first, the "singular," second, the "collective" and third, the "typical." The term I have used as synonymous with the word "singular" is "individual." What collective and individual historic action means has been defined in Chapter III on "Laws in Medical and General History."

It is necessary to define the sense in which the word "typical" is used in history. A historic event or activity is typical when it corresponds to the average knowledge of culture of the human race at the time that this event or action takes place. It must be apparent that both collective as well as individual activities can be spoken of as typical or not typical insofar as they do or do not correspond to the average state of culture at the times in which they transpire. There are, for instance, historic states where great masses act like one totality-individual, for instance during a war, or during a revolution. Such an action would be designated as typical, but it would not be typical if a mass representing, let us say, a nation of high culture, instead of taking a unified concerted action toward an external enemy, were to divide up into factions which fought each other (Northern Draft Riots during the Civil War; Threatened Desertion of Troops under George Washington). In the history of science we can apply this classification of collective, individual, and typical activities only with difficulty. In the history of medicine it is applicable only in tracing the origin and development of epidemics and diseases due to famine and deprivations due to abnormal meteorological, volcanic, and climatic changes.

This law, which the State has enforced in respect to the health of large groups of individuals, the Employers' Liability Act, Hygienic and Sanitary Control of Children in Public Schools, Pensions for those Advanced in Age, Pensioning of Old Teachers and Professors by Private Funds, and similar institutions, has in modern times brought about an interpenetration of the collective and individual activities and influences so that a sixth deduction in addition to the five I have already made, is this—that modern medicine as applied to civilization is bringing about through coöperation and State Aid an interpenetration of singular, individual, and collective activities. In all singular or individual activities there is traceable a strong element of the typical and collective character, which demands studying just as well as the powerful influence of the individual on mass action. In future it will have to be made an object of historic study to determine the activities of masses as well as of the individual in their interpenetration. But this applies so far only to such phases of medicine which the modern state can decide by legislative action. It is, therefore, of no influence on the progress of scientific medicine. The State may request an improvement in the control of epidemics, but it has no means of bringing it about except through the brain power of individuals. It may supply the laboratory equipment, good salaries, assistants, etc., but the main forces at work to make great advances in medicine are the various types of originality in human genius. (See chapter on "Ideas as Factors in Medical History"; also chapter on "Psychography of the Investigator.")

IS THE INFLUENCE OF THE MEDICAL HISTORY OF THE PAST TRACEABLE IN THAT OF THE PRESENT, AND WILL PRESENT MEDICAL EVOLUTIONS DETERMINE THE FUTURE OF MEDICAL HISTORY? The development of medical history goes back as far as the development of the human race, even beyond this. It may be traced to the prehistoric defensive efforts of animals. Prehistoric medicine, which, up to the present time, has been considered as a subject not approachable by the methods of modern historiography, has recently been found an interesting subject for investigation. (See Sudhoff, "Medizin in der Steinzeit," *Skizzen,* p. 53; also numerous articles on this subject abstracted in *Mitteilungen zur Geschichte der Medizin, etc.*) To give an example of what is being attained in the studies of prehistoric medicine, Jäger has studied the percentage of well healed prehistoric bone fractures and compared them with those that were healed badly. He makes out 53.8 percent of well healed bone fractures, and 46.2 percent of defectively united bone fractures. From these records of the dim and distant past of medicine, a steady progress of evolution of medical ideas and medical methods can be traced by the penetrating student of medical history; I mean a progress that is commensurate with the state of culture and civilization of the human race at each given historic period. This goes on steadily progressing up to the rise of the medical schools of Knidos, and Kos in the sixth century before Christ. These two schools were in close proximity and the *Asklepieion* of Kos has been reproduced on page 54 of "Geschichte der Medizin im Überblick mit Abbildungen," by Meyer-Steineg and Sudhoff. It was undoubtedly a kind of medical clinic for the education of physicians and surgeons, who, in those days, were designated as *Asklepiades* from the Greek word *Asklepios,* the God of Medicine. Those of the school of Knidos endeavored to found an actual science of medicine, an effort for which the time was not yet ripe, but those of the school of Kos limited instruction to general principles of treatment. They laid great emphasis upon those qualities of a physician which cannot be taught, but must be inborn. Medicine to the Koic school was an art, and not a science, an art which only the born physicians could carry out. The greatest representatives of this school were Hippocrates, Euryphon, and Ktesias (fifth century before Christ). It is quite certain, however, that these men were members of a long historic chain of medical teachers, the beginning of which is lost in the dim prehistoric period of the Greeks. The personal share that Hippocrates had in the works that are attributed to him is still not definitely ascertained, but this much is evident —all of the Hippocratic writings exhibit a uniform spirit. I wish to mention especially one of the writings which is entitled "concerning the *old* medicine," because this very title is indicative of the fact that even at the time of Hippocrates there must have been a traditional *"old"* medicine. The father of Hippocrates, Herakleides, was himself a physician and had been trained in the school of Kos. His brilliant son, Hippocrates, was born in the year 460 B. C. Thus far, I simply wish to indicate in extremely brief words that medicine was *old* at the time of Hippocrates, but that there was a steady progress which lasted until the second century before Christ. The struggle between science and art of those days has its beginning in the antithesis of the school of Knidos and Kos. It continued until the time of Galen (Galenos) a medical personality of colossal grandeur, who had a will that could force the currents of innumerable sources of antique medicine into one common bed, and a comprehensiveness that made the most heterogeneous ideas serviceable. All the knowledge and experience of the physicians of antiquity were unified in this great personality. But when he died in the year 201 B. C. the medicine of the European nations around the Mediterranean Sea entered upon a long period of decline. Greek medicine was reborn in the Empire of Islam and the rebirth lasted from 350 to 400 years.

In the fourth century we have the aftermath as well as the fading away of classical medicine in the Western Roman Empire.

From that time until the ninth century the records of medical history leave us in doubt as to whether medical science made progress or not. In the ninth century a medical school was in operation at Salerno, on the Gulf of Naples, and here a great medical development took place, but the records of that development are not always dependable. The geographic relations at Salerno were comparable to those of Knidos and Kos, climatologically. Those who are interested in the periods of development of medicine are referred to Sudhoff's article "Perioden im Entwicklungsgang Heilwissenschaftlicher Erkenntnis," page 31 of "Skizzen." Also in his essay on "Salerno," which has been translated by me from *Prometheus* (see "Essays in the History of Medicine," by Karl Sudhoff, with a foreword and biographical sketch by Fielding H. Garrison, published by Medical Life Press).

IS THE HISTORIC PROCESS AND CONDITION OF ANY GIVEN CHRONOLOGIC PERIOD A RESULTANT OF THE CONVERGENCE AND FORTUITOUS RECURRENCE OF EXTERNAL CIRCUMSTANCES (The teleologic principle of preceding epochs)? There have been numerous historiographers who admitted the doctrine of final causes or teleology as the ultimate factor of all historic processes, but without succeeding in making historic causality plainer or giving it a meaning that could help in applying the experiences of the past in shaping the events of the future.

In the history of medicine the assumption of a teleology is only observable by a few natural philosophers of the type of Hans Driesch, not really medical thinkers, but philosophic biologists. The doctrine may or may not be true, but in medical historiography there is no practical necessity for accepting or denying it because it does not help the elucidation of the problems of the present and future by the exposition of the events of the past or reversely.

Too often eventuation of the world is spoken of as synonymous with eventuation of history—the world process as identical with the historic process. It is necessary to repeat that historic processes have to do with the activities of human beings exclusively—but the world process was going on for æons before there were human beings and will continue for æons after the last human being has ceased to exist. The greatest thinkers of the human race have occupied themselves by profound meditations on both of these processes and when the limits of the methods of science and reason were reached they built lofty thought structures, sometimes soaring above comprehension in their sublime daring. Nor must these thought constructions be despised by the more objective and conservative thinkers. Experience has proven that if they cannot be understood as realities for the time being they have nevertheless occasionally led to realities. See for example Chapter VI on the criteria of what constitutes a great discovery, especially the prophecies contained in the astrophysical speculations of Gauss, Leverrier, and John Couch Adams, and those of Mendelejeff in his periodicity of chemic elements. These and other realities were in their incipiency nothing but thought structures and previous to that they came under that undefinable human sense called inspiration.

Thought structures may or may not be verifiable by the methods of science. They may not be verifiable at the time of their conception but become so later on, because the methods of science move slower than the processes of thought. Wherever the thought structure becomes amenable to scientific method it is subject to the conclusions of reason and must stand or fall accordingly. The phlogiston theory for example was a thought structure of Stahl that even captivated Lavoisier, but had to fall by the very researches of the great French chemist. But the theory of the mechanical equivalent of heat by Julius Robert Mayer was confirmed by Helmholtz (though reluctantly) and still later by many others. These verifications of thought constructions that seemed non-approachable by scientific methods should caution us against hasty condemnation of any effort of the human mind to penetrate into the unknown, provided the effort is honest and does not seriously conflict with any established facts and truths of science. As thought construction, either conscious or subconscious, precedes scientific method, similarly the world process and even the historic process precedes historiography. Events must occur and language as well as writing must be invented or acquired before events and processes can be expressed, described, and recorded. Goethe said that the part of time that a seed spent unseen under the ground was nevertheless a part of its life history—hence prehistoric events and conditions must be a part of history. The seed is, however, both substance and evidence in testimony of its own existence. A long, long chain of events extends regressively from the present time to the beginning of the human race and the present human mind is so constituted as to presume and seek an interaction of these successive events, and to endeavor to arrange them in order of cause or effect. The subsummation of this entire world process and history process is impossible for the following reasons:

I. All events, evolutions, and conditions that preceded the appearance of man are not ascertainable, except to an infinitesimally small extent, by the methods of science or paleohistory.

II. The epochs antedating the origin of human language are lost for historiography.

III. After human beings made use of language, events and experiences were transmitted by tradition. Oral transmission had to pass through physiologic apparatus which changed the reports that were received because it had to undergo continued changes itself, being a biologic, therefore an adaptable instrument. Therefore, all tradition prior to the invention of written language is unreliable and lost for history.

IV. The methods of writing of ancient races made the recording perishable, even that carved on stone. So that written records, monuments have been lost or destroyed and only a small part of the written records of antiquity is available for history. For example we no longer possess the Hebrew text of entire books, and parts of books of the Old Testament, but only translations of the lost Hebrew into Greek texts and by translators who did not possess a philologic ability commensurate with the great importance of this translation (Nivard Schlögl Bd. I S. 12).

V. After subtraction of the previous four sources there remains a small remnant of sources and origins of historic knowledge which have to stand the test of historic critique and discernment, because of distortions willfully provoked by human selfishness, passions, and ignorance. The documents that have been subjected to the most intensive study and research are the Scriptures of the Old and New Testaments. Dependable records of compilation and the redating of these most venerable historic records, which the human race possesses, go back beyond the year 444 before Christ (see Kautsch, *Die Schriften des Alten Testaments,* edited by Bertholet and collaborators; also Nivard Schlögl, O. Cist, *Die Heiligen Schriften des Alten Bundes Wien,* 1922). The evidence is conclusive that the translators of the original, now lost, Holy Scriptures, were not inspired men and that by incompetence or prejudice the sacred order and arrangement of the various parts has been disturbed. ("The Sacred Books of the Old and New Testament." A new English translation by eminent Biblical Scholars. Edited by Paul Haupt.)

A distrust rests even upon such sacred history. It does not leave us by perusing any of the records that have come down to us. All of them must be submitted to the penetrating scrutiny of the trained historiographer, and after the wheat has been separated from the chaff, that which is left—really only a small part of that which might have been our historic heritage becomes the stock of guiding truths for the present and future historic process, so far as the limitations of the human mind permit of understanding this process.

It is regrettable that even among historiographers there are writers with an irresistible propensity to indulge in speculations, unfounded hypotheses, and fantastic ideas, that are not constructive thought structures because they are devoid of objective proof and actually impede the development of a genuine science of historiography. Most unfounded for example is the assertion that "all that is in present history is dug out of the deposits of the past immediate or remote." That would mean that all activities of human beings of to-day have their prototype in the same activities which other members of the race have carried on before: it is the old error of history repeating itself. Where is the evidence for such an opinion? In the face of the colossal and irreplaceable losses and distortion of records, how can such an assertion be proven? Contradictory as it is to the continual evolution of man and of human society, would and could man repeat the activities of the race that transpired in the remote past even if they were known? What becomes of the conception of freedom of the will, if the race is predestined, i. e., coerced to pass through the identical historic phases to the end of time? In the History of Medicine such a hypothesis rests on no dependable foundation. There has been a steady progress without repetition of past events. Here we must, of course, distinguish between the thought, "built upon," "developed out of," and "derived from," suggested by," etc., the past from "repetition" of events or conditions of the past. Some of these developments consisted in shaking off and freeing the upward struggling mind from the incumbrance of the errors of the past; that means, it would have been better for progress if such a past had never occurred. (See Chapters XI, XX and XXI.) Of what advantage can "digging out of the deposits of the past" be to present history when the evidence proves that the past is replete with error and confusion? It appeals to reason that as our contemporary generation had countless preceding generations who labored and strove to improve the intellect, to elevate the status and economic conditions of each stage of historic existence, by inheritance and natural selection, much that is in the mind of man to-day is the result of evolution of the state of mind of past ages. But this furnishes no proof for the hypothesis of the repetition of history. Elsewhere I have referred to the method of regressive tracing of the historic process as sometimes successful in filling out gaps in the records and sources.

The modern human being rarely brings himself to realize that we can find truths by thinking—truths that are not abstracted simply from experience, but which are erected on sound logical conclusions; more admirable even than this thought research is the fact that the relation of things of which we had no intimation corresponds to the truths that are reached in this manner. This coincidence of truth arrived at by pure thought operations with the actual relation of things we did not even surmise was termed by Lotze, himself a physician and great philosopher, as "abyss of wonderfulness," the discovery of which was the great philosophic achievement of Plato. E. v. Hartmann (Kritische Grundlegung) expands this wonder into a postulation, namely: the fundamental form of the being, essence of things must correspond to the fundamental forms of perception and thinking. Of course he refers only to trained logical thinking directed specifically to such problems. There is a harmony between thinking and being—there is a kind of thought speculation in the sense of Hegel that appears creative, both in the world-process and in the history-process. There is an alarming disparity in following out these two processes. Schiller compares the world process to an uninterruptedly flowing stream, of which only one wave now and then breaks upon the banks of the history process and he calls attention to the observation that a remote historic event is more readily brought into relation with the conditions of modern or current years than with the events and conditions that preceded or were contemporary with that event.

In other words an historic event appears isolated if we endeavor to trace it regressively into its own times. Such a historic event Schiller and Ranke call Christianity and the Christian doctrines of morality that emanated from it. They admit that the Christian religion has such a manifold share in the form of the present world process that its appearance becomes the most important factor of the historic process, but neither in the time in which Christianity appeared, nor among the people from which it arose is there a satisfactory explanation for its appearance. This is explained by the deficiency of the sources. Thus a history of the world (the historic process) would never be anything but an aggregate of fragments and not deserve the name of a science. It is by the help of the philosophy of reason that this disjointed aggregate is elevated to a system and molded into a connected form conformable to reason. In order to accomplish this, philosophy must link these fragments by artificial connections. When we ask what makes such a system credible, Schiller answers us in the same manner as George Sarton (Sarton's "Knowledge and Charity," *Isis,* Vol. 5, 1, Oct. 1922, p. 9): "The greatest truth yet unveiled by man is the possibility of the existence of truth: in other words—the consistency and unity of nature." Schiller calls it in his *Was heisst und zu welchen Ende studiert man Universalgeschichte* (1789), "Die Gleichförmigkeit und Unveränderliche Einheit der Naturgesetze und des Menschlichen Gemüths" (Cotta Tübingen Edition, BI. IX S. 374), and then Schiller proceeds to a dogmatic statement which has been portentous in its erroneous applications. I will translate this statement verbally: *The events of the most remote antiquity under the influence of similar external circumstances return in the most modern time-currents, therefore the newest phenomena within the circle of our observations can be re-*

traced backwards until they become lost in prehistoric times, and conclusions can be drawn and light disseminated in this manner regressively. The method to reach conclusions by analogy is an important technical aid as everywhere so also in history, but it must be used for an important purpose, and with as much carefulness as discernment.

This statement which is further on carefully qualified by Schiller and which from the context he did not intend to formulate as a dogma, but merely announced as a hypothesis, has been taken by numerous medical historians as one of the dogmas of historiography. As an example of this I need only to refer to the excellent work by Ernst Anton Quitzmann in his *Geschichte der Medizin* (p. 19), where he even interprets Schiller's words to express a kind of predestination of medical history. He says: "The slightest events or what are seemingly the most unimportant facts of the present time are conditioned only by the coincidence of the events of all the centuries that have transpired." This is not exactly what Schiller says, but it constitutes a favorite dogma with a few modern medical historians. I have already given my objections to the dogma in the preceding, but to illustrate how even impressive writers aim to substantiate their theories by sophism I will add what Quitzmann states on the same page: "Only in the total conception of the entire past can the key for the correct understanding of the present be found." Now this statement in itself is correct, but it does not constitute a proof of his construction of Schiller's Historic Theory which immediately precedes it. For example, it may be true that the past offers the key for the correct understanding of present events, but, notwithstanding, the theory that *all* the events and historic processes of the present time are conditioned by events of *all* past centuries may be false. The reasons for proving the second part of this assertion an error, have already been given above.

I cite this matter of Schiller's pseudo-historic dogma and Quitzmann's misconstruction of it, to demonstrate the great difficulty a historiographer may have, not only in finding his way out of the labyrinth of errors in the sources, but also to point out the snares in the assertions of eminent writers of history. Schiller has written a great deal of history. Some of it concerns the Prehistoric Condition of Man; another essay is on the Legislation of Lykurgus and of Solon; A Historic Essay on Moses; A History of Migrations and Crusades in the Middle Ages; A Survey of the Conditions of Europe at the Time of the First Crusade; A History of the Disturbances in France from the Reign of Henry IV to the Death of Charles IX; Historic References to the Duke of Alba; A History of his March to the Netherlands; The Siege of Antwerp, and other Invasions into the Netherlands under Charles V and under Philip II; History of the

Thirty Years' War in Germany, and other, minor historic contributions.

I cite all this to demonstrate that the eminent German poet had a broad experience in historiography and indeed some of these historic researches have become classics. Now if he had intended in his very first address on "History" in 1789 to announce the dogma that all the facts and events of the present were conditioned by the coördination of events of all past centuries, we would have a right to expect the technical demonstration and proof of this theory in some of his various and extensive historic writings, but the reader of these historic investigations will seek in vain for any evidences proving the statement which Schiller made, and which has been raised to a dogma by Quitzmann. If it were true we would indeed have a law of history, the law that history repeats itself. I have spoken of this error in connection with the work of Leavenworth and in Chapter III.

In his introduction to the History of Philosophy of Science (*Isis,* May 1921), George Sarton states that among the scientists and scholars who have become interested in historic studies, very few seem to have appreciated their special difficulty which arises from their involving, at every step, a sound knowledge of two distinct sets of facts; namely: scientific facts and historical facts, and a sufficient familiarity with the methods required to ascertain and interpret these two kinds of facts. The method of the History of Science is a combination of the scientific and historical methods. Scientists are apt to underestimate the difficulties of historic technique; historians misunderstand the scientific data.

THE TENDENCY TO UNIFICATION IN THE HISTORIC PROCESS IS APT TO LEAD TO ABUSE OF ABSTRACTION: Even the best trained historiographers cannot resist the temptation to unify seemingly discordant events, data, etc. It is a human tendency. Man irresistibly attempts to assimilate everything in his environment and make it accord with his own point of view. All phenomena of the historic process he endeavors to formulate into a thought. Now it is not always possible to coerce past historic transactions into clear thoughts. It is, for instance, not known at the present day what were the thoughts of the dignified senators of Rome, men descended from eminent families and who had given evidence of being actuated by the highest motives—what were their thoughts that prompted them to conspire against Julius Cæsar. It is futile to compress all activities of human beings into concrete thoughts. Part of the art of the historian consists in knowing when to desist from expressing clear thoughts about past events. The best way to do this is to ask ourselves what is the undoubted evidence that backs up this or that event. But too often the effort is made to tie up the past with the present, and, if it is only half-way successful, the historian becomes inclined to interpret events, and happenings that preceded them as cause and effect, and what is more illusory than this, he reads into this change the idea of intention and purpose. Schiller has already called attention to this. How, for example, one event or phenomenon after the other fits itself into a unified totality, seemingly emerging from that which appeared blindly fortuitous and without law. Now this unified totality is a fabrication of the mind of the historiographer in many cases and very soon it becomes very difficult for him to extricate himself from this seeming consecutiveness of phenomena which, in his conception, has assumed so much regularity and purpose; once yielding to the temptation of the subtle regularity and artificial harmonizing of historic effects, it is exceedingly difficult to return to the control of necessity which the facts of history alone give without the artificial mental construction. There are historians who produce this harmony of history from out of themselves and transplant it into the order of things outside of themselves. They thus aim to bring purpose and reason into the world process; a teleological principle is introduced into the world history. The philosophic mind tests this thought construction by every phenomenon in the grand panorama of history that is offered. He sees it confirmed by the thousand agreeing facts; also he sees it disproved by an equal number, but Schiller states that as long as important connecting links in the series of world changes are missing, and as long as fate withholds the disclosure of the cause of many important events, just so long must the mind declare historic problems as undecided. Eventually that opinion will govern which gives the greatest satisfaction to reason and the greatest happiness to the heart. He adds that such a history might lead the historiographer to do violence to the truth of events, and move the fortunate epoch for exact world history writing further and further away, whilst his intention was to bring it on sooner. Evidentally such a thought by Schiller is no verification of the pseudo-dogma that the events of present or our contemporaneous times of history are conditioned by the coincidence of circumstances that happened in remote centuries. Applied to the history of medicine this means that when the great panorama of past epochs in medicine are spread before the eyes of the modern historiographer, it will prevent the hasty decisions of the moment. Another of the impressions that I have gained in the study of history of medicine is that the facts, data, events, are more easily ascertainable than the facts of general history in those periods of which we have any records at all.

In the study on Leonardo da Vinci (Chapter XXVI), I have quoted an expression of his concerning a process which he terms "Necessity"; his expression is, "Oh, admirable necessity!" which is sufficient to indicate that he sees in the extreme need of any particular historic group of beings the very incentive to action which will eventually find a means to supply that need, as we say "Necessity is the mother of invention." As simple as our explanation of this historic process is, it is not easily explained as a factor in history, but it does play an important rôle even in the history of medicine. It must be evident that the prevention of hemorrhage, and the means to produce unconsciousness in surgical operation, were the necessities that led to the discovery of ligation of arteries, and to the methods of anæsthesia. Necessity compels the human mind to seek a solution of the difficulties of the human race by applying the means of observation and experiment to the utmost. All of the greatest discoveries of medicine have had this force as an incentive. It is, therefore, not at all wise to endow the investigator so richly that every trace of this fruitful force shall be eliminated. Genius is not born of affluence. The history of medicine, and certainly that of music, shows that it requires the incentive of dire need to produce the great achievements. Some day the historian will arise who will take an inventory of all that has been accomplished in richly endowed institutions and arrange a category of these achievements side by side with those that have been accomplished by men without the aid of endowed institutions. I am not prepared to predict what such a comparative study will elaborate, but some of the accomplishments of investigators that were compelled to work without the aid of organizations or endowment will give evidence that the human mind requires a force more powerful than the influence of or the protection of great institutions. I am referring to the exceptional stimulus that comes from inspiration. Professor Poulton of Oxford believes that the kind of life which so much favored Darwin's mind has disappeared in English universities, and H. F. Osborn asserts that this scholarly, yet stimulating, atmosphere is still more truly that of old Cambridge, England, than of any American university. In "Impressions of Great Naturalists" (p. 43), Osborn remarks: "It would be an interesting subject to debate whether we could nurture such a man; whether a Darwin, were he entered at Columbia, Harvard, or Princeton, could develop mentally, as Charles Darwin did at Cambridge in 1828." Osborn believes that conditions for the favorable nurture of such a mind are not with us. These conditions are repose, time for continuous thought, respect for the man of brains, and of individuality, and of such peculiar tastes as Darwin displayed; freedom from mental conventions, general sympathy for nature, ardor in the world of ideas. If the genial mind cannot find the kindred mind it cannot develop. Many American school or college men are laughed out of the finest promptings of their natures. In short Osborn believes, and I believe personally, that our intellectual environment would be distinctly against the young Darwin of to-day. Ostwald in his *Grosse Männer* expresses somewhat similar opinions, namely: "That the classical idea of education is hostile to culture," and describes the effect of bad teachers at universities, and in the *Die Medizin Der Gegenwart in Selbstdarstellungen* (Vol. 2), Hueppe and Meyer describe their disheartenment at the system of education even in German universities. After all there is much truth in the saying attributed to President Garfield: "A University is Mark Hopkins on one end of a log, and a student on the other": that means, that the inspiration of a great teacher is 99 percent of the success of universities. Such teachers that I have known in America were Simon Newcomb, W. K. Brooks, Basil Gildersleeve, Ira Remsen.

CONCEPTIONS OF WHAT CONSTITUTES A GREAT MAN: Any one who desires to learn the widely different, even contradictory ideas, of what constitutes a great man is advised to read the *New York Times* for Sunday, July 23rd, 1922, Section VII, headed "Twelve Greatest American Men." The objective historian and generally scientific man will view the twenty-four lists of great men there presented with an amused tolerance. The Democrats enumerate William Jennings Bryan and Woodrow Wilson; the Republicans list Warren G. Harding and Herbert Hoover; the admirers of wealth list John D. Rockefeller and Charles Schwab; in one list Gompers, Schwab, William E. Borah, and Babe Ruth are enumerated, It is encouraging to find investigators mentioned, like William H. Welch, Simon Flexner, William J. Mayo, Alexis Carrel. Edison's name occurs in eighteen out of the twenty-four lists. Out of the really selected list of twelve names there is not one scientific man, nor a single medical man; the only educator is Dr. Charles W. Eliot, President emeritus of Harvard University. The list that received the highest number of votes begins with Thomas A. Edison and ends with Booth Tarkington.

Evidently there was no definite principle of guidance in selecting such a list. An international standard was not considered in the selection of the twelve living greatest Americans; nor does the condition which limits the choice to men living at the present time make it possible to select the really great Americans. For example, Benjamin Franklin, Joseph Leidy, and all of the seventeen leading American men of science, described in a book of that title by David Starr Jordan, could not be enlisted. The exclusion of historic American scientific characters and the lack of appreciation of the value of pure science has brought it about that scientific and artistic men are notable by their absence. I find only one musician, Walter Damrosch. Here again the limitation to living composers excluded McDowell and Horatio W. Parker, and others. That William H. Welch, William J. Mayo, Simon Flexner, and Alexis Carrel were mentioned, indicated an appreciation of the great public service of these medical men. The entire list serves to demonstrate that the general public is incompetent to determine by this manner the twelve greatest Americans in the entire history of our country, because this would necessitate a knowledge of history.

CRITERION OF HUMAN GREATNESS

As I have stated elsewhere the most important criterion of human greatness is that of unselfishness. It is the man who seeks not his own personal interest, but only the objective purpose of usefulness, of serving humanity. The great man is he, without whom the world would appear incomplete because certain necessary great achievements of his time and environment were made possible only through him and would be unthinkable without him. Emerson stated his views of this question in the following words: "What is so mean and so inclusive and comprehensive of all commonness as the rapacious greed for reward; this makes the difference between a laborer and an artist, between simple talent and genius, and between sinner and saint. The man whose eyes are not glued to the quality of his work, but only to its reward, is common, and he is equally common whether this reward consists of money, station, or glory. He only is great, whose view is broad enough to see that no man can escape the reward of his deeds because he transforms himself into his work and assumes the nature of that work, because deeds and work produce their own fruit, like every tree."

In judging the master minds in medicine, self forgetfulness and forgetting the applause of the world constitute the harmony in the music of scientific endeavor. Only such men become master minds in medicine who can produce great things with entire freedom and in the absence of external coercion. Men who in their work need not consider the favor or the disfavor of single individuals, but only the interests of the great generality of the race and the fulfillment of natural duty. Most of the educators of the present time believe that this freedom, and exemption from external coercion and from the favor of those that are in power, can only be brought about by placing the scientific genius in institutions of endowment and the cry is forever, more and more endowment. On reflection it must become evident that the very poor man, if he has great genius, can accomplish the results of enduring excellence even better when he is entirely free of the favoritism of endowed institutions. Paradoxical as this may seem at first sight it is confirmed by almost every serious investigator of human genius: by Ostwald in his *Grosse Männer;* by Alphonse de Candolle's *History of the Sciences and of Scientific Men Since Two*

Hundred Years; by Sudhoff; by Charles J. Whitby in his *Makers of Man;* by David Starr Jordan in his *Leading American Men of Science;* by Lord Brougham and others. Personally I feel that the unfolding of a self determining spontaneity and the finding of a true way for a taste of profound issues will take place with greater assuredness of direction among the few great men that have had to live a life of poverty than among those who became hothouse plants in endowed institutions.

Scientific institutions of the present time are engaged in a colossal experiment and are seemingly all unconscious that it is an experiment, namely this: to what extent can money help the solution of the great problems that confront the human race, and what are the collateral circumstances by which money can bring about progress in the solution of these problems, and when does money become a menace? Dr. Ernest Barker of Kings College, London, in a recent address at Toronto, Canada, said: "There is a limit to the extent to which private benefaction can safely endow universities. They belong to the general commonwealth; they must be above suspicion of belonging to one or the other side of social cleavage. They belong to both. A university which relies to any great extent on private benefaction may tend, however unconsciously, to teach and preach acceptable things, and that is the greatest offence which it can permit against the spirit of truth."

I have not the time in this chapter to enter upon the manner in which money can do harm to academic freedom, and to the evolution of human genius, but traces of it can be seen at the present day in the influence which the private opinion of rich donors exerts on the teaching of evolution in its relation to religion. Also the similar effect of the influence of private opinion concerning disciplining the general public in the use and abuse of alcohol. There are other subjects of a similar nature where the private opinions of wealthy donors have influenced the direction which university training took or declined to take. The frantic and undignified tendency towards the accumulation of wealth on the part of universities, an accumulation which is out of all proportion to any real increase in scientific achievement, especially that form of achievement that could advance human culture will, let us hope, soon reach its limit.

At the Chandler Chemical Museum at Columbia University in New York, three pieces of chemical glassware are on exhibit; two small flasks and a "U" tube. They were sent to the United States by the Dean of the School of Chemistry, at the University of Lille, and seventy years ago were used by Pasteur at Lille in his studies of fermentation, which led him to the discovery of micro-organisms and a knowledge of their significance in fermentation and in disease. (See Chapter XXII, this volume and "The Life of Pasteur," by René Vallery Radot, translated from the French.) The manufacture of glass is mentioned by Dioskorides (Pedanios) in the year 64 A. D. and Plinius, in his "Historia Naturalis" (Lib. XXXVII), states that the Roman Emperor Nero made use of a polished stone to view the gladiatorial contests. According to other historical accounts Nero was short sighted and this polished stone appears to have been a kind of primitive eye glass, the first historical example of this kind. This was in the year 66 A. D., so glass has been known a long time; some even assert that the art of glass making was known to the Egyptians before the time of Moses. During many centuries, therefore, glass blowers could have prepared the apparatus that Pasteur needed in his epochal work. It was not the lack of apparatus that kept mankind in ignorance of bacteria—it was the lack of men like Pasteur and Koch.

Great advances in science will be made just as soon as the special human intellect appears and is ready to make them, and the chances are that many of them will not require complicated and expensive apparatus, nor richly endowed laboratories. A master mind should be sought out by other master minds who are competent to judge already during the formative period of childhood what the developing intellect may be capable of accomplishing. This does not require new and rich institutions; these institutions are already here; they are the public schools, but in them are needed master teachers with penetrating critical discernment to select the young individuals that give promise to be leaders. The first discovery of bacteria by Anton Leeuwenhoek in 1683 did not require expensive apparatus. His microscope was of the utmost simplicity, but as he was not a deep thinker he could do little more than simply observe, being incompetent to interpret the rôle and meaning of bacteria in the living world. The discovery of anæsthesia (see Chapter VI), the discovery of the nature and the transmission of the infectious agent of yellow fever by an infected mosquito (see Chapter XV), did not require very elaborate apparatus. It is astonishing to be told in Berlin with what simple apparatus Virchow and Koch worked, or in Frankfurt in the "Institut für Experimentelle Therapie" with what simple apparatus Ehrlich accomplished his work. The pursuance of the original great idea is independent of material equipment. What science needs is proper minds to conceive the great ideas. The schools must develop departments for the equipment of pedagogic psychologists to be employed among the children of all public schools, not only for the detection of those that give evidence of becoming eminent scholars, but also for the direction of those that are not eminent. It is equally important that there should be many good followers as it is to have a few good leaders. The modern American prating about leadership to every school boy is waste of breath. He must be taught that he can be as useful a citizen by being a good follower as by being a prominent leader. One depends upon the other. There is no use for leadership if there are no followers, and it is harmful to discourage the average pupil by impressing him too much with the importance of leadership when nature had equipped the great majority of the human race to be followers, and only a few to be leaders. *Multi vocati pauci electi.*

CONTRAST AND REACTION: In Chapters I and II, and also in Chapter IV, the effect of contrast has been emphasized as a condition causative to interest. As soon as purposeful actions develop from evidences of contrast we observe a series of so-called actions and reactions. Historians look upon these as elements of historic development. In the beginning of this chapter I have held apart the conceptions of the world process and the historic process. Now action and reaction, the effects which they actually accomplish, as well as those which they aim to accomplish and do not achieve, and finally those which they fear and wish to avoid, all these elements form new centers of interest, and ever new original material for the historic processes. For heuristic purposes the methods of studying general history, and those of medical history have been held apart. In reality medical history is a part of general history, and the methods of study of the latter must, in the long run, coincide with those of the former. We have taken out of the vast panorama of the human past a series of object lessons which may have erroneously been mistaken for biographies. As a matter of fact, these great personalities are simply keys to certain phases of the medical historic process, certain types which unify in their life history the evolution of some great idea. (Chapters VIII and XVIII, and Chapter XXIII are not included in this meaning.) These types represent the fulfillment of human demands upon life and the gratification of human needs, but it may not be so evident that the achievements and activities of these master minds cannot exert telling effects as isolated factors of human endeavor, but only in a reciprocal relation with other human interests. In short the achievement of the master mind, the great individual, stands in historic correlation with the masses, and this relation evokes feelings of contrast. The human master mind is an object lesson to be contemplated as an exponent of a certain phase of work that must be accomplished, a certain need that must be gratified, and with the complexity of human nature this is only possible through the causation of interest, and where there is no contrast there is no interest. All of the intellectual sciences are either consciously or unconsciously rooted in this principle, the principle of certain causal connections which are founded in the chain of interests that most immediately concern the needs of the race, so that we can really say that necessity, in the sense of Leonardo da Vinci, and contrast, as well as interest, in their reciprocal relations, come very close to being historic factors.

THE PROGRESS OF MEDICAL SCIENCE AS INDICATIVE OF THE PROGRESS OF THE HUMAN RACE, VIEWED IN THE PERSPECTIVE OF THE PHILOSOPHY OF HUMAN HISTORY: When we speak of the progress of the human race we have to make clear whether we are dealing with the entire human race as a totality, or with only certain particular people that constitute the race. The next essential for clearness is the understanding of the fact that history can never have the uniformity of a philosophical system, but this would not prevent history from possessing an inner connection, and that this connection can, in many instances, be demonstrated by a scientific historic methodology. A third necessary insight is that medical history, especially historiography, cannot be understood apart from general history.

Having made clear these three essentials let us proceed to investigate whether the underlying principles of general history are applicable to those of medical history. First of all, it is necessary to avoid certain long continued errors of historiography which have been taken over from the general historians by medical historians, and which have existed and been practiced for so long a time that, because of a peculiar respectability which they have acquired, they are very hard to eradicate. Medical history must not be conceived as if it were merely a record, the endeavor of the race to preserve itself by the prevention and cure of diseases and suffering. More far-reaching and greater than this is medical history. It is the exponent of culture and civilization for the entire human race. This is the most noble and sublime function of medicine. It may fail in healing a great many diseases. Old and well known diseases may be eradicated or disappear (the plagues of the Old Testament), although the period contemporaneous with them may know nothing about them. New and unknown diseases may develop and, in spite of the disappearance of the old and the coming in of the new, both of them unknown, this great power for the preservation and betterment and culture of the human race continues on and on, just like a religion. This history of medicine is a holy thing; it is to the devotee so sublime, attractive, and pleasant that it is almost irresistible to dwell on the past millenniums of its existence.

Is there a philosophy of medical history? Undoubtedly there is, if we can approach it with a spirit of altruism and inspired by a belief in the holiness of the moral law of man. But there has been an unfortunate controversy between philosophy and history. Historical philosophers from *a priori* reasons have aimed at conclusions concerning things that they thought should be in history without observing that their work was weakened by such doubtful features. They went to work to seek a realization of their thoughts in history. I am not speaking now of purely speculative philosophy, or even the more enlightened philosophy of the followers of Socrates, but this speculative philosophy is evident in the writings of Galen, and even traceable in chemistry up to the time of Stahl. When a historian proceeds with a pre-conceived opinion it is almost unavoidable that certain facts should be selected out of his material which appear to confirm his opinion. I speak of this in the article on the "Psychography of the Investigator," where Goethe calls such thinkers *DIE UMFASSENDEN*. Two of the most difficult pre-conceptions which have had their effect on the history of medicine, as well as on general history, are the following: first, that the entire human race is involved in an uninterrupted progress, a steady development to perfection; and secondly, the idea of the freedom of the will. It may be questioned whether these two conceptions properly belong in a work on the history of medicine, but certainly medicine is part of the progress of humanity and the study of the human will belongs to psychology and psychiatry, which are parts of medical science.

There are two ways of studying human things and human affairs: one is to gain knowledge concerning single things or individual affairs, conditions, and transactions, the other is by way of abstraction. The latter is the way of philosophy, and the former is that of history. One specially inclined to religion would suggest there is a third way: namely, that of revelation. I would answer that this includes both of these two processes that I have just mentioned. What is known by revelation includes abstraction as well as history. There are very many historians, some of whom have attained considerable fame, who look upon history merely as an enormous aggregate of facts which one must memorize. Then it happens that single things linked into other singularities are held together—if by anything at all—simply by an infirm general system of morals. This has the regrettable tendency to place the human creature both into the background as well as in the center of all historic activity, as the prime and original mover and instigator of events. I have never been able to find just such a historian who would make the suggestion that the historic process may be something over and above human will and human power which process uses human beings as figures on the chess board of life. Naturally it is more conventional and more agreeable to man's incorrigible egocentric viewpoint to conceive himself as the prime mover of history, and immune to all influences outside or above him. Those who hold that there is nothing in the universe but what can be understood and eventually controlled by man, constitute the monistic school of modern thinkers. The subject is discussed at length in Merz's "History of European Thought in the Nineteenth Century" (Vol. 4, page 60). The brilliant medical philosopher, Lotze, declared it the task of philosophy to reconcile the results of experience and science with the demands of our emotional nature, or to show how the world of values or worths finds its realization in the world of things.

History deals both with the world of real things and the world of values in their relation to man's activities, but the most important thing that we must hold before our eyes as historians is, that man's activities are only to a very limited degree the result of his own personal will. If Lotze places the idea of personality in the center of all historic processes, he leans decidedly toward a reconciliation of the contradictions which human life and human experience present to us in history on a religious basis, and indeed this is also the foundation upon which Leopold v. Ranke founds his great "System of World History."

To assume that there is a moving power to history above and outside of the human being, does not necessarily imply that the search after the meaning of history is beyond human methods of investigation.

CAPACITY OF HISTORIC METHODOLOGY TO ELUCIDATE THE INNER OBJECTIVE CONNECTIONS OF HISTORIC PROCESSES: No matter what standpoint we occupy concerning the nature of historic methodology, an earnest and penetrating search by means of historic science will reveal that it is able, yes even destined, to rise from an investigation of single things, events, characters, etc., to a general aspect of the historic transactions, and eventually to a knowledge of the objective inner connections of all historic events. There are two character traits necessary to make a true lover of history.

The first one constitutes sympathy and joy in single things and events, of and by themselves. One must not shrink from a sympathetic contemplation of even the repellent and horrible things that man has done, but mindful of the saying of Terence, quoted in Chapter XXV: *Homo sum, nihil humani mihi alicnum sit,* and also of the quotation from Kant in the same chapter, *That whilst man himself may be unholy enough, the humanity in him must be holy to him,* if one has a real sympathy with the genius of those many-shaped creatures of which we are individual parts; of this being, that is always something old and then again something new; that is so good and yet so bad; that is spiritually so noble and yet so brutal; so cultured and yet so coarse; so much trained toward Eternity and yet absolutely subjected to the moment; that is so happy and yet so unhappy; that is so satisfied with little and full of covetousness for everything. If one inclines with a warm sane heart to this living phenomenon of man for better or worse one cannot fail to rejoice at the fact that in the historic process as revealed by him he has never tired to live in spite of the most portentous opposing forces and one will overlook the defects in man—his fortune and his misfortune and the slow development of human nature under the most manifold conditions—one will overlook all this, I say in becoming convinced that man's attention and aim were always directed to virtues which he sometimes attained, but even where he failed, his aims were always of the loftiest kind.

The history lover is conscious of a genuine joy in these singularities of the human processes; just as he can rejoice in a flower without making a scientific effort to determine its botanical classification, or enjoy contemplating the habits and characteristics of an animal without thinking to what species, order, or genus it belongs.

The second requisite for the lover of history is open-mindedness, and an eye for broad general interests. If the lover of history, has observed the singularities to which I have just referred, there will arise in his consciousness an understanding of the process of human development in general.

Coming back now to the special history of medicine we shall say that whilst there are discordant and unhappy contrasts to be found they disappear in significance beneath the magnificent and glorious general purpose of medical science. From its very beginning to the present time it has been the aim to help and to heal, and even in the most despairing periods of medical history, this goal was never lost sight of. It almost seems prophetic that in the most discouraging times an original medical genius arose—a masterful being that lived his own life; that had the power to fulfill very specific and peculiar conditions, and who rarely failed to form a circle of effectiveness. It is to these great apostles, gifts of Heaven, some of them martyrs to their convictions, to whom our science returns ever and again for inspiration and encouragement. But nevertheless, outstanding peaks as they are in the range of high human endeavor, they are really an integral part of the historic process, a function of the vast mechanism of the general human life. That process which really cannot be designated by such a thought or a single word such as LIFE. Some of the leaders of the human race have had a conception of that process which far exceeds our comprehension; as physiologists, we can understand what is meant by the *functions* of life and the preceding chapters abundantly reveal such an understanding. But life to the great seers of our race was something that preceded the advent of the human race, and will continue after its extinction, a process in which the entire history of the human race is but a *function*—(Ps. XVII, verse XIV), "Men have portion in this life." Also (Sam'l. XXV, verse XXIX), "The soul of my Lord shall be bound in the bundle of life." And this is a conception that finds its verification in the study of history. Life is but a small phase of some unfathomable process beginning and ending in Infinity, and that Spirit, which appears in this world as the historic process, defies limitation of its nature by human conception. It fills all the boundaries of human existence by its own presence; nothing is accidental in it, its appearance has a foundation in everything.

Stunned at the vastness of the conception we are fortunate at the same time to gain an insight concerning the so-called Laws of History, namely, at the utter helplessness of the present race, to predetermine the value, conduct, and action of the future race by so-called laws; for it would be aiming at the fixation and predetermining of a process in which we are only a temporary part and largely a passive factor in that which produces the human race, and which will continue after it. But this does not mean that we should not continually strive to improve the lot and manner of existence of our race, particularly its health and physical strength, for in the physical health rests and is founded the perfection of the soul. *Mens sāna in corpore sano.*

So much is true in the History of Medicine. Men who have become prominent thinkers are all characterized by an innate ideal. Such an inborn ideal is present also in eminent human beings generally, but nowhere is it so evident and so lofty in its endeavors as in the revelations of the History of Medicine. In medicine there are two kinds of historic research. One is the investigation of single things which could be called the analytic method. Opposed to this is the other which could be designated at the synoptic, combining the philosophic methods. I have considered the analytic and synoptic methods in Chapters I and II and there elucidated that these seemingly diagonally opposed methods may in reality be only different aspects of one and the same process. Be that as it may, the two methods are helpful in bringing out different viewpoints, which later may be combined, if they are the results of the analytic method, or they may be analyzed and separated if they are the result of the synoptic method. The lover of history will find great enjoyment and instruction from familiarizing himself with these two apparently opposing methods and their results. They can be played one against the other, but also one coöperating with the other.

The next characteristic is that there is a largely successful endeavor to penetrate intellectually into that which is actual in history. The possession of an ideal and the wish to penetrate into the warp and woof of the texture of history is the inspiration of the general, as well as the medical, historian. One of the chief aims of this work will always be the perspective on the totality of human activities which is never to be lost sight of in the investigation of single events, details, and happenings. Historians of great renown have been guilty of causing confusion instead of producing clearness by laying too much emphasis on either one of the processes of historic investigation, namely, analysis or synthesis. Either they penetrate too far into detail or, overlooking detail work, they endeavor to cover the entire historic process by what may be called a blanket motive or force, such as Hegel does in identifying the historic process with his *Weltgeist* or "world spirit." He states that what we can discern in thesis, antithesis, synthesis, that all these processes and various phases of history are the "Weltgeist" coming to the consciousness of His spiritual freedom, and that this is particularly manifested in the moral and intellectual "freedom-consciousness" of the state. Here we have a great philosopher covering the entire world process as well as the historic process, with a transcendental conception. The "Weltgeist" is God. In the long and many centuries in which medical history was under control, and even carried on, by the priests and religious representatives of the race, this was a natural explanation of the causality of history, and it evokes a sense of surprise, but, at the same time, a sense of restful meditation, to observe that historical philosophers have not been able to advance any further than this conception in their explanations of historic causality.

It must be evident that having reached such a final and insurmountable conception of the cause of history, that the domain of historic investigation, which is in a ceaseless state of progress, cannot be molded according to laws of logical categories. From what has been said in the preceding chapters it is evident that the highest intellects, considered as historic phenomena, always manifest their own spiritual, moral, and intellectual characteristics. They are not conformable to any law or logical category, and, therefore, they reveal no unconditional necessity of their actions as dependent upon events that preceded them.

WHAT IS MEANT BY IMPROVEMENT OF THE RACE, AND HAS HUMANITY MADE ANY PROGRESS AS EVIDENCED BY HISTORY

When we contemplate this great problem from the medical standpoint, it appears natural to have faith in what we call medical progress, and thereby strengthen the belief that eventually the time will come when all diseases will be either entirely prevented or those that do develop will be met with their proper cure. With this prospect of the abolition of disease and suffering, human beings would no longer die from diseases but they would slowly come to an end like a worn-out machine whose parts refuse to work because they are worn down. Such a prospect is at once shown to be impossible by the fact that the causes of disease are changing and the human constitution upon which these causes act is changing also and, to make things worse, that we do not as yet know in what direction either of these factors is changing. Associating this fact with the well known modern conception that infectious bacteria do not always maintain their specificity, the problem becomes still more hopeless. (See Chapter II.) It appears that virulent types of bacteria may undergo mutations whereby they lose their infectious quality. On the other hand bacteria that have been known to be harmless have been observed to acquire an infectious quality. This may be the explanation both of our ignorance concerning the nature of the historic plagues as well as the character of the new diseases to which the human race may become subject in the future. It is therefore, impossible even in such a well organized science and art as medicine, to predict the entire abolition of the objects and purposes of its existence. Nevertheless, with the experience of the last hundred years, a decided progress and evidenced control of medical science over diseases may be confidently expected in the next centuries to come, provided the human race does not again relapse into its incorrigible folly of war-making.

There are three kinds of progress: Moral Progress, Intellectual Progress, and Economic Progress. The progress of medicine is inseparably bound with the sciences in general, likewise medical history is only a part of general history, and must rise and fall with the elementary forces that produce and influence general history. It cannot be said, however, that there has been that steady and sustained progress in general history which is evident and traceable in medical history. When the great historian, Leopold von Ranke, was giving a course of historic instruction on the History of the World to King Maximilian of Bavaria, an intensely interesting conversation which consisted of questions and answers between the master of history and the king took place. I shall reproduce part of this conversation in a socratic method.

The King: "You have spoken of moral progress: have you had in view also the internal psychic progress of the individual?"

Ranke: "No, Sir, only the progress of the human race as a totality; the individual, on the contrary, must elevate himself to an even higher moral level or plane."

The King: "But as humanity is made up of individuals, the question arises, if the individual elevates himself to a higher moral plane, must, not this progress also embrace the entire human race?"

Ranke: "The individual dies; he has only a finite existence, but the human race is infinite. I assume and grant a material progress because here one thing arises out of another thing, but it is different in moral affairs. I believe that in every generation the real moral greatness is equal to that of every other generation, and that there really is no higher potential in moral greatness: for example, we can not exceed the moral greatness of antiquity.

It even occurs in the intellectual world that intensive and extensive magnitude stand in inverse relation to each other: for example, compare our contemporaneous literature with that of classic antiquity."

The King: "Could we not assume, however, that Providence without restricting the free self-determination of the individual, has set up a definite goal for humanity as a whole, toward which the race is conducted, though without violence?"

Ranke: "That is a cosmopolitic hypothesis which cannot be demonstrated historically. Of course, we have the declaration of the Holy Scriptures that there will once be only one Shepherd and one Flock, but up to the present time this has not been demonstrated to be the dominant factor of world history. As a proof of this I refer to the history of Asia, which has experienced periods of the greatest grandeur and evolution only to drop back into barbarism. Asia has gone through several epochs of culture, but the oldest was the most flourishing; the second epoch and the third, in which the Greek and Roman element dominated, were not as elevated in culture as the first, and finally the third epoch, when the Mongolians conquered all of Asia, the culture of Asia was completely destroyed."

The King: "But isn't it true that at the present time a larger number of individuals have risen to a higher moral development than in former ages?"

Ranke: "That I concede partially, but I do not concede it as a historic principle, for history teaches that many people and nations are not at all capable of culture, and those that are capable of moral culture were far more moral in former epochs than they were in later ones. France in the middle of the seventeenth century was far more moral and cultured than it was toward the end of the eighteenth century. A greater expansion or breadth of moral ideas can be asserted but only in very limited circles. From the general humane standpoint it appears probable that the idea of humanity which is, however, only represented historically in the larger nations, should gradually embrace the total human race, and that would be the so-called 'inner moral progress.' History does not oppose such a view, but it does not prove it. We must guard ourselves against erecting such a view as a principle of history."

Ranke had very decided personal convictions about the essence of history. Concerning the question of ideas as factors in history, he did not believe that ideas had an independent life of their own, for this would make human beings nothing but mere shadows or living schemes that fill themselves with the ideas. As a matter of fact we know that this is the conception of Plato and that there is much in history and its evolution which confirms such a view.

All physicians incline to biologic views and, therefore, it is natural that they should incline to the view that there is an evolution of history. For the finer discernment of the term "evolution" I refer to the article on "Goethe," Chapter XXIX, and the one on "Darwin," Chapter XXX. To repeat, evolution means not that a new thing emanates out of a preexisting thing, but that the elements for the development of the new thing are contained in the preëxisting things. It requires two processes to bring about evolution, the growth and unfolding of the potential powers in the preëxisting things, and the external formative influences in the environment. In the processes of history we can sometimes discern the evolution of the events, and activities occurring in the human beings of one time out of radicles of activities and events of some preceding time. These are not repetitions (there is no repetition of history), but they are simply the further development of things that existed in potential form previously. That would be one factor in which evolution of history is comparable to evolution of biology.

The second factor which is the influence of environment is exceedingly difficult to trace, because the term "environment" at any historic period includes so much, is so complex, and has been so neglected in the historic records that have been left to us, that at the present time it is exceedingly difficult to ascertain what the natural, religious, social, political, climatic, and geographic environment was at the time of any given historic event.

Ranke identifies the ideas as historic factors with the prevailing tendencies of any given epoch or century, but these tendencies cannot be summated in one conception; they can only be described. He conceives the duty of a historiographer to be the analyst of the great tendencies of the centuries, the unfolding and disclosing the great historic activities of humanity which really constitute the complex of those tendencies. Humanity bears within itself an infinite manifoldness of developments, which appear one after the other in course of time, but the laws, if any, that underlie this progress are unknown to us because they are more mysterious and transcendental than our thinking powers permit us to comprehend. It is an error to assume that progress embraces all branches of human beings and of human ability, everywhere at the same time. Art flourished much more extensively in the 15th and 16th centuries than in the 17th and 18th centuries. I could almost assert this for the science of medicine, for in medicine we have embraced all of the three types of cultural progress which I have mentioned: intellectual progress, economic progress, and moral progress, for where the morals of a nation become decayed, there intellectual, as well as economic, progress is inevitably reduced and destroyed. As Horace puts it:

Doctrina sed vim promovet insitam,
Rectique cultus pectora roborant:
Utunque defecere mores,
Indecorant bene nata culpae,

meaning: "It is education that improves the powers implanted in us by nature, and it is good culture that strengthens the heart; whenever moral principles are wanting, vice degrades the fair endowments of nature."

The reason why moral progress has not kept pace with intellectual and economic progress is due to the short sightedness of the human race in not perceiving that morals are the very basis of a sane, enduring existence of any people. The race has laid great stress upon the provision of physical and physiological needs: land, clothing, and food. It has in the nations governed by white races provided abundantly for culture of the intellect and religious culture, but the pragmatic application of religion and intellectuality is far behind that of the various systems of economics as practically carried out at the present time. It is, therefore, an exaggerated valuation of that which is immediately useful and necessary to life which blinds the race to the indispensable safeguarding of morals. Provision for sustaining life and health of the body are natural, but this need not exclude the culture of morals or permit them to be neglected.

The great intellectual tendencies which rule humanity seem to undergo alternating variations. Some are drawn asunder and others attracted and closely ordered to each other. In every epoch there is one particular direction which predominates and forces the remaining tendencies to recede. Most all works on the history of the world describe the predominant religious tendencies of the 16th century which forced the literary tendencies to the background. In the 18th century Utilitarianism predominated and overwhelmed Art. Since 1914 there is evident in all civilized nations a tendency for the democratization of the sources of wealth and power, as well as of the institutions for the culture of the intellect. This is a very ancient conflict between masses and classes, which even affects medicine, for the practice of medicine is being extensively socialized. Whether under such a tendency the great master thinkers of the race can be produced in numbers equal to those in the centuries past is problematic. The history of the past is no guarantee for such a hope. Elisha Friedman in "America and the New Era," defines social education as progress at a minimum cost to society, and that it must be universal and available to all that are capable of assimilating it. He conceives that it aims to conserve the current order, namely: to compress into infancy, the period from birth to maturity, the experience of the human race and of the particular group into which the individual is born. Education enables the individual to overtake his social group before maturity, to make up his handicap, but he concedes that education must develop also the pioneer of society, the research mind that can apply existing knowledge to charting the unknown. Such a hypothesis overlooks the fact that intellectual growth is concomitant with physical and physiological growth. The body cannot be made to grow more rapidly by systematically stuffing it with an amount of food beyond the capacity of the digestive organs. If this be done, disease results; similarly, the mind cannot be rushed through the period from birth to maturity by any forced educational means. It would mean the replacement of natural selection by a form of rational selection. The organs of growth, both physical and physiological, are the result of uncountable years of evolution and natural selection. They will continue to develop by virtue of the same elementary forces which have guided them hitherto. Physical and intellectual growth are safer in their development if left to the inherent or autonomous growth than to the authority of others exclusively. Autonomy is the right of the individual with regard to species. Authority is the right of the species over the individual. Our tendency at the present time is to increase authority over the individual and even over his intellectual and educational training. History is in that light, a type of moral pedagogy, whose vitality appears to be in a perpetual conflict between the autonomy of conscience and social authority. Most all of the problems which the more highly developed nations of to-day have to face are born of this struggle. The tendency, so marked in the first twenty-five years of the 20th century, is affecting even the discipline of medical teachings, namely, the replacing of autonomy by authority and the general tendency of socialization and democratization of medicine.

The sacrifice of individual initiative and growth to society, the repressing of the culture of the great individuals for the purpose of maintaining the welfare of the great masses, may be compared to a mother who would devour her own children, and the individual who, by his intense egocentric exploits, would weaken the social bonds between himself and the mass is comparable to a heedless child who to warm himself, sets fire to the house of his fathers (Sabatier). The moral dignity, the cultural standard, and even the economic progress of the mass is justly measurable by what it does to produce the great personality from out of its members and it is a self-evident fact that the dignity and worth of the individual is measurable by what he does for the social body, but whilst the latter is perfectly evident in all truly great men, the opposite relationship, namely, what the mass does for the great individuals, is frequently not detectable.

FREEDOM OF THE WILL AND NECESSITY AS FACTORS IN HISTORIC

CAUSALITY: By far the greater majority of exact historiographers lay emphasis on the willingness to perform sacrifices, the altruism, the philanthropy, and the religious sense of the great men of our race. The shortest way to eliminate doubt concerning the judgment as to whether a man is great or not, would be to exclude every one who did not possess these moral characteristics. The reciprocal action and reaction between the propelling force of man to preserve himself in contending with adverse forces in his environment, and the impulses that lead him to yield to the environment, to subordinate himself to it, to adapt himself to it, constitute one of the most fundamental powers of all human life, and are at the same time one of the principal driving forces in history (See B. Kidd, "Social Evolution"). Harry Elmer Barnes and B. Kidd have represented this seeming antagonism as the chief impelling element of development, and deducted therefrom the wonderful significance of altruism, specially of organized altruism, the religions. Kidd defines religion as forms of creed that create a normal standardization for the great majority of cases that regulate the relation of the individual, where his personal interests seemingly stand in conflict with the interests of the total social organization.

It is self-evident that such a process is largely above human control; man imagines that he may control it and a partial control can at times be conceded in history, but a sustained control is not verified by the events of history. This bears then directly on the definition of "Freedom of the Will." Our most distinguished American psychologists agree in general that the so-called freedom of the will is, as a rule, evident only when the individual has to make a decision of choice in which his motives reveal themselves. Motives that spring from intellectual considerations eventuate in subordinate results, and are of comparatively lower significance. Such processes of motivation are classed among those that are called voluntary or decisions of choice. The so-called free decisions of choice are bound to deliberations of reason, and they can be spoken of as *free* only in so far as they can choose between mental contents that are given simultaneously. Mental contents that are not given simultaneously, but in succession may continue uninterruptedly, without any function of the will (Ach, *Handwörterbuch der Naturwissenschaften,* Band X, page 627). The thinkers among the medical profession are the very men to whom the profound problem of the reason and nature of history should appeal most strongly, and if they are interested in those departments of medicine that concern the human mind more particularly, they are not only interested in the subject, but cannot resist the altruistic impulse to communicate the best that there is known to them to fellowmen who have less opportunity and ability to meditate over these profound subjects.

It can be truly said that the meaning of history can be found in the striving toward a general higher ideal of human existence and augmented realization of that which we designate as humanity. The expansion and deepening of humanism, is the purport of the historic process according to Lotze and Herder.

To remember that man is the measure of all things, i. e., of his whole experience-world, and that if our standard of measurements prove false all our measurements are vitiated, to remember that man is the maker of the sciences which subserve his human purposes, to remember that an ultimate philosophy which analyzes man away is thereby merely exhibiting its failure to achieve humanity's purposes, is the real root of Humanism, whence all its auxiliary doctrines spring (F. S. Schiller).

That freedom of the will is restricted and encompassed by what K. Lamprecht calls the social, psychologic causality which checks the individual activities and choices is already indicated by the fact that some historians operate with a conception of *necessity* in history, for if something checks freedom of the will it can only be a form of necessity. It appears to me, personally, that these two conceptions, will-freedom and necessity, are two phases of the same process, comparable to what Plato conceives to be one process when he speaks of analysis and synthesis. This subject has already been spoken of in Chapters I and II and Chapter XXXI; we also have spoken of the factors of contrast and interest as constituting one of the subtle processes or motives for historic action. Thus what often confronts man as two diagonally opposed forces may be in reality the same impelling force acting in two different phases. Leonardo da Vinci is one of the earliest philosophers to have recognized this, so that all polemics and controversies giving preference to one or the other of these phases are virtually nothing but errors of conception. When Leonardo da Vinci exclaims "Oh, admirable necessity, etc.," he conceives it as the balance wheel and controller on the machinery of will-freedom, not only as a balance wheel but, under certain conditions, actually the impulse and releasing force for the exercise of will. In the historic sense freedom of the will manifests itself most often as a voluntary decision, immediately under the control of the individual, in other words as spontaneity; but when all the concomitant circumstances are taken into consideration the seeming spontaneity reveals itself as an inevitable resultant of forces and conditions to which the individual was actually impelled. So that the question may arise—is there a real spontaneity in human beings that manifests itself as a factor in history, and, if there is, how can it continue to exist in the great general historic process that is entirely independent of the will of the individual? Is man subject to an illusion and deception when he thinks there is such a thing as freedom of the will? This question had best be considered from the pragmatic viewpoint, for if we descend into the complex labyrinth of those great metaphysical questions concerning the reason and foundation of all life and the manifold systems of aspects concerning the Universe we will soon find ourselves in a tangle of paths that lead nowhere. Pragmatically, freedom of will and necessity really do actuate and impel human beings, sometimes singly, and sometimes in an alternating fashion, and at still other times it is impossible to discern which of these forces has the upper hand. It is comforting that some of the profoundest thinkers on history have, after consideration of all its possibilities, been driven to the idea of God, and the necessary natural laws are only the manner in which so-called inner spontaneous will impulses are realized in their response to this idea. Man, the human mind, has no mechanism for the comprehension of these vast problems and the realization of this is beneficial because it prevents the wasting of energy in the quest of a goal that is not attainable. There must always be a something which commands the highest and best that man is capable of for the very reason that this something is incomprehensible to man.

Human nature is such that the Divinity is stripped from everything with which it becomes too familiar. This is beautifully expressed in the Old Testament where Jahwe on the question of Moses who he is, answers "I am He that I am."

Concerning the Freedom of the Will it can be said that it is not possible to deny or to affirm it without specific qualifications. To deny it would be to undertake a negation that could not be supported either theoretically or practically. Haeckel attempted a denial of Freedom of the Will, because he believed that every so-called free action was brought about by a conception that preceded that action, and that these rooted in inherited or acquired mind characteristics. In other words, human character and human will is a gift of Fate that is forced upon every individual. The adherents of Monism expressed similar opinions. The great Belgian astronomer and statistical mathematician, Quetelet, and the philosophic speculations of the British historian Buckle, have established a precedent which many, who deny the freedom of the will, accept as their authorities. According to Quetelet in a large community so many marriages must be concluded, and a definite number of crimes will be committed, and a certain number of deaths by suicide will occur every year. Such astronomic, mathematic predictions of human events, if feasible, would be convenient to a great many shallow thinkers of the race, because it would satisfy them by the radicalism contained in these assertions. But human events do not show the regularity that is observed in the movements of the stars, and if there is any regularity at all it does not apply to all humanity. Those who deny the Freedom of the Will base their opinion on the view that every human action is a consequence of some previous conception, i. e., all actions are the result of motives. They conceive that the motive is the incentive, the stimulus to the will, which is thought to be in a resting condition. On penetrating investigation this relation reveals itself as exactly the opposite to that which has just been stated. It is not the motive which sets the will into motion, but rather it is the will which decides for one or another motive, because, in case there are several motives, the will can determine upon one single one as its choice. To this the opponents of Will Freedom have argued that although the will does not act according to a parallelogram of forces, it is always determined by the strongest motive. Here again, if we inquire what does it mean that one motive in comparison to another is the stronger, by what qualities is one motive stronger, and another weaker, we find that it is the "will" by its subjective judgment which decides the strength of the motives in any particular case. Goethe once said (in his revision of the work of Alexander v. Joch): "It seems to us that the real point of this controversy is always missed. It is not a question whether a being must act according to its nature; who would deny that. The real question should rather be put and decided something like the following: An active being is neither free nor under necessity when all actions which it undertakes work out its own individual pleasure and enjoyment. But it is not free: it is under compulsion when its actions aim to control the pleasure and enjoyment of another being. Because in that case there must be an impelling power in the being that acts. Freedom is a relative and sometimes even a negative conception; it really must be, for in the absence of any determination, and therefore without compulsion, nothing is thinkable and nothing is possible. Freedom expresses the absence of a certain determination and decision, and from which one? Is it from an essentially inner determination? Impossible. Accordingly it is absurd to use the word freedom when we speak of such determination and decision. It can only be used then when we are speaking of relations that are not essential, without which the being could continue to exist." From this argumentation we see that it is also absurd to speak of Necessity under conditions when the human being is active under his individual inherited qualities. Freedom and Necessity are only abstract conceptions, applicable only to actions in broad generality, not to concrete human nature, which is neither free nor under necessity. The connection between the two consists in the fact that decisions of choice or determinations, are the exponents of the free will and at the same time also deliberating reason. It is only by representing to the mind of an acting being the possibilities of different forms of activity, and the consequences that appertain to them that it becomes capable of drawing conclusions from past experiences and thinking of applying them to the future. Only in this way can a living creature give evidence of a free will. It is in the nature of living things that the opportunity for free choice becomes rarer with the simplicity of the creature. The more complex a living thing is, the more frequent will be the opportunity for the exercise of free will. One should really not ask if will is free or not, but, preferably, how can we harmonize our consciousness of free will with the theoretic postulate that everything that happens enventuates acording to "Unchangeable Eternal Laws." Now this inquiry is one of those dilemmas caused by a misinterpretation of the word "Law" (see Chapter III). The dilemma is not in the nature of the problem but in the misconstruction of a word, for Law in this sense does not mean an imperative commandment or order, but rather a description. It does not command what shall be done, but it merely describes what actually is done. We can thus conceive that all of our intellectual processes, conscious and unconscious, proceed according to natural law without expressing the slightest objection against the Freedom of the Will. For we do not say thereby that our will is forced under determinations which lie outside of it, but we merely say that when many and manifold operations of the will have been investigated we can find many signs and indications during the process of will-acts which we can observe and describe, and which agree among themselves. Any operations of the mind that determine its own manner of progress must be called free. We can only call such processes not free, or in other words, determined by necessity, that are regulated by causes that are outside of these processes. Now as our decisions and actions are decided by what we feel and think and know, and therefore by what is a part of our own being, we are actually in the possession of free will.

This does not mean that there are no factors which could not influence the will, but only that there is no influence of the free will which does not pass through the intellect of the one who is influenced. As it has been said before the will can be educated and this goes parallel with an education of the intellect. This observation is proof of the close connection between the will and intelligence. As far as the activities of human beings are concerned in the evolution of the historic process we can say that history, wherever accurate sources are obtainable, gives us an abundance of evidence of the operation of free will.

In writing history the problem of free will commands the attention of the historiographer almost at every step when we are dealing with great personalities. We see them not only following the first impulses of their free will, but, as in Pasteur's case for example, we observe them struggling with their first will-consciousness and determining upon a course that is opposed to their original inclinations.

THE REAL INCENTIVE TO TRUE HISTORIOGRAPHY: It would be an error to gain the impression from the first six chapters of this book that historiography takes it origin from any of the disciplines described therein. True historiography does not take its starting point from sources, records, documents, or any methods. The initiative to it is a purely intellectual process: namely, the intense interests in observed changes, and in the alteration of conditions, things, processes, actions, events, and conducts of human beings. The intellectual initiative leads to an effort to explain and resolve the observed changes in a manner free from contradiction. The next aim of this initiative is to bring these explanations, which unavoidably can be made only of small parts of these processes at one time, in connection and harmonious relation with reality and with our conscience. The methods are only a means to this end. All observed changes are perceived as such, because they do not occur regularly. Historic processes do not occur regularly. If some occurrences in the human environment and in the world of human experience transpired regularly they would not be appreciated as changes, and they would cease to be history because a regularly occurring thing can be predicted with exactness and that is not the character of historic occurrences. All men possess a stock of accumulated experiences, and the desire and effort to harmonize the environmental and social activities of men with this stock of experience is the main incentive to historiography.

The main content of history was defined by Jean Bodin (Johannes Bodinus) in his highly intellectual work *Methodus ad Facilem Historiarum Cognitionem* (Paris, 1566), as the activities of human beings as social entities, *actiones hominis in societate vitam agentis.* This is also very closely Bernheim's opinion. Verily, this should be incentive enough to stir minds that love culture to occupy themselves with the study of history, but the study of medical history which is so exceedingly important to preserve the cultural side is by no means honored with the brain work of the eminent thinkers of the profession. The best incentive of this kind of historical research cannot be given by methods alone. It must come from within the investigator himself, as Arago says, "The true historic investigator must admire at the right time and place"; to admire and marvel, to be astonished and marvel, causes the definition of correct questions, and questions that are correctly put, and inquiries that are honestly made, are always answered by history and science at the right time. The true historian is born; he cannot be made by any system of discipline. The chief object of studying the master minds of medicine was not, as might be thought, to aim at increasing their number by any scheme, method or plan, but rather to prevent their being crushed in their early years of life by false systems of education and total absence of psychologic viewpoints while searching for them when they are children. A master mind accepts instruction of a teacher, but as a rule he quickly exceeds it by growing beyond it; he does not follow any school, he creates a school. Great is the teacher that can discern the talent for master research and by calling attention to the defects in our systems of education, point out the closed portals which prevented science and research from finding the truth.

Let them come forth, the teachers, the prophetic teachers that can foretell the genius and master mind, masters that make masters of their pupils.

> Schön ist es überall, ein Stellvertreter sein,
> Zu gelten für die Welt, und nicht für sich allein.
>
> Die vielen gehn dahin, vom Drang des Tags getrieben,
> Und wo sie gingen, ist nicht ihre Spur geblieben,
>
> Stehn bleiben Wenige, das Zeugniss nachzutragen
> Vom Streben ihrer Zeit, wenn andre Zeiten tagen.
>
> Das sind die Geister auf der Menschheit höchsten Stufen,
> Bei deren Namen sind die Zeiten aufgerufen.
>
> Doch wie ein weit Gebirg am Horizonte sinkt,
> Und endlich sichtbar nur der höchste Gipfel blinkt,
>
> Die vielen Gipfel, die im Ferneduft verschwammen,
> Sind gleichsam unsichtbar im Einen nun zusammen;
>
> So von den Geistern auch wird Einem aufgetragen,
> Im Namen aller, die hinuntergehn, zu ragen;
>
> Und alles sammelt sich, was gross nur ist und schön,
> Um die am Horizont geblieb'nen Menschheitshöhn.
>
> (FRIEDRICH RÜCKERT)

> Glorious it is everywhere—to take another's place,
> To be worthy of the world, and represent the race;

Many pass away, driven by the heat of day,
And as they went, left not a trace to stay.

Few remain behind to carry testimony on,
Of the labor of their time, when newer epochs dawn.

These are the spirits on humanity's highest planes—
The eons are aroused at the mention of their names.

But as a distant range—on the horizon seeming,
And finally only highest peaks stand gleaming;

The many heights, diffuse in azure haze
Seem melted into one, to our marvelling gaze;

Of all great souls, 'tis destined that but one,
In the name of all, shall carry glory to the Sun;

And everything assembles, that is great, and good and light,
About the horizon of humanity's height.

(Translated by JOHN C. HEMMETER)

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